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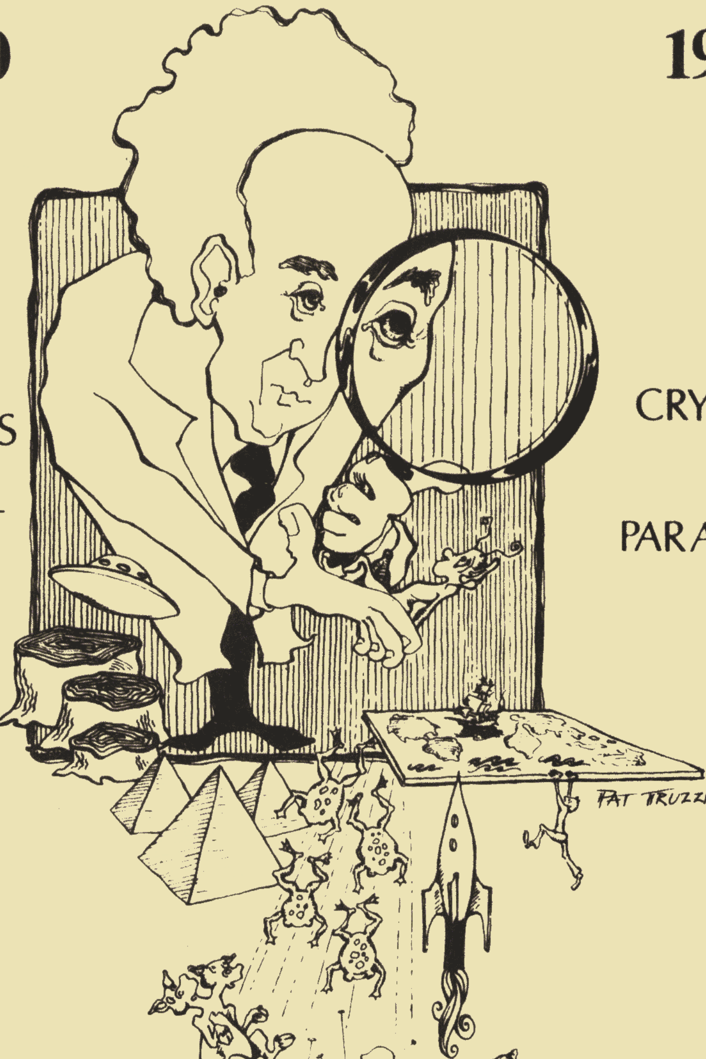

TRUE DISBELIEVERS AND THE MARS EFFECT CONTROVERSY

CRYPTO-SCIENCE

CHINESE PARAPSYCHOLOGY

UFOS

Zetetic Scholar

JOURNAL OF THE CENTER FOR SCIENTIFIC ANOMALIES RESEARCH (CSAR)

SCIENTIFIC REVIEW OF CLAIMS OF ANOMALIES AND THE PARANORMAL

ISSUE NUMBER 10

DECEMBER 1982

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ERRATA

In ZS#9, the remarks on Patrick Curry's article on the Mars Effect which were attributed to J. Dommanget should have more properly been attributed to the Belgian Committee Para for whom J. Dommanget was acting.

In ZS#9, on the table of contents page, the Random Bibliography was shown to be on page 68 when it should have read page 18.

In ZS#9, on p. 19, the Nye paper on "N Rays" is mistakenly shown as being in volume 1 rather than in volume 11.

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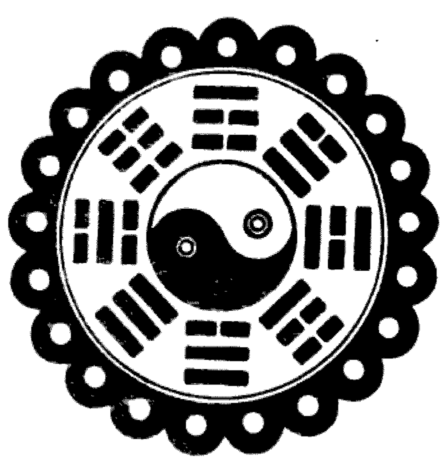

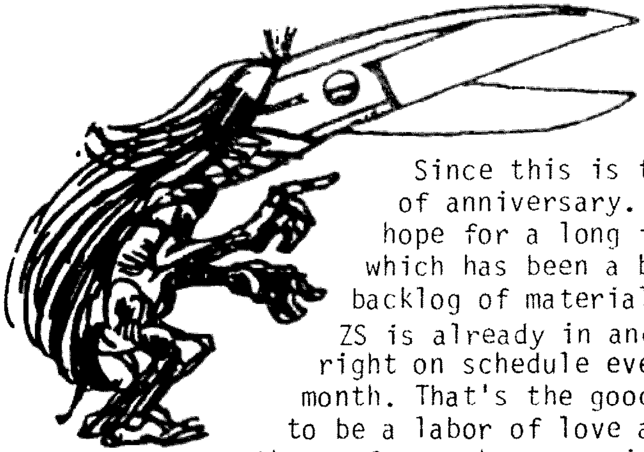

EDITORIAL

Since this is the tenth issue ZS, it constitutes a kind of anniversary. Readership has stabilized a bit, and I hope for a long future. Even the ZS publication schedule, which has been a bit erratic may soon improve since the backlog of materials is growing such that most of the next ZS is already in and nearly ready. So, ZS#11 should be out right on schedule eventhough this issue is delayed about a month. That's the good news. The bad news is that ZS continues to be a labor of love and struggles along financially. So, I hope those of you who appreciate ZS will (1) resubscribe, (2) seek to get us new subscribers with serious interest (ZS has never been intended for a general, popular audience), and (3) try to get your university or local libraries to subscribe. Since ZS is basically a one-man operation, what is needed is a quality readership that justifies the effort but which is quantitatively large enough (about 600) to make it financially secure.

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Two major areas of confusion persist which might be clarified here, at least as far as the policy of ZS is concerned. The first concerns the meaning of the term paranormal. The second concerns the character of our focus on anomalies.

The terms supernatural and paranormal are constantly confused, particularly by critics of the paranormal. Many of us are critical of any sort of supernaturalism but remain comfortable with claims of the paranormal. The notion of the supernatural concerns the idea that there are things outside of the natural world, the empirical world of science. The central idea of the supernatural is of a world of forces and/or entities beyond the natural order. The supernatural is a major part of most theologies and usually concerns a transcendent order (heaven, angels, etc.) that does not obey natural laws. When the supernatural intrudes into the natural world, it is usually in the form of a miracle. A miracle is a suspension of the natural order of things; it is an exception to what normally happens in nature. For religions, miracles take place through divine interventions. The point is: the supernatural is not law-like. It is not part of nature whose laws scientists seek to formulate. On the other hand, the paranormal is part of the natural order which is not yet understood. Scientists who study the paranormal begin with the assumption that the esoteric phenomena they seek to study is lawful. Thus, the parapsychologist, for example, does not think psi is a miracle, an exception to the laws of the natural world. He thinks psi is something that exists in the natural world and follows some sort of law-like pattern yet to be discovered. (There are a few parapsychologists who believe that psi may be "random" and without pattern knowable to man, but this makes psi preternatural --beyond everything-- no longer paranormal since it is no longer amenable to scientific generalization.) The paranormal is not "miraculous" and it ceases to be paranormal as soon as scientific theory advances to the point that it can become part of "normal" science. Many, if not most, claims of things paranormal are probably errors. But there will always be new anomalous facts which are now denied but which will eventually become accepted, often via reconceptualizations quite unlike those of their current proponents.

The important thing in this distinction is to realize that claims of the supernatural are ascientific or even antiscientific. They claim things outside of science. But claimants for paranormal phenomena are trying to extend science in normal methodological fashion to examine possible new phenomena and perhaps incorporate them into new scientific theories. The paranormal claim may oppose current scientific majority opinion as to the adequacy of our existing scientific theories, but that opposition can be on purely scientific grounds and should not be confused with antiscience. And arguments against "miracles" and other supernaturalisms should not be invoked to dismiss paranormal claims. One can be opposed to metaphysics and mysticism while still open to evidence and arguments for claims of the paranormal.

This distinction is an important one for ZS in that supernatural claims are simply outside the scope of our dialogues. For example, a paranormal explanation of a supernatural claim (e.g., a psychokinetic explanation for the Shroud of Turin) would be unsatisfactory for that claim's proponents. Ironically, while critics of the paranormal confuse it with the supernatural, supernaturalists (e.g., the Catholic church) have long recognized that a paranormal explanation eliminates a miracle.

A complementary confusion exists about the proper focus on anomalies. We know that anomalies do exist. We know that many claims of anomalies are in error but that some will probably emerge validated. Science is always incomplete and fallible. Ideally, it should be a self-correcting system that will slowly and conservatively accept and integrate new anomalies into our scientific view of the world. Anomalies represent a crisis for existing theories, but they also represent opportunities for new, advanced theories. But we should not confuse our constructive and open attitude towards anomalies with mystery mongering. Many Forteans, for example, seem to enjoy anomalies for the discomfort they cause scientists. An anomaly is important to science only in so far as it can lead us to better theory and incorporation of the anomaly so that it is no longer anomalous. The goal is to produce better and more complete science; it is not to embarrass science. Obviously, some scientists are unduly dogmatic,and we may enjoy seeing them have to eventually admit an anomaly they denied (forcing them "to eat white crows"). But if anomalistics is to be a scientific orientation, its goal must be to explain anomalies not gloat over their being unexplained. I think we must admit that many anomaly-seekers want things "unexplained" and enjoy anomalies because they are puzzles. They want puzzles, not solutions to puzzles. Such puzzle seekers may play a valuable role in alerting scientists to new puzzles. But puzzles for the sake of puzzles is not basically a scientific attitude. In fact, it may actually become an antiscientific attitude if carried to extremes. Anomalies are a means to an end: improved science. They should not be an end in themselves. They may be useful in reminding us of the limitations of current scientific theory; they become abused if they are centrally used to attack rather than extend science. When an anomaly is accepted and incorporated into normal science, we should be pleased; but I think we need to face the fact that many proponents of the anomalous would be terribly disappointed rather than pleased if tomorrow their pet anomaly (whether UFOs, psi, or whatever) was accepted and neatly explained. Examining the "unexplained" can be fun, but insistence that it remain "unexplained" even after it is reasonably examined and either explained or dismissed, may distort healthy anomalistics into dysfunctional mystery mongering.

THE MYSTERY MEN FROM HOLLAND, III: THE MAN WHOSE PASSPORT SAYS CLAIRVOYANT

PIET HEIN HOEBENS

"He has solved some extremely complex crimes, has located graves that have been 'lost' since 1917, foretold a great many events that defied probability, and once tracked a thief in a distant country by telephone. His fame is solidly established in his native Holland and in a number of European countries. He has actually been licenced by the Dutch government authorities as a 'practitioner of the psychic arts.'"

Thus, in his 1974 Crime and the Occult, Paul Tabori summarizes the extraordinary career of Marinus Bernardus Dykshoorn, the man whose passport bears the entry: Occupation: Clairvoyant.

Unlike his famous countryman Peter Hurkos, who received his clairvoyant abilities as a result of an accident, Marinus Dykshoorn was "born psychic." This happened in 1920 in the little town of Gravenzande near The Hague. Young Marinus was troubled by his unusual gift, the nature of which was a mystery to him and to his community. The word "surprise" had no meaning for him. He would know beforehand what his parents would buy him for Christmas. He would often be punished for "eavesdropping" because he knew things he was not supposed to know. He occasionally caused great embarrassment to his parents by revealing intimate information about visitors to the Dykshoorn house. He was a living lie-detector. "...I could not understand why anyone would say something that was patently not true. Surely everyone else realized there was not truth in what was being said?" he later recalled in his autobiography. The turning point in his life came in 1938 when a German scientist diagnosed his deviation as ESP. Soon after, Dykshoorn decided to become a professional clairvoyant. He practiced in his native Holland until 1960, when he moved to Australia. The Australian episode was a frustrating one. The local police were strongly prejudiced against psychics and refused to even listen when Dykshoorn wanted to tell them where they could find the body of their Prime Minister who in 1967 had disappeared while swimming in the sea. "Mr. Holt's body, of course, was never found," the sensitive regretfully records in his memoirs. In 1970 Dykshoorn moved to the far more hospitable shores of the United States where he has become something of a celebrity.

In this article, I will restrict myself to Marinus Dykshoorn's pre-1960 exploits. As in the earlier articles on Peter Hurkos and Gerard Croiset, I will critically examine a number of prize cases as they have been published in English. "For the rest, the reader will have to believe that a few represent the many," to borrow Mr. Dykshoorn's own words.

The Sources

The principal source on Dykshoorn is the autobiography My Passport Says Clairvoyant ("As Told To Russell H. Felton").[1] There are sections on the psychic in Tabori's Crime and the Occult[2] and in Laile E.

Bartlett's recent Psi Trek.[3] Of considerable interest is an extensive feature article by Dan Greenburg in the February 1976 issue of Playboy, entitled "I Don't Make Hocus-Pocus," based on a lengthy interview with the sensitive.[4] After reading ("in a single sitting") My Passport Says Clairvoyant, Dr. Gertrude Schmeidler wrote: "It is a fascinating account of almost unbelievable successes in tracking criminals, finding buried treasure, and similar clairvoyant or even precognitive feats."[5] "Almost unbelievable" claims demand almost unbelievably strong evidence. In the light of this criterion, how does the case for Marinus B. Dykshoorn stand?

The blurb of the Dutch version of My Passport[6] guarantees that "all claims in this fascinating book can be checked." Unfortunately, when I actually tried to check some of the claims with Mr. Dykshoorn himself, the latter declined to cooperate. No reply was received to a letter sent to him on May 6, 1982. On July 14, I phoned the psychic at his New York office. He flatly refused to give me any of the information requested. He suspected that I wanted to filch from him material he was going to use in a second book. I pointed out to him that I merely wished to check some claims made in the first book, but to no avail. In spite of his claimed proficiency at "Long Distance ESP" (see section below), Mr. Dykshcorn repeatedly asked about my personal background.

Local Game

According to Tabori, Dykshoorn's "fame is solidly established in his native Holland." The clairvoyant himself conveys the same impression when, on p. 16 of his autobiography, he states that his work was "public knowledge" and that he was "accepted by the press, the police, the public, and finally the government..." There are no objective and unanimously agreed-upon criteria for celebrity, so I could not possibly disprove such claims. However, while Peter Hurkos and Gerard Croiset are household words in the Netherlands, questions about Marinus B. Dykshoorn are liable to be greeted with the counter-query "Marinus Who?" Having spent months in attempting to track the psychic's record in this country, I know what I am talking about. Dykshoorn claims that most of his work for the Dutch authorities was done in strictest confidence. If so, the Dutch authorities must be commended for knowing how to keep a secret. Neither the files of De Telegraaf nor the invaluable private archives of the late Mr. Ph. B. Ottervanger in Bussum contained more than a handful of clippings relating to the man whose passport says clairvoyant. Little of this press material can be said to be very favourable to Dykshoorn. Consider the following story, taken from the weekly Privé of May 6, 1978. Young Truus van der Voort from Voorburg disappeared on June 28, 1975. About one year later her parents consulted Dykshoorn, who was then visiting his native country. The psychic took a pendulum, watched its movements, and cheerfully announced that the girl was alive and would be heard from "in three months time." Privé quotes the parents as remarking bitterly that, as late as 1978, no trace of their daughter had been found. (The body of Truus van der Voort was discovered in 1981 in a plane wreck in the Swiss Alps.)

Marinus Dykshoorn Tested by European Scientists?

"In the Netherlands and in Belgium I was tested many times at universities - among them the universities of Amsterdam, Delft and Utrecht - and was found to be a bona fide, or genuine, psychic. The researchers concluded that, although my abilities could not be explained, they could be seen to work, and I was allowed to practice as a professional clairvoyant," Dykshoorn states on p. 16 of his autobiography.

As Mr. Dykshoorn refuses to disclose the names of the European scientists who are supposed to have tested his abilities, it is impossible to verify this claim. The parapsychological literature is curiously silent about these experiments.

The only European researcher of the Paranormal named in My Passport is "Professor Greven, a professor of psychology and parapsychology from the University of Cologne" whom Dykshoorn met "one evening in early 1938." Professor Greven, described as a totally blind septuagenarian, immediately recognized the Dutchman's extraordinary gift. "He told me that I might be able to perform very valuable work, for my friends about whom I was constantly worried, and for the community. He told me that I was lucky to have been born in the Netherlands, where the attitude toward ESP was considerably more enlightened than in most countries" (p. 28). Professor Greven boldly predicted that the young sensitive would "meet skepticism and hostility."

In order to find out more about this remarkable scientist, I consulted several reference books and made inquiries with the parapsychology institute in Freiburg i. Breisgau. Strangely, no trace of a "Professor Greven, professor of psychology and parapsychology from the University of Cologne" could be found. The name is not listed in the index of Handbook of Parapsychology or any comparable work. Dipl. psych. Eberhard Bauer, an authority on the history of psychical research in Germany, had never heard of such a person. He kindly offered to contact Cologne University. At his request,Frau Lichtenfeld, Dekanatssekretarin of the Philosophy Department (of which the Cologne Psychology Institute forms part), consulted the complete Index of Lectures for the years 1937-1940. "Professor Greven" remained as elusive as ever. The exhaustive Kurschners Deutscher Gelehrter-Kalender only mentions a Dr. Theol. Joseph Greven who in 1929 was appointed Professor Extraordinary at Bonn University. This Professor Greven was a theologian not a (para) psychologist. Moreover, he was not older than 56 in 1938.

Official Recognition?

According to Dykshoorn, the investigations by (anonymous) University researchers resulted in his being "allowed to practice as a professional clairvoyant." Tabori and Greenburg claim that the psychic was "licenced" or "endorsed" by the Dutch government and had to undergo a most difficult examination before being granted this distinction. The theme of "official recognition" recurs throughout the Dykshoorn literature.

Mr. Dykshoorn has ignored my request to be shown an official document supporting the official endorsement claim. It is, to put it mildly, unlikely that such a document exists. Contrary to what seems to be widely believed abroad, the Dutch authorities have never licenced anybody as "a practitioner of the psychic arts." The "psychic arts" are free in Holland, and there is no legal ground for "endorsing" (or, for that matter, for refusing to "endorse") anyone who claims to be a practitioner. The official examinations mentioned by Tabori and Greenburg (and at least strongly hinted at by the psychic himself) must be the products of somebody's fertile imagination.

Then, of course, there is the matter of the passport entry, referred to in the title of the autobiography. This is what Dykshoorn has to say about it (p. 70): "In any event, I believe that my work in this area (psychic detection) led to my claim being endorsed by the Dutch government when I was issued a passport listing my occupation as helderziende - "clairvoyant." As far as I know, I am the only psychic ever to have been so honored."

In July 1982, I made inquiries with the Ministry of Foreign Affairs in The Hague, which is competent in all matters concerning passports. The official spokesman, Mr. Schutter, bluntly told me that the claim was "apekool" - rubbish. Professions are no longer listed in Dutch passports, but before the new regulations went into effect Dutch citizens were entirely free to state any profession they happened to fancy. The entry in the passport does not imply any sort of "recognition" on the part of the government. Thus, if I had ever wanted my passport to say Clairvoyant, all I would have had to do would have been to convey this wish to the passport office clerk.

Occult Historian

Dykshoorn's first prize case involved royalty. According to Bartlett, "He reconstructed the assasination of Willem the Silent (first Prince of Orange,and founder of the Royal Dutch dynasty, no less), a murder that had taken place in 1584, over three and a half centuries before. The Director of the Prinsenhof Museum in Delft wanted to know whether Dykshoorn could fill in any of the details of the assasination, or of the particular people involved." On pp. 43-45 of his autobiography, the psychic vividly recalls the scene: "We went into the chamber where the killing was known to have taken place, and I concentrated on the action. Immediately, I knew what had happened. Willem had been shot once in the throat, and another shot had missed. Both bullets had lodged in the stone wall. On the wall of the chamber was a small glass-fronted case protecting two neat holes from the potentially damaging fingers of sightseers. 'These holes were originally much lower, down,' I said. The director smiled. 'If you are a trickster,' he said, 'you have certainly done your homework. How else has the room changed?' 'The floor was much lower,' I said. 'This is not the original floor. The level we are on would have been at about chest height in those days.' 'Excellent!' he beamed. 'You're absolutely right!'

"But I was much more interested in testing my gift than impressing the director. I set out to reconstruct everything that had happened on that dark day for Holland in 1584. 'Gérard really did do it,' I said. 'Philip had promised him instant elevation to the Spanish nobility if he succeeded...'" (Dykshoorn is referring to the assasin Balthasar Gérard and to Philip II, King of Spain, against whose tyranny the Dutch revolt had been directed - PHH) "'Gérard gained an appointment with Willem,' I said, 'to request permission to leave the country for Spain. Without such a permit he could not have escaped to collect his reward, so he waited until Willem had signed before firing the shots.' I walked over to the wall. 'There was a doorway here, lower down. Gérard escaped through it and hid under a dung heap outside. When the guards found him, they brought him back inside and walled him up in another chamber. They hoped to perserve him from the mob, at least until he could be tried and made to confess to having acted for Philip. But some of the crowd noticed the new brickwork. They tore down the wall and took him. " So the hope of preserving the assassin from the mob had been in vain. On p. 43 Dykshoorn writes: "They dragged him into the open square and roped each of his limbs to a different horse. Gerard was torn apart." The director of the Prinsenhof Museum paid Dykshoorn "on the spot" the sum of one hundred guilders for "clairvoyant services rendered."

There is something fishy about this story. The reward was paid "on the spot" so the director could not possibly have had the opportunity to check any of the supposedly fresh information given to him by Dykshoorn. One hundred guilders was a considerable sum at that time (Dykshoorn mentions that it occurred in 1948). Is it conceivable that anybody - let alone a director of an important historical museum - would be so reckless as to pay a small fortune for an unverified psychic statement?

What Dykshoorn reports having said about the assasination would have been known to any Dutch schoolboy, except for two details: 1) that Willem of Orange had been shot in the throat and 2) that Gérard had been lynched by the mob before he could be brought to justice. On both points, Dykshoorn was dead wrong. Willem the Silent was not hit in the throat but in the chest. The autopsy report has been preserved and can be seen in the State Archives in The Hague. Balthasar Gérard was not lynched by the mob but arrested, tried by a special Commission consisting of members of the High Court, the Court of Holland and the City Court of Delft and sentenced to death on July 14, 1584. The execution took place the same day. The horrible sentence has been preserved and can be seen at the Algemeen Rijksarchief in The Hague (3d Dept., Archieves of the States of Holland After 1572, brown cabinet nr. 44.) The way Dykshoorn describes Gérard's fate vaguely suggests that he may have confused Willem's assasin with the De Witt brothers, prominent Dutch politicians who were lynched by a mob in The Hague in 1672. A Dutch schoolboy who would have made a similar mistake would have been punished by the history teacher instead of receiving a hundred guilder reward.

If Marinus Dykshoorn ever attempted to give a psychic demonstration at the Prinsenhof Museum the event does not seem to have made a lasting impression. Neither the present director, Drs. R.A. Leeuw, nor his predecessor, Drs. D.H.G. Bolten, could recall ever having heard the story. At Drs. Leeuw's suggestion, I contacted the art historian Dr. Anne Berendsen who had been the custodian of the Prinsenhof since 1949. In her reply she wrote that she had never heard of Dykshoorn's alleged feat. "It is unlikely that such a visit would never have been discussed afterwards," she added. According to Dr. Berendsen, the published account is "worthless."

Long Distance Clairvoyance

Writers on Dykshoorn seem to agree that the Duisburg Long Distance affair must be regarded as the psychic's chef d' oeuvre. According to Tabori, it was "Dykshoorn's most spectacular case." According to Bartlett, "solving a robbery case in Germany by telephone from Holland established Dykshoorn's international reputation." The case is the subject of a special chapter in the autobiography.

A summary of the claim: On March 25, 1958, Dykshoorn, in Breda, received a phone call from Franz-Joseph Becker, the captain of a Rhine barge. Becker was calling from Ruhrort near Duisburg in West-Germany. He reported the theft of his launch. It had been missing for two days, and the Duisburg river police had been unable to find a trace of it.

Dykshoorn replied that the boat had not been stolen, but had been set adrift by mischievous teenagers. It would be found "about two-and-a-half kilometers downstream" (says the autobiography) or "six miles upstream" (says Tabori). Becker reported Dykshoorn's statement to the police who found the launch where Dykshoorn had said it was - wherever that was. On March 28 (says the autobiography) or "a few hours later" (says Tabori), Herr Becker called again. This time, a considerable amount of money had been stolen from the barge's cabin. The police had been notified. In fact, they were listening on another line. Dykshoorn reported a vision of a 17-year old member of the crew of a fifteen-tonner moored near Becker's barge. The lad had stolen the money and put it in his travel bag. He planned to leave for vacation the next morning. The captain and the police immediately set out for the ship indicated by the psychic. Sure enough, they found the 17 year old scoundrel and the travel bag full of money. After the police had confirmed to newspaper reporters that "Yes, Dykshoorn solved the case by telephone - long distance!" the story was picked up by the press and radio. Dykshoorn suddenly became a celebrity in Germany.

Critical evaluation of the claim: It is true that the affair received a certain amount of publicity at the time. I have a copy of an article that appeared in the Frankfurter Abendpost on May 13, 1958. This (popular) paper basically confirms Dykshoorn's and Tabori's accounts, except that it ignores the missing launch. From the newspaper article it would appear that Becker first contacted the Dutch psychic after the money had been stolen. The Abendpost has the police "smiling" at Becker's request for permission to call Dykshoorn. The police would have had little reason for being ironical if, two days (or a few hours) previously they had been witnesses to a remarkable instance of psychic detection. However that may be, Abendpost has the Duisburg river police confirming the claim -- which would seem the most confidence-inspiring feature of the case.

At the time, the Abendpost article caught the attention of the then active Deutsche Gesellschaft Schutz vor Aberglauben ("German Society for Protection against Superstition"), a group of (mostly) scientists strongly opposed to any sort of "occult" belief. The Society made inquiries with the Wasserschutzpolizeidirektor von Nordrhein-Westfalen, whose reply, dated June 19, 1958, is quoted in the Society's Mitteilungenblatt (No. 10, August 1958, pp. 12-13). The chief of the water police wrote: "Our inquiries have revealed that the story in the newspaper sent to us does not conform to the actual facts. In the relevant instance, the evidence against the offender was produced by normal police methods."

From the brief note in Mitteilungenblatt, it is not entirely clear whether the letter from the police chief has been reproduced in its entirety. Especially as German proponents of parascience have frequently complained about what they perceived as a penchant for quoting-out-of-context on the part of the Society, an attempt at double-checking was made for the purpose of the present article. The files of the now defunct Society could not be located. In the Spring of 1982 Herr Gerd H. Hövelmann of Marburg a.d. Lahn at my request contacted the Wasserschutzpolizei, the Public Prosecutor in Duisburg and the municipal police of Duisburg-Ruhrort. His letters contained an accurate outline of the claims in Dykshoorn's and Tabori's books.

Both the city police and the Public Prosecutor denied having any information on the matter. However, in May a most helpful reply was received from Herr Kriminaloberrat Kitschenberg of the Wasserschutz-polizei. Herr Kitschenberg unequivocally denied that in 1958 his department had cooperated with any clairvoyant. After having read Herr Hövelmann's letter, he had spoken to the officer who had handled the Becker case. He had been assured that the case had been solved by normal means. This was confirmed by the documents in the police archives. Herr Kitschenberg further wrote that the claim that the Wasserschutzpolizei had "admitted" the psychic's success is untrue. He reminded us that, at the appropriate time, a state law forbidding the police to employ clairvoyants was still in force. In a second letter, dated June 3, the Herr Kriminaloberrat added that the police files do not contain any indication that, in the Ruhrort affair, a psychic was consulted by a private party. He repeated that the extant documents show that the case was solved without paranormal assistance.

What happened in 1958? Is the claim a complete invention? Or is the police covering-up the fact, that, a quarter century ago, they dis-obeyed a state law by cooperating with a clairvoyant? A reconstruction of the actual events seems impossible at this time. My private guess is that a victim of a theft privately consulted Dyshoorn and was treated to the customary diffuse and ambiguous "psychic statements." After the police had solved the case, the captain selectively remembered and sub-jectively validated Dykshoorn's utterances, convinced himself that the clairvoyant had scored a few remarkable hits and informed the press accordingly. In the journalistc process the account underwent further embellishments. All this is conjecture. The repeated statements from the Wasserschutzpolizei - cast in the role of chief witness in both Dykshoorn's and Tabori's reports - are clear and unambiguous. The principal claimant has refused to provide me with solid documentary evidence to the contrary.

Two Further Claims

One of the most intriguing episodes in Dykshoorn's career is de-scribed on pp 73-77 of My Passport Says Clairvoyant. On Tuesday, February 12, 1952, the psychic underwent tests "at a Dutch provincial university that shall remain nameless here." The anonymous researchers required him to state whether smears of blook on glass slides came from a man or a woman. The psychic set to work, until he was given a sample of blood "from which I received a very strange psychic impression." At first, he was surprised, then enraged. He stood up and told the scientists that he did not like being trapped. "You are playing games with a very serious subject, and I deeply resent the implication that I am merely a fraud who has never been exposed. My abilities and the way I use them are public knowledge, and until you can disprove my abilities, please do not degrade them. This blood sample has been taken from a female. A pregnant female. A pregnant female dog - or maybe a fox; I don't know. Now if you will excuse me..." In an angry mood, the orator went home. As he entered his Breda apartment, the telephone rang. The caller's daughter and the daughter of a neighbour had disappeared in nearby Tilburg. Half an hour later, the man came to collect the clairvoyant. They set out for Tilburg, went to the police station, and then to the banks of the Wilhelmina Canal. "My gift led me to the exact spot from which the children had fallen into the water and then, immediately, to the body of the first child. There had been no foul play...a few minutes later the police recovered the second tiny body." The next morning, one of the university parapsychologists phoned to say that he "had read in the newspaper about my help in finding and recovering the children's bodies..." and to apologize for what had happened at the laboratory. The researchers had not been aware that not all the blood samples had been human. A naughty laboratory assistant had taken blood from a pregnant fox and slipped it in among the human samples.

It is of course vaguely suspicious that Dykshoorn does not mention the name of the university where the remarkable experiment is supposed to have been conducted. He refers to "a Dutch provincial university," but such institutions did not exist in 1952. It is curious that the amazing demonstration of ESP never seems to have been reported in the parapsychological journals.

The blood sample test also features in Tabori's book - but in a completely different context. According to Tabori, the experiment was part of the examination Dykshoorn had had to take in order to get his government licence. "'A dog,' he said. He was wrong - it was a fox. But that did not prevent him from getting his licence." To complicate matters even further, the Dutch version of My Passport Says Clairvoyant has it that Dykshoorn correctly guessed that the blood sample came from a pregnant fox terrier - a dog owned by the parapsychologist who called to apologize on February 13. Regarding the case of the missing girls, I wrote to the Tilburg Police Superintendent on May 2, 1982. I further contacted the Tilburg municipal archives, where a complete collection of the local newspapers is kept.

On June 18, Superintendent T.P. de Vries replied. He confirmed that on February 12 two children had drowned in the Wilhelmina Canal: a boy and a girl, both three years old. Contrary to what is suggested in the Dykshoorn autobiography, the cause of the disappearance had been clear from the start: the boy's sister had witnessed the tragic accident. A quote from the original police report: "About 17.00 hours Mrs. van Z. (mother of one of the victims) told me (mother of the second victim) that her daughter J. had come home reporting that her little son A. and my little daughter P. had fallen into the water near the boat-house." So the location of the accident was known exactly, which throws a dubious light on Dykshoorn's claim that "my gift led me to the exact spot from which the children had fallen into the water."

The police report in no way mentions assistance from Dykshoorn or from any other psychic. It will be recalled that the enigmatic parapsychologist who apologized to Dykshoorn on the 13th had "read in the newspaper about my help in finding and recovering the children's bodies." However, the personel of the municipal archives have ascertained that none of the local papers contained any mention of Dykshoorn's role. It is unlikely that the parapsychologist could have read in any paper about the recovery of the two bodies on February 13. Both the police report and the newspaper accounts reveal that, while the girl was found late in the evening of the 12th, the body of the other victim was only recovered four days later. Mr. Dykshoorn's memory must have played a nasty trick on him - and on his readers.

Conclusion

Marinus B. Dykshoorn is the third of the famous Dutch "paragnosts" whose alleged feats are critically examined in this series. As with Peter Hurkos and Gerard Croiset, the successes in psychic detection ascribed to this sensitive do not bear skeptical scrutiny. As far as the claims discussed in this article are concerned, the facts flatly refuse to corroborate what Mr. Dykshoorn's passport says.

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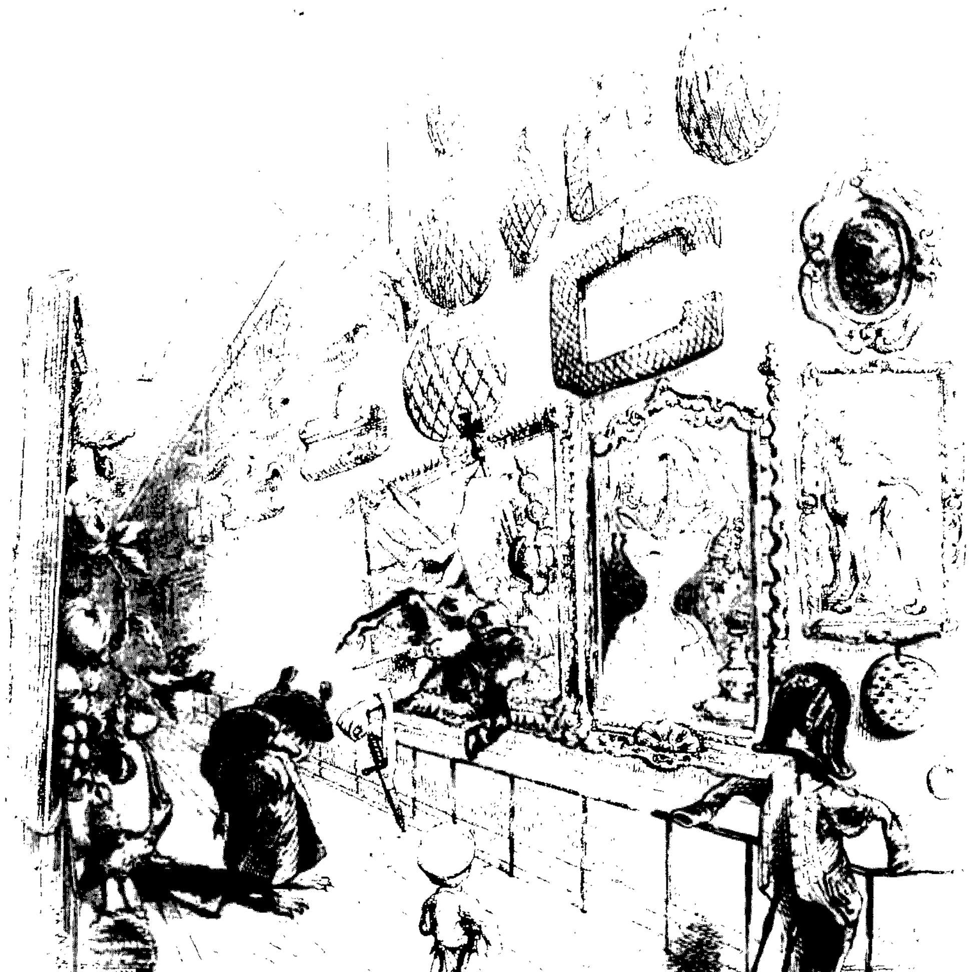
Acknowledgment: I am indebted to Mr. Gerd H. Hövelmann for his help in checking the Duisburg case.

NOTES:

1. Dykshoorn, M.B. (as told to Felton, R.H.): My Passport Says Clairvoyant, N.Y. Hawthorn, 1974.
2. Tabori, P.: Crime and the Occult, N.Y. Taplinger 1974 (pp. 143-145).
3. Bartlett, L.E.: Psi Trek, N.Y. McGraw-Hill (pp. 76-81)
4. Greenburg, D.: "I Don't Make Hocus-Pocus - an eerie visit to a man whose passport is stamped "clairvoyant"" in: Playboy February 1976. Marinus Dykshoorn is best known for his supposed proficiency in psychic detection and private psychic counselling. His autobiograghy is almost exclusively concerned with successes in these fields. I first learned from the Playboy interview that Mr. Dykshoorn also claims to be a Dutch Uri Geller. He reportedly told the interviewer that he had psychokinetically stopped clocks "hundreds of times in laboratories," that computers get upset "when I get very cranky" and that "everybody around me gets sick, really" when he is in a bad mood. Apparently, one is well advised never to pick a quarrel with Mr. Dykshoorn. A big Australian fellow who threathened to give the psychic "one good lick" next moment found himself lying on the ground, paralyzed. "Did you touch him at all?," Mr. Greenburg asked. "No," said the clairvoyant. Unfortunately, the "hundreds of times" Mr. Dykshoorn worked PK miracles under laboratory conditions do not seem to have resulted in a commensurate number of scientific reports.
5. Quoted on cover of Dykshoorn (1974)
6. "Translated and adapted" by Louis Rebcke, entitled Mijn Beroep is Helderziende ("My Profession is Clairvoyant"), Haarlem, Gottmer, 1976. The "adaptation" has resulted in a few minor but curious discrepancies with the original English version.

POSTSCRIPT

After the manuscript for the above article had been type-set, I accidentally discovered what must be the solution to the "Professor Greven" mystery. In the thirties there existed in the Hague an obscure "Society for Philosophy and Parapsychology" led by a Dr. E. Greven. This Dr. Greven was a Dutchman with strong Germanophile leanings. The Hague daily newspaper Het Vaderland on March 6, 1942, reported that Greven had been appointed Professor of Parapsychological Philosophy at Leiden University. Presumably, Professor Greven never actually lectured there, as the previous year Leiden University had been closed down by the Nazi invaders after the outcry of professors and students against the dismissal of the Jewish personnel. According to Mr. George Zorab (who in the European Journal of Parapsychology, Vol. 1, Nr. 3, November 1976, erroneously states that Greven was appointed already in 1940), the appointment had been a personal favor from the Nazi Governor, the notorious Dr. Seyss-Inquart. Greven acquired the status of "Professor" only as a result of the unusual political circumstances of the time. After the war (if he survived at all), he immediately relapsed into obscurity. It is quite possible that Mr. Dykshoorn some time during the thirties met this gentleman.



THE INVENTION OF WITCHCRAFT:

USES OF DOCUMENTARY AND ORAL-HISTORICAL SOURCES IN RECONSTRUCTING THE HISTORY OF THE GARDNERIAN MOVEMENT

AIDAN A. KELLY

In a paper titled "Observations on Systemic Methodological Problems with Theories of Ancient Female Monotheism," which I delivered to the Women and Religion section of the AAR Western Region meeting at Fuller Theological Seminary, Pasadena, in March of this year [1981], I made three points that I would like to summarize, in reverse order, as an introduction to the concerns of this paper.

1. Robert Graves, among others, proposed without proof more than 30 years ago that a type of female monotheism had existed throughout neolithic Europe [1]. This proposal has been repeated quite frequently in recent years [2]. However, the serious research of the last 30 years has shown that the entire Aegean area from 2500 to 1500 B.C. (this being the only area at issue for which we have significant data) seems to have been economically and culturally the westernmost outpost of the civilization common to the Eastern Mediterranean lands, a civilization whose religious beliefs now seem fairly clear [3]. That is, for any time and place for which we have data, the data leave no room for a female monotheism.

2. It has all too often been proposed that the veneration of the Blessed Virgin in Christianity began simply as an imitation of the cult of the Goddess who was the supreme deity of that supposed ancient female monotheism [4]. There is, in fact, no significant pre-Christian evidence for the existence of any such exalted concept of a Goddess. The writings most often cited as supposedly preserving evidence of such a pre-Christian concept all date from the second century [5]. Plutarch's and Lucius' descriptions of Isis could be based on the Gospel according to Luke. The reverse relationship is not possible.

3. It is currently being claimed that a form of that ancient female monotheism survived into this century, and is still being practiced under its traditional name of "witchcraft" [6]. In fact, all such claims are based on the claims to historicity for the magical religion invented in the 1940s and 1950s by Gerald Gardner and various of his colleagues.

In March I offered this third point as a conclusion, without proof, and promised to provide that proof in the future. I am now beginning to redeem that promise. The full proof, based on analysis of relevant documents, is contained in a manuscript currently titled "The Invention of Witchcraft" that will, if all goes well, finally be published next year [7]. I am here going to summarize its contents and report on an oral-historical verification of its conclusions.

Adequate synchronic descriptions of the Gardnerian movement have been published and need not be summarized here [8]. The movement may have up to 100,000 serious adherents worldwide, and, taken by itself, the claim by members of this movement to have some historical tie to the witchcraft of a century or more ago might seem curious but quite unimportant. However, the Gardnerians do not exist in isolation, and their claims to historicity have been made the basis for claims that are beginning to have serious effects on religious scholarship and on various political movements. These claims therefore cannot be allowed to pass unchallenged.

As it happens, many radical feminists who are active in the current Goddess movement never mention Gerald Gardner in their writings [9]. Perhaps their position (which they do not explain) is that, if one form of their religion was passed down through Gardner, another form could have been passed down independently of him. Also, many current attempts to reconstruct the supposed ancient Goddess religion are based, in part, on an intuitive argument, of the form "Since I know from my own experience how this religion feels, I also know what bits and pieces of ancient culture might have been part of it, and what could not possibly have been part of it." This is a perfectly sound argument in itself. It is used by adherents of all religions to some extent in unraveling the history of their faith, and, kept within the bounds of common sense, it can provide rough guidelines for historical research. But its cogency here depends on the assumption that there is an unbroken historical continuity, however slender, between the current movement and the ancient religion. I contend that there is no such continuity, because there is no historical basis whatsoever for the Gardnerian claims to historicity; so the claims for the existence and survival of an ancient female monotheism are a charming fantasy, but lack any basis in serious scholarship.

Let me emphasize here that I am not arguing from silence. I am NOT saying "There is no evidence to support such claims." I am saying that there is now a great deal of evidence, all of which adds up to a virtual impossibility that such claims could be true.

THE HISTORICAL PROBLEM

Gerald Gardner, a retired British civil servant, claimed that he had been initiated in 1939 into a coven in the New Forest area that dated back at least to Elizabethan times. He also claimed to have renovated and augmented the fragmentary traditions of that coven into a viable system, which he then used to found new covens during the 1950s. His position was that, despite his eclecticism, he was preserving the essential concepts and practices of an ancient religion; and this has ever since remained the position of almost all members of the movement he founded [10].

I became curious more than ten years ago about Gardner's claims. Specifically, I wondered what had constituted the traditions of that older coven, and what Gardner's contributions had been. Of course, no reliable conclusions about such a question could be drawn from published versions of the "Book of Shadows" (henceforth, BoS), the Gardnerian liturgical manual. Instead I would need access to original documents, and since the Gardnerian BoS is kept secret, and only exists as a family of manuscript traditions, I thought that the question would have to remain unanswered [11]. To my surprise, all the necessary documents became available to me within a few months in 1974. Since then, other bits and pieces of published evidence, and oral-historical sources, have served to refine and confirm my basic conclusions -- which let me now proceed toward.

THE DOCUMENTARY EVIDENCE

The documentary evidence here consists of the following.

(1) Gardner's three published books: HIGH MAGIC'S AID (HMA), 1947; WITCHCRAFT TODAY (WT), 1954; and THE MEANING OF WITCHCRAFT (MW), 1958 [12].

(2) A set of eighteen typewritten documents owned by Carl L. Weschcke, President of Llewellyn Publications, in St. Paul, along with various handwritten and typewritten letters from Gardner that allow the documents to be indisputably identified as being from Gardner's hand. These documents, sent to Weschcke by a former member of one of Gardner's covens, turn out to be versions (typed between about 1958 and 1960) of documents written between 1953 and 1960.

(3) The documents and letters bought by Ripley's International, Ltd., Toronto, as part of Gardner's witchcraft museum. Of this, the single most important item of all is a manuscript book titled "Ye Bok of ye Art Magical" (BAM), which turns out to have been written between about 1944 and 1953.

The analysis of these documents occupies several hundred pages of my, God willing, soon-to-be-published manuscript. Here I can only summarize my methodology and my conclusions.

The Gardnerian claim to historicity amounts to a claim that some aspect or level or bits or pieces of the rituals and other documents that make up the BoS are traditional, that is, are derived from the beliefs and practices of the pre-1939 coven. The Weschcke documents in St. Paul, and the BAM MS. and several other items in Gardner's notebooks in Toronto, are working drafts of items included in the (now published) versions of the BoS [13]. I sorted out Gardner's "additions" by the obvious, though tedious, method of identifying the literary sources of the bits and pieces that make up the rituals in the BoS [14], and setting such identified passages aside. I had intended to ask whether what then remained made sense as the sort of religious tradition that Gardner claimed. In fact, after these literary additions have been set aside, what is left could easily have been constructed from such obvious sources as Murray, Leland, and the great Cambridge classicists -- but that is not at all what the Gardnerians claim.

Actually, since the preceding remarks apply only to the rituals, this conclusion is all but trivial, for two reasons.

First, it is obvious to anyone familiar with western magical traditions that the Gardnerian rituals derive from the magical system of the Hermetic Order of the Golden Dawn, and not from any native British religious traditions.

Second, the Gardnerians have never claimed these rituals to be traditional; they admit the rituals are exactly where Gardner did the most reconstruction work. Rather, they claim the pre-1939 stratum is one of concept, customs, traditions, laws, and, in brief, the sorts of things incorporated in the BoS document called the "Craft Laws" or the "Old Laws" [15], which is the only piece of writing in the BoS that claims to be (and, prima facie, could be) historical. I had felt that the Gardnerian claim to historicity must stand or fall on this document ever since I had seen a complete, published version of it in 1971 [16]. Internally it purports to have been written in the mid-18th century, and to include materials dating back to the 16th century. The oldest complete version of it I have found was included among the Weschcke documents, in two separate parts typed about 1959 or 1960.

I went to Toronto hoping to find even older versions of these "Craft Laws" -- and so I did, though not in the form I was expecting. In a file of letters I found a document titled "Proposed Rules for the Craft"; it could be dated to June 1957 because of a reference to it in a letter from Jack Bracelin, Gardner's supposed biographer [17]. These rules, written by someone named Ned, greatly overlapped the concerns of the "Craft Laws" document. In fact, I finally realized, these rules were the first draft of the "Craft Laws." Given that hypothesis, I was able to identify an earlier draft of every passage in the "Craft Laws" somewhere else in Gardner's published or unpublished writings. Where later and earlier versions of the same passages have been published, we can see that the later passages have been archaicized by incorporation of obscure words gleaned from the Oxford English Dictionary [18].

In sum, there is nothing in these "Craft Laws," or in the entire BoS, that could possibly be part of a tradition received from a pre-1939 coven. In nuclear physics, even if a particle has no charge and almost no mass, and is therefore very difficult to detect, its existence must have some detectable effects [19], and if those effects cannot be detected after statistically sufficient efforts to detect them, we can be sure the particle does not exist. Similarly, the existence of a pre-1939 coven would have left detectable traces somewhere in the Gardnerian BoS. Since no effects at all can be found, we can be virtually certain that such a coven did not exist, and that there was no sort of Stone Age religion surviving in England in 1939. Hence the intuitive argument about the nature of an ancient female monotheism also collapses.

However, the Gardnerian movement does exist, and is a viable new religious movement. What is its actual history? One clue here is the fact, not yet mentioned, that Gardner was dyslexic: his letters and journals show that he could neither spell nor punctuate, and his grasp of grammar was rather shaky. Hence, despite his intelligence, which I do not discount, his disability would not have allowed him to write the books published under his name. The literary help did not come from his publishers: the carbon copy of the unpublished MS. of WT in Toronto is in excellent shape [20]. Gardner must have had a co-author. When Doreen Valiente finally admitted in her most recent book [21] that she had been initiated by Gardner in 1953, the pieces fell into place.

AN ORAL-HISTORICAL SOURCE: FLOYD

During this past year, I had the pleasure of a visit from a man who was able and willing to answer many questions I had not thought I would ever get answered. He is now still a member of Gardner's original coven, into which he was initiated in January 1957 by Gardner and by Doreen Valiente, who was the High Priestess of that coven [22]. The following month, the coven split in half, with Doreen and Ned, as her High Priest, taking four or five others with them to form a new coven, and Gardner soon wandered off to begin initiating new members also. This was the situation when the "Craft Laws" were created in 1957; Floyd thought they were the work of Doreen's coven, since they had never been part of the original coven's tradition.

Floyd assured me that all the Gardnerians of his time knew Doreen Valiente had written virtually everything in the BoS, and that the master copy of that book was still in her possession [23]. Floyd believes there was a pre-1939 coven, and said he knew several people who had met "Dafo," who had also been a member of it. However, he admitted, she had never been forthcoming with any data about that coven.

Given this and other information Floyd shared, I reconstruct the history of the Gardnerian movement as follows.

Gardner met "Dafo" during World War II in some sort of magical group, and they decided to found a religion of "witchcraft" that they would claim dated from the Middle Ages. They collaborated on the writing of HMA and on creating the BAM MS., which is clearly intended for liturgical use in rituals. Apparently they met with very little success until they initiated Doreen Valiente in 1953. She promptly put her very real talents as a writer to work for the new religion, rewriting the existing rituals and creating new ones, and doing probably the majority of the writing on WT. The useful material from the BAM MS. was copied into her new master copy of the BoS about 1953, and BAM was retired to the back of a cabinet [24]. Apparently Doreen very early in this process took over Dafo's position as High Priestess of the coven.

The working drafts of the rituals in BAM, in the Weschcke documents, and so on, can all be dated to within a year or so by comparison with the versions of the rituals in the published books. Some specific discoveries that arise from such comparison are the following.

(1) Many things claimed to exist in WT did not exist at that time, and were only in the planning stages. For example, none of the circle rituals in use in 1954 were based on a "pagan" theology; instead, they were all adapted from the Kabalistic system of the Greater Key of Solomon. In fairness, I should emphasize here that the work which Valiente and Gardner carried out in creating their "pagan" rituals during the 1950s represents a major advance in magical technology, since it adapts the often cumbersome procedures of the HOGD system to the needs of small groups.

(2) WT incorporates the concept of "traditional laws" that govern the "Craft," but, as I said, the writing of the "Craft Laws" document did not begin until 1957, when, for the first time, there were two covens, and therefore a need for a common set of ground rules for all members of the religion.

(3) There was no emphasis on the Goddess as the major deity, and on the High Priestess as the central authority in the coven, until after 1957, when Doreen Valiente became the first such "Gardnerian" High Priestess, and began to adopt the "White Goddess" myth invented by Robert Graves as the official theology of her coven. One can see the beginnings of this late stage in MW, which Doreen also seems to have helped Gardner write; the "hiving off" of her new coven represented growth more than a schism.

CONCLUSIONS

I think the women's rights movement has always been much too important to be based on a bogus historical claim. I think the need in our society to explore and experience the femininity and motherliness of God, both theologically and socially, is far more important for our future well-being than any merely political issue could be. The Gardnerian movement, which is a viable, growing, and very interesting religious movement in its own right, exists because its emphasis on the concept of the Goddess is meeting a need in our society that is not being met by the "mainstream" churches, which had therefore better look to their laurels.

Although I am denying that there was an ancient female monotheism, I am offering a positive result in exchange: to wit, that the "Gardnerian" movement, although organized by Gerald Gardner, was essentially the creation of two women who have not before been given (or claimed) adequate credit for their creative achievements. Doreen Valiente and the woman called Dafo were not preserving or reviving something ancient; rather, they created a new religion which is, for better or worse, carving out a place for itself in the modern world. Insofar as its growth depends on its spurious claim to ancient authority, it may yet suffer some setbacks among persons whose interest in it depends on that claim; but as a uniquely Western form of spiritual discipline, it merits the attention of serious scholars, and its adherents merit being dealt with as adults, not as children who must be fed on milk.

NOTES

1. Robert Graves, THE WHITE GODDESS (London: Faber & Faber, 1948; 3d, rev. ed., 1961). This proposal of female monotheism is a variant of the classic theory of "matriarchy" proposed by Bachofen, Lewis Henry Morgan, Briffault, etc. Also, neither the obvious general importance of goddesses at all times and places in the ancient world, nor henotheistic worship offered to a particular goddess, is at issue here.

2. E.g., in such popular works as Starhawk's THE SPIRAL DANCE: A REBIRTH OF THE ANCIENT RELIGION OF THE GREAT GODDESS (Harper & Row, 1979), Merlin Stone's WHEN GOD WAS A WOMAN (Dial, 1976), Naomi Goldenberg's CHANGING OF THE GODS (Beacon, 1979), or Charlene Spretnak's LOST GODDESSES OF EARLY GREECE (Moon Books, 1978). All these works simply assume the truth of Graves' hypothesis, and offer no analysis, proof, or defense of it. I find it even more disturbing that essentially the same unexamined assumption underlies works intended to be scholarly, such as Christine Downing's THE GODDESS (Crossroad, 1981), and many articles and contributions to anthologies (see further in note 9).

3. A sober history of this period arises out of such works as Joseph Fontenrose's PYTHON (University of California Press, 1959), Michael Astour's HELLENOSEMITICA (Brill, 1967), Gordon's COMMON BACKGROUND OF GREEK AND HEBREW CIVILIZATION (Norton, 2d ed., 1965), and, in general, the articles in Voume I, Part II, and Volume II, Part I, of the Third Edition of the CAMBRIDGE ANCIENT HISTORY (in contrast to those in the second edition, which still left the question open).

4. This also was one of Graves' contentions, adopted uncritically by many current feminists. Geoffrey Ashe's THE VIRGIN (Routledge and Kegan

Paul, 1976) is most useful for its elegant scriptural defense of the doctrine of the perpetual virginity of Mary and for showing that the hypothesis that there was a pre-Christian Great Goddess religion of any sort must lead inexorably to historical conclusions that contradict most of the major premises of Christianity.

5. E.g., Lucius Apuleius' METAMORPHOSIS dates from ca. 155, in the midst of the Gnostic effloresence. Plutarch's essay on Isis and Osiris probably dates from 110+/-10, and so could be contemporary with Luke; however, J. G. Griffiths, PLUTARCH, DE ISIDE ET OSIRIDE (University of Wales Press, 1970), points out (citing the work of Torhoudt, 1942) similarities between De Is. 19 and Hippol. Refut. 6.30.1 (on p. 354), and between De Is. 54 and Hippol. Refut. 6.30.6-31.6 (on pp. 49, 504). These indicate that Plutarch's concept of Isis is already very close to the Gnostic Sophia speculations of, e.g., the Valentinian Gospel of Truth 9.14-17. Given the overall thrust of the massive scholarship of Charles Talbert and of Raymond Brown, one must currently assume that any influence by Gnosticism creates a presumption of dependence on Christianity.

6. Continuity with an ancient female monotheism has been claimed in virtually every popular book advocating modern witchcraft since the 1950s. The claims by radical feminists only began in the mid-1970s, when the women's rights movement and the Gardnerian movement began to interpenetrate, with the resulting invention of "Dianic" (women-only) witchcraft. P. E. I. Bonewits, REAL MAGIC (Creative Arts, 2d ed., 1979) gives a witty account of how this happened on p. 110.

7. This MS., originally written in 1976 for Llewellyn Publications, was accepted in partial satisfaction of my comprehensive examinations in theology at the Graduate Theological Union, Berkeley, in 1977. It is essentially the same MS. referred to by Adler, Russell, and Bonewits (see notes 6 and 8) under the title "The Rebirth of Witchcraft." This change of title does reflect a change in my interpretation of the data. The MS. has been accepted for publication by the Institute for the Study of American Religion, Evanston, Ill.

8. The major study is Margot Adler, DRAWING DOWN THE MOON (Viking, 1979). J. B. Russell, A HISTORY OF WITCHCRAFT: SORCERERS, HERETICS, AND PAGANS (Thames and Hudson, 1980) is important as the first serious study of witchcraft as a whole that deals with the Gardnerian movement sympathetically but accurately. See also the articles by Marcello Truzzi in Edward A. Tiryakian, ed., ON THE MARGIN OF THE VISIBLE: SOCIOLOGY, THE ESOTERIC, AND THE OCCULT (Wiley, 1974), and in I. J. Zaretsky and M. P. Leone, eds., RELIGIOUS MOVEMENTS IN CONTEMPORARY AMERICA (Princeton University Press, 1974); the latter gives an excellent survey of the literature to that date. See also Susan Roberts, WITCHES USA (Phoenix House, 2d ed., 1974) for a well-written popular survey.

9. E.g., none of the books listed in note 2 discuss Gardner. Two of his books appear in Starhawk's bibliography, but she never mentions him in her text. Even more disturbing is Emily Culpepper's "The Spiritual Movement of Radical Feminist Consciousness," in J. Needleman and G. Baker, eds., UNDERSTANDING THE NEW RELIGIONS (Seabury, 1978), pp. 220-234. On pp. 222-224, she discusses Feminist Witchcraft specifically, quoting with approval a statement that its tradition goes back to the "middle ages and . . . ancient religions and rituals focused on the

worship of a Goddess who was a Divine Mother." She never mentions Gardner, and recommends Z. Budapest's FEMINIST BOOK OF LIGHTS AND SHADOWS (Luna Books, 1976) without mentioning the fact (and it is no secret) that this is merely a rewriting of Gardner's BoS to eliminate all mentions of males both mortal and immortal. I do not object to advocacy as such; I object when advocacy is passed off as scholarship.

10. Gardner's mentions of this story are fragmentary and scattered, and the description in GERALD GARDNER: WITCH (Octagon House, 1960), published under the name of Jack Bracelin (see note 17), is brief and vague. The most detailed version is given by Doreen Valiente, in the entry for "Gerald Gardner" in her ABC OF WITCHCRAFT (St. Martin's, 1973), but, as we will see, she is anything but an independent witness. The only scholar before now who has considered the Gardnerian claim to historicity is Elliott Rose, A RAZOR FOR A GOAT (University of Toronto Press, 1962). Although Rose supposes the Gardnerian claim to be inherently fraudulent, he makes the strategic error of believing Gardner's claim that there had been an earlier coven, and so becomes lost in speculations about what sort of person might have perpetrated such a fraud around the turn of the century, a point on which there can, of course, be no evidence.

11. I did write an essay applying some basic techniques of New Testament textual criticism to the two versions of the "Craft Laws" that appeared in June Johns' KING OF THE WITCHES (Coward McCann, 1969) and in THE GRIMOIRE OF LADY SHEBA (Llewellyn, 1972). Isaac Bonewits, just before he left for his year as Editor of Llewellyn's GNOSTICA, had asked me to submit this essay, which was subsequently published as "Textual Criticism and the Craft Laws" (GNOSTICA, July 1974). The arrival of this essay in St. Paul reminded Carl Weschcke of the Gardnerian documents he had filed away, and Isaac, recognizing that the documents were probably very important, sent me copies of them.

12. Gardner's books, which have gone through many reprintings, were originally published as follows: HIGH MAGIC'S AID (London, Michael Houghton, 1947); WITCHCRAFT TODAY (London: Rider, 1954); THE MEANING OF WITCHCRAFT (London: Aquarian Press, 1959). The pamphlet WITCH, by "Rex Nemorensis" (Charles Cardell), published privately in 1964, and in which the pot calls the kettle black, is also useful for dating some of the BoS documents.

13. Stewart Farrar's WHAT WITCHES DO (Coward, McCann, 1971) contains the full text (EXCEPT for the "Craft Laws") of the copy of the Gardnerian BoS that Alex Sanders obtained when he was initiated by Patricia Crowther in 1963. THE GRIMOIRE OF LADY SHEBA (Llewellyn, 1972) is slightly less complete, since she apparently never received the Third-Degree initiation, but she makes up for that by a burst of uninspired inventiveness.

14. By "literary" I mean only "existing in a published book." The major sources used include: Aleister Crowley's MAGICK IN THEORY AND PRACTICE (Castle, 1934); S. L. MacGregor Mathers' edition of THE GREATER KEY OF SOLOMON (apparently in a British version that differed in some details from the DeLaurence edition of 1914); Charles Godfrey Leland's ARADIA, THE GOSPEL OF THE WITCHES (Scribner's, 1897); and apparently some Masonic and/or Golden Dawn rituals (aside from those transmitted in Crowley's writings).

15. For texts of the "Craft Laws" see the works in note 11.

16. In Johns (see note 11).

17. "Floyd," my British visitor, told me the book had actually been written by Idries Shah, who then decided he would prefer not to have his name on it, and asked Bracelin to take responsibility. Since Bracelin's spelling, punctuation, etc., were as poor as Gardner's, this story makes at least prima facie sense.

18. E.g., compare the passage that is supposed to go on the first page of a "witch book" in its first appearance in WT (about p. 53, in most editions), where it begins "Keep this book in your own hand of write," with the archaicized versions of it in Johns and Sheba. Since this passage appears on the first page of BAM, we must conclude that Gardner considered BAM to be the earliest version of the BoS.

19. SOME effects must be detectable, since it is not parsimonious to hypothesize the existence of a particle that cannot affect the rest of the universe in any way.

20. Hence Francis King's assertion in RITUAL MAGIC IN ENGLAND (London: Spearman, 1970), p. 180, that "the reader responsible for its acceptance, himself an occult scholar of distinction, managed to blue-pencil most of the more rubbishy passages" seems baseless. I might also comment that I have managed to date every document in the BoS, and thus discovered that the assertion by Crowley's friends (reported by King, loc. cit.) that Gardner commissioned Crowley to write the BoS is utterly groundless, since ONLY the rituals published in HMA existed while Crowley was still alive.

21. Doreen Valiente, WITCHCRAFT FOR TOMORROW (St. Martin's Press, 1978), p. 14. On p. 21 she says that she coauthored "Darksome night and shining moon" and other materials used in the Gardnerian BoS in 1954-1955.

22. One reason I knew Floyd's statements were accurate was that he knew the names (which I knew from reading the letters in Toronto) of the totally obscure persons who were members of the coven in 1957. It was a great pleasure to ask, "Who was Ned?" and be told, "Oh, he was someone in the City," which, of course, is British for "rich and well-connected."

23. On p. 21 of her 1978 book, Valiente says that she owns "Gardner's original Book of Shadows, which he gave to me." I doubt that. Since she wrote it, she simply took it with her when the original coven divided in half in 1957.

24. Derek Copperthwaite, then Vice President for Research and Development of Ripley's International, told me that he had personally discovered the BAM MS. in the back of a cabinet while supervising the packing up of the museum holdings for transport to the New World. There were no working copies of the BoS in the collection when it was bought, but the Wilsons (Gardner's heirs) apparently had not known of the existence of this MS. book, since it had been retired before their arrival at the Witches' Mill in Castletown. Mr. Copperthwaite was the first person to recognize that BAM must be important for reconstructing the history of the Gardnerian movement.

THE LOCH NESS MONSTER: A GUIDE TO THE LITERATURE

Supplement 1

HENRY H. BAUER

Since publication of the annotated bibliography in 1980 (Z.S. no. 7), the following books have been drawn to my attention. They are numbered so as to fit into the sequence used earlier.

6A. David C. Cooke and Yvonne Cooke, The Great Monster Hunt, W. W Norton, 1969.
Photos III, VIII, IX, XI. Story of the Cooke's visit to Loch Ness, meeting a number of the monster-hunters and eyewitnesses; gives a good feel for the place and the mystery. No references or citations, but the accuracy is commendable (I noted only two errors: that the Mountain expedition was in 1935 (actually, 1934); that the 1954 sonar showed a 50-foot long object (no estimate of size was possible)). For children (grades 4 to 8).

15A. Warren Smith, Strange Secrets of the Loch Ness Monster, Zebra Books, Kensington Publ. Corp., 1976.
The cover of this paperback promises sensationalism and unreliability: "Now that sonar photographs have proven that the monster does indeed exist....Is there a connection... with Hollow Earth Theory....Bermuda Triangle....UFOs..." The contents are considerably better, and quite sober; but there are many errors (mostly on minor matters), numerous misspellings, no bibliography, very few references. Photo III on the cover. Another potboiler.

19A. Gerald S. Snyder, Is There a Loch Ness Monster?, Julian Messner, 1977.
For older children. A laudable attempt to present pros and cons rather than one side only of the matter; but the presentation is badly organized, and quite a number of inaccuracies have crept in despite the author's obviously wide knowledge of the literature: for instance, on p. 83 there is photo XI, correctly labeled as taken in 1960, with no indication that some doubts have been expressed; on p. 161 is the claim (from Burton) that O'Connor photographed a stick and inflated bag -- but we are not told that it is the photo on p. 83 that is under discussion, in fact we are shielded from such realization by the wrong date of 1959 used on p. 161! Photos III, V, XV as well as XI.

19B. Ian Thorne, The Loch Ness Monster, Crestwood House, 1978.
For children (grades 4-5). Some silly errors -- that Dinsdale spent several months on his first expedition in 1960, for example. Photos III (labeled 1961 instead of 1934!!), VIII, XV, XVI (in two versions, as though different shots!), XVII. Why is it apparently so difficult to keep the simple facts straight??

UFOS, FIREBALLS AND METEORITES

HARVEY H. NININGER

PREFACE

One of the great figures of American meteoritics, Harvey H. Nininger has published works spanning six decades. Among his many publications are his textbook on meteorites, Out of the Sky (1952) and his autobiography Find a Falling Star (1972). Since Dr. Nininger's wide-ranging experiences in the field investigation of meteorite falls is outstanding, ZS is honored that he has allowed us to print the following essay on his observations of human perceptual errors.

-- Ronald Westrum
Associate Editor, ZS

Mister X said "I was standing right here. It was 5 o'clock in the morning. You see, I haul trash for people and I get out early, and that big ball of fire went right over that telephone pole - not ten feet above it. Some people are saying it was a meteor or something. I know better. I seen it with my own eyes; it was within 100 yards of me. I was so scared I couldn't talk. I tried to call my wife. She was right inside that kitchen door (about 30 ft. from where we were standing), but I couldn't say a word. I was shakin' like a leaf. That was one of them spudniks and I knew that the guy in it could just evaporate me. No, they can't tell me it was a meteor. I seen it so plain it scared the wits out of me." We were in north Denver, and he was pointing in a westerly direction.

At the same moment that Mr. X was undergoing this awful fright, a driver 200 miles west of him stopped his truck to watch a sight the like of which he had never before seen; when we interviewed him, he pointed to a spot about a half mile east of him where he thought the fireball struck. Bear in mind, these men were 200 miles apart - the man in Denver looked west-northwest and the truck driver in Utah looked east. And, at this same moment, the workmen at an oilfield encampment in Wyoming 200 miles north-northwest stood breathlessly watching to see if that ball of fire was going to blast their nearby oil tanks. They didn't have to hold their breath long, for in a few seconds it was ending in a swarm of sparks some 200 miles south of their camp; but they thought it hit only a short distance beyond their tanks.

I was busy with my meteorites here in Sedona when the telephone rang, and I heard the City Editor's voice coming from the Denver Post asking for help. He said they had been receiving a host of reports concerning sightings on that day which had them puzzled. They would like to know if what people were reporting was a meteorite, a flying saucer, a sputnik - (this was only a short time after the first Russian Sputnik was put in orbit) - or some kind of space craft that they knew nothing about. They said reports had been coming in from many locations in several states and that one had come from a Lt. Commander in the Air Force which made it seem pretty serious.

I asked him to read what teletype they had on it, and he read for about 20 minutes, reports from newspapers and radio stations scattered over Colorado, Wyoming, Utah, Kansas, Nebraska and New Mexico. I asked him to give me an hour to digest what he had read, and I would call him back. He said O.K.

In an hour I called to tell him that the reports all had to do with a meteorite that had come in from the north and had ended up somewhere over central Colorado and nowhere near where the Lt. Commander thought he dodged it. I further told the Editor that I thought something should be done to clear up the matter because such mistakes might lead to serious consequences. He wanted my advice. (The Denver Post had been calling on me for information on meteors, etc for 15 years.) I said "If you will pay our expenses, Mrs. N. and I will start out tomorrow making a survey that will tell us where this thing landed and may, hopefully, lead to a better understanding of how to interpret such incidents. Knowing how to distinguish between meteorites and missiles just might prevent a war." I estimated that the survey would take about 10 days of driving and interviewing and that a few hundred dollars might cover the expense. An hour later he called authorizing the survey. We started out the next day.

It was a bit surprising how many people had been out at 5 o'clock in the morning; but all of those who were out were treated to a sight never to be forgotten. For some it was a frightening experience; for others it was a sobering puzzle. To all the fireball seemed to be very near to them, just over the first hill, whether in Colorado, Wyoming, Nebraska, Kansas or New Mexico; except in the little village of Eagle, Colorado, there they all thought it failed to reach the earth - "died out high up above the village - just burned out." It made a terrible noise though and shook the earth, "pretty frightening." One elderly couple, living a few miles south of town, were awakened by the noise and then heard the thudding of heavey objects hitting nearby.

We had determined by the sightings in several states that the vanishing point of the fireball was at a height of about 12 miles.

What constitutes dependable (usable) reports?

We were perfectly aware of the fact that none of the people we interviewed were able to give us precisely accurate information. All of the estimates of size, distance and height were ignored; but, if we had a person take us to the spot where he or she was standing or sitting when the sighting was made and point for us, we could be certain as to the direction the meteor was from the witness. When we heard from 2 persons 200 miles apart, each sure of the approximate spot a half mile away where the meteorite hit, surely both were mistaken; but if we had each take us to the spot from where the sighting was made and point to the place in the field "where it hit," then we could record that sighting by a line on our map and do the same with other witnesses in other locations, then we had useful information. Where the lines crossed would be the general area where the fall occurred. Such witnesses need not be highly educated - it was a simple common sense proposition. But if either an expert scientist, or a plow-boy, stated that he saw the meteor or fireball and points to "the spot where it hit," his testimony might be very misleading if taken literally. A reputable astronomer was alerted to the great meteor of March 24, 1933. Seeing the large dust cloud that it had left in its wake, he started driving toward it thinking it was not far away. After driving 40 miles and the cloud appearing no nearer, he turned toward home, which was just as well since the cloud was 150 miles from him when he started and was drifting faster than he drove. Another respected scientist, hoping to recover a meteorite that he saw, he thought nearby, went 10 miles and inquired of those who had seen it. They indicated that it landed about ten miles farther on. Another 10 miles yielded similar information, and he turned back.

Many such examples could be cited, but these are sufficient. These people were not stupid. They simply acted on insufficient information. Years ago I learned that the question of meteorites was not discussed in courses in geology and astronomy in any college or university where I inquired. I hope the situation has improved, but during those years after I lectured before the New England Geological Society, a Professor came to my hotel room and said he had taught Geology in Harvard for 25 years, and he never thought there was as much to be learned about meteorites as he had learned that evening.

The fact that nothing had been taught about tracing fireballs to their landings was and is no doubt, a principal reason why the excitement about UFOs is such a lively subject today. I've investigated a lot of UFOs, and they always turned out to have been meteors, fireballs, weather ballons, birds or even high flying insects, aeronautic spiders, thistle tufts or dandylion seeds, etc., in addition to various types of aircraft in peculiar light-reflecting situations.

There was the case of a formation of UFOs flying over a city. The item received worldwide publicity for the reason that the sighting was by 2 college professors, (geologists), who estimated that the UFOs were flying at perhaps 65,000 ft. altitude and at miles per second. The fact that the professors estimated height and speed should have given the incident a zero rating because no person without special instrumentation can make a dependable estimate of distance, size or speed of any object in the open sky. It so happened that a young man, with no special training other than what had been obtained when duck-hunting, saw this interesting bevy of ducks go over the city several successive evenings. They were white-bellied ducks, and this reflected the city lights rendering them quite visible; but because the sighting had been made by important people, the news media gave it worldwide coverage. I saw the incident publicized 2 or 3 years later in some widely respected news media.

Confusion on the part of a newsman is understandable. Reports come from puzzled or frightened individuals hundreds of miles apart, each of them insisting that a fearfully bright object was seen only a few yards or perhaps half a mile from him. To the newsman, for whom a scoop is of high priority, there is not time to travel hundreds of miles to check these reports. By telephone he can check the reputations of each source, and so the reports are publicized. The paper by which he is employed is a respected news source, and the report is quoted far and wide.

The facts were that each of the widely distributed witnesses was honest, but his estimate of distance was greatly in error so that honest, respected citizens gave false reports which were broadcast as truth.

During much of the Second World War, I was doing geological work in New Mexico. One evening after working hours, as I drove to my quarters, my attention was drawn to a huge flock of Starlings, evidently enjoying a recreative flight before going to roost. I must say that starlings are expert flyers, capable of maintaining perfect flock formation. I pulled to the side of the road and watched.

The flock, which I estimated at 200,000 birds, confined its maneuvers to the air above a field of about 80 acres adjacent to the road from which I had pulled off to watch and, as it turned, dipped and rose; swung from right to left and back again, apparently keeping the volume of the flock about constant but its dimensions changing constantly. I watched in amazement. The distance between birds never seemed to vary even though they were so close together that collisions or at least wing-touching seemed envitable; yet I failed to see even one such contact.

The general appearance of the flock was very dark gray; but often it appeared black, and osmetimes, as its orientation relative to the low afternoon sun was just right, it shone like silver; and with another turn the flock almost disappeared. I wondered if there was a recognized leader or general who gave orders when the form of the flock changed form a spindle shape to a nearly round ball or again lengthened into a cigar shape. Up and down, round and round with the grace of a Viennese Waltz it went, then suddenly came to rest on the dark soil and seemed to vanish.

I described this performance in a letter to Addie*as I sat there in my car, and I said "Please file this letter because the performance I am watching is so unusual I might some day find reason to refer to it. And it is well she did.

Year later the U.S. Air Force made an extensive survey of flying saucer reports which was published in Life Magazine. In their report it was stated that they had been able to explain in terms of known natural phenomena all of the several thousand sightings with the exception of 12, and these 12 were briefly reviewed. One of them was a perfect description of what I had witnessed that afternoon, only the witness had viewed it from a distance such that individual birds were not distinguishable.

When driving along the Gunnison River in Colorado, I saw on a mountain slope on the opposite side of the river a gray serpentine apparition of great length. I stopped and reached for my binoculars, and the object resolved into a large flock of sheep strung along a crooked narrow mountain trail being led and driven, no doubt, to better pasture.

The hundreds of flying saucers or UFO sightings that have been reported to me since 1947 were always witnessed from a single location. Often the sightings were multiple and this fact was emphasized as proof of reality. But multiplicity is not so important as two or more sightings from different locations. The most confusing aspect of the UFO problem is that judgements are made on the basis of sightings from a single location.

A case in point: Two welders in Artesia, New Mexico, came to my trailer house one night just as I was starting to get ready for bed. They said they heard me give a talk to the Chamber of Commerce a while back and they wondered if I could hilp them out. They said they had been working on a construction in the central part of the village when just about 5 o'clock they saw a small plane fall burning right in the southern part of the village. They had gone to look for it and had gone on foot and by jeep, but could find nothing. They wondered if what they saw could have been a meteor. I looked at the clock and said "Let's hurry over to the theater. It will close in a few minutes." I handed the projection

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* Dr. Nininger's wife. - Ed.

operator a note requesting that any person from Maljamar, a village 40 miles east of Artesia, who may have seen a strange light in the southwestern shy about 5 o'clock please see me at the door. Two people stopped to tell me they saw it, and they pointed in the same direction as did the welders even though they were 40 miles east of where the two welders were. El Paso was 120 miles west, and I said "Let's go to my office." There I telephoned to the principal newspaper in that city asking if it had received any calls regarding a burning plane or other fire in the sky about 5 o'clock. Yes, they had been flooded with calls. "The plane came down right across the river south of the city," but they had not yet had time to go and investigate. I said they needn't go and explained who I was and what had happened. I asked them to run a note in their morning paper asking that others who saw the phenomenon report to me. They did and reports came from many parts of Arizona, New Mexico and Texas and also one from Chihuahua City Mexico which said that west of that city some 40 miles windows rattled and noise like thunder was heard over a wide area.

Regardless of how convincing a report sounds, it is always a good thing to get several reports from widely different areas before placing much reliance on any report such as "I saw right where it hit." For example, in 1931 I had been working on a fireball for several days and had pretty well pinned it down to a certain county in Kansas when the Museum Director where I worked called me in to show me a letter. As he handed it to me, he said "There were two meteorites that day. This fellow can't be referring to the one you've been working on, for he is 200 miles away from your location, and he saw his meteorite hit and kick up dust within 1 1/2 miles of him." I read the letter, and after a moment's hesitation, said, "Director, I think he is talking about the same one I've been working on." Somewhat irritated, he replied, "You can't ignore a letter like that; the man saw it hit. If you ignore him he'll bring in a meteorite and make a fool of you, and of the Museum too." I said "I'm not going to ignore the man; but when I go to see him I expect him to take me to where he was standing when he saw that thing, and he will point in the direction of the area that I have zeroed in on." The Director was not in a very good mood when I left his office.

I drove to the town where the letter had been posted, namely Holly, Colorado, and called the man's telephone number. Now his letter gave the impression that the meteorite had landed southeast of the town of Holly, and my meteorite location was 200 miles north, northeast of Holly, so it is not difficult to see why the Director was a bit impatient with me. When the man came to the phone, I said, "I came in answer to your letter to the Museum; where do you live?" He said he lived 14 miles southeast of town and told me how to reach his place.

When I arrived I asked him to take me to the spot where he stood when he saw the big meteor hit. He led me out to the yard gate and said he was just opening the gate when he saw this thing and "it hit right up there beyond that clump of Cottonwood trees." The trees were north and a little east, about 1 1/2 miles from us. I took a compass bearing and the spot lined up perfectly with my location 200 miles away. I explained to this man that the dust he saw kicked up was actually the meteoritic dust cloud left in the wake of the meteorite and not soil dust, and that the meteorite had ceased to burn while still 10 or 12 miles high; and due to the curvature of the earth and a slight ridge where the Cottonwood trees were, the meteorite was actually over his horizon when he saw it hit.

Soon after Mr. Glenn Huss began working at the Meteorite Museum in Sedona, Arizona, he returned from a drive to Cottonwood one day and reported that he had seen what he believed was a meteorite fall ahead of him just as he was approaching the Spring Creek bridge. This would place it a little west of south. We packed bags and started to Phoenix, a little more than 100 miles. Arriving there we went to the control tower on the Sky Harbor Airport, got permission to interview the man in charge and told him our mission. He had seen the big meteor, and he thought it landed; but he, wtith some others of the force, searched the southwest part of the landing field and found nothing. (It is hardly needful to point out that 40 years ago Sky Harbor was not the busy place it is today.) Well, Glenn and I didn't want to embarrass an important employee by telling him that we were sure the object he saw was far beyond Phoenix, and we went on to Tucson. There, Mr. Rothrock, a high school teacher of science - and a one time student of mine - told us that several of his students saw the big fireball, and he put us in touch with 2 or 3 of them. After taking a bearing from Tucson it looked like our meteorite probably landed in Old Mexico; but we drove over to Ajo to get a cross bearing and did get some very good lines on it and, sure enough, it had ended up over some of the most rugged uninhabited mountainous country in Mexico. Needless to say we did no searching.

What constitutes dependable reports of sightings? Dr. Geo. P. Merrill one time wrote that human testimony cannot be used in the identification of meteorites. This, of course, is not exactly correct. Without human testimony science would never have learned that meteorites exist. What he was trying to warn against was the acceptance of human judgement concerning matters such as distance, size, velocity, heat, chemical or mineralogical content.

For example, a superintendent of schools, after hearing one of my lectures, spoke to the effect that since I had convinced him that I had great interest in securing meteorites, he would gladly point out just where he had seen one fall. He then went to a window and pointed to a spot on a slight ridge, perhaps 800 yards away, saying "a gerat fireball struck just to the east of the Cottonwood tree". It was early one morning just before daylight. The time of day and the direction that he pointed rang a bell. I said, "Just when was this?" He couldn't give the exact date, but his wife spoke up, "I know; it was on my sister's birthday, and we were up early in preparation to go and celebrate. It was March 24 of last year." He agreed. I said, "Would you believe me if I told you that I picked up several pieces of that meteorite more than 300 miles from here, but in exactly the direction you have pointed?" I explained that the fireball vanished at a height of 17 miles leaving a cloud of dust just such as he described.

When the daylight meteor or fireball was seen on August 8, 1933, north northeast of Denver, Addie and I, with packed suitcase, started north toward Cheyenne, Wyoming. We stopped in Greeley, about 50 miles from Denver, to inquire at the newspaper office for names of people who had reported the incident. The Editor said there were many and that Mr. X saw it hit in his field northeast of town, but he didn't want anybody going out there, tramping over his crop. I said we wouldn't but would like to talk to some of the townspeople right where they stood when they saw it. He gave us names, and we went, took their bearings and then headed for Cheyenne, 50 miles farther north. There we took bearings again and went on another 85 miles. Then we turned east because now our witnesses pointed slightly south of east.

All of the people we had interviewed gave us useful information, but we never went to search in the locations any of them had suggested. After traveling east, until those we interviewed pointed south, we turned south, and when witnesses told us it burned up high in the sky we knew we were in the general area of the fall. We got 30 meteorites from an elliptical area 10 miles in length.

What the general public does not seem to realize is that no one is able to reliably estimate the distance to objects in the air. Several years ago a number of people witnessed the flight of grasshoppers a few hundred feet overhead. Excitement was running high regarding flying saucers at that time, and soon estimates were being made concerning the size and speed of those UFOs. A Wellsian invasion from Mars was about to cause panic when an old man came by who had lived through the 1919 Grasshopper Plague in Kansas; he soon told the folks what they were seeing.

Several times I've been called to see a "silvery disk" in the sky in broad daylight only to find a weather balloon or, in some cases, the planet Venus!

In June of 1941 my son, Robert (Bob), Geologist Harvey Markman and I were headed north on Highway 85, about 50 miles south of Denver, when we were treated to the sight of a large fireball on our left traveling in a northerly direction. Bob immediately pulled out and stopped in time to see the fireball break into 3 red sparks and disappear about 3° above the horizon. One ranch lady, who was milking a cow, said the cow leaped kicking the bucket of milk, and the lady looked up to see what appeared like "a million sparks coming down." This was in the extreme northwestern corner of Colorado.

About the time I had finished my survey a call came from a pilot in Cheyenne, Wyoming, saying he had been sent out by the C.A.P. in Kansas City to rescue a pilot who had been seen to fall in a burning plane north of Casper, Wyoming, and that he had been searching for 3 days and had found nothing. He had seen by the paper that I had been working on a meteor, and he wondered if he had been "given a bum steer." I asked about the exact time, the location of the sighting and of the man who made it. As it turned out, the man who said he had seen the burning plance come down had been driving toward Casper, Wyoming, from the north, and the plane came down just off the highway ahead of him a short distance west of the highway. The time was the same as when we had seen the big fireball!

CONCLUSION: The driver who was sure he had seen a burning plane fall near him was 270 miles northeast of where the fireball ended. Out of sympathy he stopped and searched, hoping he might render help to a pilot in distress within what he judged was a half mile from him. Failing to find any plane, he was still so certain that the plance had fallen in the area that he reported it as a fact, and a search and rescue pilot was sent out from a base 675 miles distant.

From my 40 years of hunting meteorites, I could cite scores of similar mistakes made by all sorts of intelligent citizens, including college professors, air-force personnel, and scientists trained in their particular profession; but apparently no discipline includes a training in the very important procedure of determining where an observed meteorite falls. There was the instance when the pilot flying from Los Angeles to El Paso reported a plane crash right across the Mexican border when what he saw was a meteorite which landed 200 miles south of the border, west of Chihuahua City. And there was a pilot who said he dipped the wing of his plane to save his passengers from the meteor he saw "coming at him." A survey proved that the meteor, a large one, passed about 60 miles north of him; and another pilot said he dived into a canyon in California to avoid a collision when the fireball was actually many miles distant. Then there was the pilot who boasted that he had watched a spectacular fireball from a "ringside seat" where he had the unique privilege of "looking down" on the great meteor of March 24, 1933. I began a survey of that one immediately after it was reported and had the good luck to collect quite a number of meteorites that it dropped. The fireball quit burning at an altitude of 17 miles, and the pilot was flying at only a height of a few thousand feet!

If pilots and other intelligent people were taught the facts about meteors and fireballs, there would not be all the fuss about "flying saucers and UFOs." There lack of knowledge about meteorites and fireballs could cause serious consequences, as consider the case of the Lt. Commander in 1957 who said he saw a fiery object coming in what he considered a collision course, so he banked and turned to avoid it and saw it disintegrate under him, starting some forest fires. He reported all this to the North American Defense command at Colorado Springs, Colorado, NORAD.

NORAD sent an investigative delegation to investigate but these men returned utterly confused. NORAD has the tremendous responsibility of protecting our nation from possible missiles from Russia. But, if a great meteor such as that just described can confuse them, what protection do they provide?

SUGGESTED FURTHER READINGS
(R. Westrum)

H.H. Nininger, "What Constitutes Reliable Data Regarding Meteors or Fireballs?" Popular Astronomy, 41, #7 (1933), 367-370.

C.C. Wylie, "Psychological Errors in Meteor Work," Popular Astronomy, 47 (1939), 206-209.

Wiiliam Hartmann, "Processes of Perception, Conception, and Reporting," in University of Colorado, Scientific Study of Unidentified Flying Objects (New York: Bantam, 1968).

"FINGERTIP SIGHT": A BIBLIOGRAPHY

LEONARD ZUSNE

Amrine, M. Psychology in the news. American Psychologist, 1964, 19, 218.

Ancona, L., & Bellagamba, A. Percezione dermo-ottica: un caso clinico. [Dermo-optical perception: A clinical case.] Archivio di Psicologi, Neurologia e Psichiatria, 1980, 51(3), 305-331.

Barrett, S. M., & Rice-Evans, P. Sensitivity of hands to visible light. Nature, 1964, 203, 993.

Bashkirova, G. Devochka-"sensatsiya." Znanie-Sila, 1964(9), 40-44. Transl.: Little-girl "sensation." International Journal of Parapsychology, 1965, /(4), 379-394.

Becker, H. E., & Cone, R. A. Light-stimulated electrical responses from skin. Science, 1966, 154, 1051-1053.

Bikard, V. Sootvetstvie mezhdu vysotoĭ i stabil'nosti krasotnykh granits. [The relationship between height and stability of color boundaries.] In Voprosy Slozhnykh Issledovaniĭ po Dermo-optike. Sverdlovsk: Pedinstitut, 1968.

Boirac, E. L'avenir des sciences psychiques. [The future of psychical sciences.] 1917.

Bongard, M. M., & Smirnov, M. S. O "kozhnom zrenii" R. Kuleshovoĭ. [On the "dermal vision" of R. Kuleshova.] Biofizika, 1965, 10(1), 148-154.

Brewer, F. A. Samuel Johnson on dermo-optical perception. Science, 1966, 152, 592.

Buckhout, R. The blind fingers. Perceptual and Motor Skills, 1965, 20, 191-194.

Buckhout, R. Dermo-optical perception. Science, 1966, 152, 1109-1110.

Can fingers seè? _Time_, February 6, 1964.

Chertok, L. Recherches et doutes sur la vision extra-rétinienne. [Research and doubts on extra-retinal vision.] _Encéphale_, 1964, _53_(6), 686-699.

Christopher, M. _Mediums, mystics, and the occult_. New York: T. Y. Crowell, 1975.

Color identification by fingertips. _USSR_, February 1964, p. 35.

Davy, J. (Interview with Jules Romains on eyeless vision). _Observer_, February 2, 1964.

Devushka s "vidyashchimi" pal'tsami. [The girl with the "seeing" fingers.] _Uchitel'skaya Gazeta_, June 27, 1964.

Dinwiddie, D. Midwestern woman "sees" color through her fingertips. _Popular Mechanics_, 1964(4), 89.

Dobronravov, S. N., & Fishelev, Y. R. Fenomenal'naya chuvstvitel'nost' kozhnykh retseptorov. [The phenomenal sensitivity of skin receptors.] In _Sbornik Materialov Nauchnoĭ Sessii Vuzov Ural'skogo Ekonomicheskogo Raĭona_. Sverdlovsk, 1963.

Dobronravov, S. N., & Fishelev,Y.R. O "kozhnom zrenii." [On "dermal vision."] _Byulleten' Eksperimental'noĭ Biologii i Meditsiny_, 1964, _58_(8), 13-16.

Dobronravov, S. N., & Fishelev, Y. R. O vozmozhnosti razvitiya kozhnogo zreniya u cheloveka. [On the possibility of developing dermal vision in man.] _Izvestiya APN RSFSR_, 1964(133), 190-195.

Duplessis, Y. _The paranormal perception of color_. New York: Parapsychology Foundation, 1975.

"Eyeless vision" unmasked. _Scientific American_, 1965(3), 57.

Farigoule, L. _La vision extrarétinienne et le sens paroptique_. [Extra-retinal vision and the paroptic sense.] Paris, 1921.

Fingertip sight. _Newsweek_, December 30, 1963.

Fishelev, Y. R., & Dobronravov, S. N. O "kozhnom zrenii" u cheloveka. (K fenomenu Rozy Kuleshovoi.) ["Cutaneous vision" in man. (The phenomenon of Roza Kuleshova.)] _Uchenye Zapiski Sverdlovskogo Pedagogicheskogo Instituta_, 1967, sb. 33, 52-62.

French, C. N. Tactile-vision: Thermal and texture cues in the discrimination of black and white. Nature, 1965, 208, 1352.

Frey, A. H. Behavioral biophysics. Psychological Bulletin, 1965, 63, 322-337.

Gardner, M. (Exchange of letters with R. P. Youtz on eyeless sight). New York Times Magazine, April 5, 1964.

Gardner, M. (On eyeless sight). Linking Ring, 1964, 34, 23-25.

Gardner, M. Dermo-optical perception: A peek down the nose. Science, 1966, 151, 654-657.

Gellershteĭn, I. Ne ogranichivat' poisk. [Don't set limits to the search.] Nauka i Religiya, 1966, No. 3.

Gilev, D. K. Kharakteristika taktil'nykh i temperaturnykh priznakov tsveta. (K fenomenu Rozy Kueshovoĭ). [The nature of tactile and temperature cues of color. (The phenomenon of Roza Kuleshova).] Uçhenye Zapiski Sverdlovskogo Pedagogicheskogo Instituta, 1967, sb. 33, 81-89.

Gol'dberg, I. M. K voprosu o uprazhnyaemosti taktil'noĭ chuvstvitel'-nosti. Voprosy Psikhologii, 1963(1), 35-40. Transl.: On whether tactile sensitivity can be improved by training. Soviet Psychology and Psychiatry, 1963, 2(1), 19-23.

Gol'dberg, I. M.Fenomen Rozy Kuleshovoĭ. The phenomenon of Roza Kuleshova. Uchenye Zapiski Sverdlovskogo Pedagogicheskogo Instituta, 1967, sb. 33, 23-51.

Gol'dberg, I. M. O raspoznavanie priznakov tsveta cherez metal-licheskie ekrany. [On the recognition of color cues through metallic shields.] In Voprosy Slozhnykh Issledovaniĭ po Dermo-optike. Sverdlovsk: Pedinstitut, 1968.

Groth, G. He writes with your hand. Fate, 1952, 5, 39-43.

Houswife is unable to repeat color "reading" with fingers. New York Times, February 2, 1964, 77.

Ivanov, A. Soviet experiments in "eye-less vision". International Journal of Parapsychology, 1964, 6, 5-23.

Jacobson, J. Z., Frost, B. J., & King, W. L. A case of dermooptical perception. Perceptual and Motor Skills, 1966, 22, 515-520.

Khvorin, A. I. Redkaya forma giperstezii vysshykh organov chuvstv. [A rare form of hypersensitivity of the higher sense organs.] Voprosy Nervno-Psikhicheskoĭ Meditsiny (Kiev), 1898(2, 3, 4), 3.

Kolesnikov, M. M., Filimonov, Y. M., & Belousov, V. N. K voprosu o fizicheskoĭ prirode kozhno-opticheskogo chuvstva. (K fenomenu Rozy Kuleshovoi). [The question of the physical nature of the dermooptical sense. (The phenomenon of Roza Kuleshova).] In Uchenye Zapiski Sverdlovskogo Pedagogicheskogo Instituta, 1967, sb. 33, 116-128.

Konstantinov, B. Shestoe chuvstvo. [The sixth sense.] Kul'tura i Zhizn', 1964(5), 40-41.

Kozhnoe "zrenie." [Skin vision.] Tashkent: Ob"edinennoe Izdatel'stvo TsKKP Uzbekistana, 1967. (A 16-page collection of articles in Uzbek).

Kozhvenikov, M. M. Problema kozhno-opticheskoĭ chuvstvitel'nosti. (K fenomenu Rozy Kuleshovoi). In Uchenye Zapiski Sverdlovskogo Pedagogicheskogo Instituta, 1967, sb. 33, 5-22.

Krupnov, A. Individual'nye razlichiya v vysote tsvetovykh granits. [Individual differences in the height of color boundaries.] In Voprosy Slozhnykh Issledovaniĭ po Dermooptike. Sverdlovsk: Pedinstitut, 1968.

Leont'ev, A. N. Problemy razvitiya psikhiki. [Problems of mental development.] Moscow: Izdatel'stvo APN RSFSR, 1959.

Liddle, D. Fingertip sight: Fact or fiction? Discovery, London, 1964, 22-26. Also in R. S. Daniel (Ed.), Contemporary Readings in General Psychology. Boston: Houghton Mifflin, 1965, pp. 299-302.

Lozanov, G. [Suggestology and suggestopedia.] Sofia: Institute of Suggestology, 1969.

Makous, W. L. Dermo-optical perception. Science, 1966, 152, 1109.

Makous, W. L. Cutaneous color sensitivity: Explanation and demonstration. Psychological Review, 1966, 73, 280-294.

Mashkova, V. Sharpsighted fingers. International Journal of Parapsychology, 1965, 7, 368-370.

Momchev. [Skin sight in Bulgaria too: A visit to the experimental parapsychologist.] Narodna Mladej, Science Section, April 26, 1965.

Nash, C. B. Cutaneous perception of color with a head box. Journal of the American Society for Psychical Research, January 1971.

Nauchnye iziskaniya psikhologov na Urale. [Scientific investigations of psychologists in the Urals.] Nauka i Zhizn', 1962(11), 104.

Nevel'skiĭ, P. B. et al. Snova o Tagil'skoĭ zagadke. [The Tagil mystery revisited.] Nauka i Zhizn', 1963(2), 92-96.

New "eyeless vision" case reported in Massachusetts. Newsletter of Parapsychology Foundation, 1964, 11(6), 2.

Novomeĭskiĭ, A. S. Nauchnye iziskaniya psikhologov na Urale. [Scientific investigations of psychologists in the Urals.] Tagil'skiĭ Rabochiĭ, 1962, No. 202.

Novomeĭskiĭ, A. S. Rol' kozhno-opticheskogo chuvstva v poznanii. Voprosy Filozofii, 1963(7), 131-139.

Novomeiskiĭ, A. S. O prirode kozhno-opticheskogo chuvstva u cheloveka. Voprosy Psikhologii, 1963(5), 99-117. Transl.: The nature of the dermo-optical sense in man. International Journal of Parapsychology, 1965, 7(4).

Novomeĭskiĭ, A. S. Sdvigi v kozhno-opticheskom chuvstve pri raznykh usloviyakh osveshcheniya. [Changes in dermo-optical sensitivity under different conditions of illumination.] In Voprosy Slozhnykh Issledovaniĭ po Dermooptiki. Sverdlovsk: Pedinstitut, 1968.

Novomeĭskiĭ, A. S., Bikard, V., & Krupnov, A. Lokalizatsiya tsvetochnoĭ granitsy na osnove ostatochnogo izlucheniya. [Localization of the color boundary based on residual radiation.] In Voprosy Slozhnykh Issledovaniĭ po Dermooptike. Sverdlovsk: Pedinstitut, 1968.

Novomeĭskiĭ, A. S., & Yaklov, B. O vozmozhnoĭ chuvstvitel'nosti kozhno-opticheskogo chuvstva u cheloveka. [On the possible sensitivity of the dermo-optical sense in man.] In Voprosy Slozhnykh Issledovaniĭ po Dermooptike. Sverdlovsk: Pedinstitut, 1968.

Nyuberg, N. D. "Zrenie" v pal'tsakh. Priroda, 1963(5), 61-67. Transl.: "Finger-sight" - the phenomenon of Rosa Kuleshova. Proceedings of the Federation of American Societies for Experimental Biology, 1964, 22, T701-T705.

Nyuberg, N. D. "Zrenie pal'tsami" i "yasnovidenie." ["Fingertip sight" and "clairvoyance."] Priroda, 1964(6), 74-76.

Oppel, T. W., & Hardy, J. D. Studies in temperature sensation: (1) A comparison of the sensation produced by infrared and visible radiation. Journal of Clinical Investigations, 1937, 16, 517-540.

Ostrander, S., & Schroeder, L. Psychic discoveries behind the Iron Curtain. New York: Bantam Books, 1970. Pp. 297-298.

Ostrander, S., & Schroeder, L. The ESP papers. New York: Bantam Books, 1976.

Pat Marquis of California can see without his eyes. Life, April 19, 1937, 57-59.

Pick, H. L., Jr. Perception in Soviet psychology. Psychological Bulletin, 1964, 62, 21-35.

Platonov, K. Fenomen Rozy Kuleshovoĭ - ne izklyuchenie. [The phenomenon of Roza Kuleshova is not an exception.] Nauka i Zhizn', 1963(2), 106.

Plumb, R. K. Woman who tells colors by touch mystifies psychologist. New York Times, January 8, 1964, 23. Also in Science Digest, 1964(4), 5-7.

Plumb, R. K. Sixth sense hinted in finger "seeing." New York Times, January 26, 1964, 114.

Price, H. Confessions of a ghost-hunter. New York: Putnam, 1936. Ch. 19.

Rhine, J. B. (Reports on having caught Pat Marquis nose-peeking in an eyeless vision test). Parapsychology Bulletin, 1963, 66, 2-4.

Robinson, L. W. We have more than five senses. New York Times Magazine, March 15, 1964.

Romains, J. Vision extra-rêtinienne. Paris, 1919. Transl. by C. K. Ogden as Eyeless sight. New York: G. P. Putnam's Sons, 1924.

Romains, J. A fruitful series of experiments. International Journal of Parapsychology, 1965, 7, No. 4.

Rosenfeld, A. Seeing colors with the fingers. Life, June 12, 1964, 102-113.

Ruki vidyat. Sovetskiĭ Soyuz, 1963(4). Transl.: Seeing hands. USSR, 1964(2), 32.

Russian "eye-less" vision symposium. Newsletter of Parapsychology Foundation, 1965, 12(2), 4-5.

Russians investigate fingertip vision. Fate, 1964(5), 46-48.

Saltzman, P. Testing the fingertip vision of Pat Stanley. Fate, 1964(5), 38-45.

Seeing fingertips. Time, January 25, 1963.

Sheval'ev, A. From sensational uproar to serious research. International Journal of Parapsychology, 1965, 7(4), 371-374.

Snyakin, P. G. K voprosu o razvitii vzaimosvyazi glaznogo i kozhnogo svetovospriyatiya u cheloveka. [On the problem of the development of ocular and dermal photoreception.] Byulleten' Eksperimental'noĭ Biologii i Meditsiny, 1964, 58(8), 16-21.

Soviet interest continues. Newsletter of Parapsychology Foundation, 1964, 11(6), 3.

Steinberg, D. D. Light sensed through receptors in the skin. American Journal of Psychology, 1966, 79, 324-328.

Steven, D. M. The dermal light sense. Biological Review, 1963, 38, 204-240.

Svoren', R. Zrenie v konchikakh pal'tsev. [Fingertip sight.] Izvestiya, June 13, 1963.

Tarbell, H. X-ray eyes and blindfold effects. In The Course in Magic. New York: Tannen, 1954. Vol. 6, pp. 251-261.

Teplov, L. (On eyeless sight). Nedelya, March 1-7, 1964.

Teplov, L. (On eyeless sight). Literaturnaya Gazeta, May 25, 1964.

Trisvyatskaya, V. She "sees" with her fingers. Fate, 1963(7), 26-34.

Tsvet na oshchup'. Color perception through touch. Izvestiya, October 24, 1962, 6. Transl.: Seeing fingertips. USSR, February 1964, 34.

Views on "eye-less vision": An international symposium. International Journal of Parapsychology, 1965, 7(4), 435-449.

Weintraub, D. J. Dermo-optical perception. Science, 1966, 152, 1109-1109.

Wimsatt, W. K., Jr., & Pottle, F. A. (Ed.), Boswell for the defense. London: Heinemann, 1960. P. 134.

Youtz, R. P. Aphotic digital color sensing: A case under study. Paper read at the meeting of Psychonomic Society, August 29, 1963, at Bryn Mawr.

Youtz, R. P. (Exchange of letters with Martin Gardner on eyeless sight). New York Times Magazine, April 26, 1964.

Youtz, R. P. Aphotic digital color sensing — a progress report. Paper read at the Eastern Psychological Association meeting, Philadelphia, April 1964.

Youtz, R. P. The case for skin sensitivity to color; with a testable explanatory hypothesis. Paper read at Psychonomic Society meeting, Niagara Falls, Ontario, October 9, 1964.

Youtz, R. P. Letter to the Editor. Scientific American, 1965(6), 212, 8-10.

Youtz, R. P. Tactile color sensing under three intensities of illumination. Paper read at Eastern Psychological Association meeting, New York City, April 1966.

Youtz, R. P. Dermo-optical perception. Science, 1966, 152, 1108.

Youtz, R. P. Can fingers see color? Psychology Today, 1968, 1(9), 37-41.

Youtz, R. P., & Broome, R. S. Cutaneous color discrimination (DOP) at two stimulus temperatures. Proceedings of the 77th Annual Convention of the American Psychological Association, 1969, 17-18.

Zavala, A., Van Cott, H. P., Orr, D. B., & Small, V. H. Human dermo-optical perception: Colors of objects and of projected light differentiated with fingers. Perceptual and Motor Skills, 1967, 25, 525-542.

Zielinski, L. Dr. A. N. Khvorin and the Tambov experiments. In E. J. Dingwall (Ed.), Abnormal hypnotic phenomena, Vol. 3. New York: Barnes & Noble, 1968.

Zrenie bez glaz. [Eyeless sight.] Tekhnika dlya Molodezhi, 1963, No. 4; 1965, Nos. 2, 5.

Zubin, J. Dermooptical perception: A cautionary report. Science, 1965, 147, 985.

GUARDIANS OF ORTHODOXY:

The Sponsors of the Committee for the Scientific Investigation of Claims of the Paranormal

R.A. McCONNELL & T.K. CLARK

Uncle Remus' tale of sTARBABY has been told (Rawlins, 1981a) and pre-told (Curry, 1982a) and discussed (Abell & Kurtz, 1981; CSICOP Council, 1981; Curry, 1982b; De Marré, 1982; Dommanget, 1982; Eysenck, 1982; Gauquelin, 1982; Good, 1982; Hoebens, 1982; Kreps, 1982;;Rawlins, 1981b; Truzzi, 1982) until one might think there was little left to be said. And yet, our feeling has been from the beginning that emphasis has been misplaced.

Who are the important characters in this story? Tarbaby? Brer Rabbit? Uncle Remus? Nonsense! Their tale is as old as time itself, and nothing will be gained by lamenting animal frailty. What is new in this instance is a supporting cast without which there could be no children's bedtime story. We refer, of course, to the Fellows and Consultants of the Committee for the Scientific Investigation of Claims of the Paranormal. It is their prestigious support that makes The Skeptical Inquirer an economically successful venture, and it is to them we should look for the moral to this tale.

With this in mind, RAM sent the following letter in September, 1981, to the sponsors of CSICOP.

* * * * *

Dear ______

Because you are listed as a public supporter in the Skeptical Inquirer, the journal of the "Committee for the Scientific Investigation of Claims of the Paranormal," I am writing to you for assistance in understanding that organization.

Perhaps you have already seen the enclosed paper "sTARBABY" from Fate magazine for October, 1981, in which Dennis Rawlins describes his experiences as a member of the Executive Council of the Committee. If you have not yet read it, may I suggest that it is worthy of your attention....

[This was followed by a three-paragraph characterization of sTARBABY from Rawlin's point of view.]

On the basis of personal knowledge gained directly from present and past members of the Executive Council of the Committee, I am convinced that the Rawlins report is certainly true in broad outline and probably true in every detail....

The dispute in which Rawlins was involved concerns astrology. From my publications on the sociology of scientific controversy, you already know that I have no more sympathy for astrology than I do for the Scientific Creationists' denial of Darwin. What draws my interest to the present case is that the Committee's actions in opposing astrology parallel those of the Creationists in opposing evolution. Both the Committee and the Creationists attempt by semanti-magical methods to resolve a scientific controversy. The parallel is not exact, however, because the Creationists are honorable men who do not violate their religious principles; while the Committee, by deceiving the public, defiles the cause it defends....

I would like to explore the psychological mechanisms by which a professional philosopher, almost singlehandedly, managed to deceive so many scientists and scholars into publicly supporting an intellectually dishonest enterprise. I hope you will take time to write to me your opinion of the Rawlins report and that you will answer the following question for me: In the light of Rawlins' revelations, do you intend to continue your public association with the Committee for the Scientific Investigation of Claims of the Paranormal?...

Sincerely yours,

* * * * *

Out of 89 sponsors (Fellows, Consultants, etc.) listed by name in The Skeptical Inquirer, Vol. 5, No. 3 (Spring, 1981), RAM's above-shown letter of inquiry was sent (and not returned as undeliverable) to 75 sponsors (84%). From the latter, replies were received from 39 (52%).

In the table, sponsors are grouped as writers, psychologists, philosophers, astronomers, and miscellaneous. The "miscellaneous" category includes other scientists, educators, engineers, medical doctors, a few magicians, and several sponsors whose profession or training could not be determined.

The 39 replies were categorized in three ways: according to criticism expressed, judgmental action underway, and substantive action planned in response to sTARBABY. The subheading "Will reserve judgment" was used for cases where there was no indicated intention to pursue Rawlins' charges. The classification "Satisfied with CSICOP" contains those sponsors who indicated that they were satisfied as to the falsity, irrelevance, or triviality of Rawlins' charges. Two replies, not listed in the table, came from sponsors currently listed in The Skeptical Inquirer who said they had previously resigned their association with the Committee.

The only entries worthy of special mention are the 12 who said they were satisfied with CSICOP and the 17 who made it plain that they planned to continue their sponsorship of that organization. Their combined total was 15 after deleting four "defendants." This group presumably offers the hard core of support for CSICOP.

What these numbers do not reveal is the heterogeneity of CSICOP's sponsors. From their letters we could sense, but not measure, gamuts of personality and belief that we think would be incompatible in an organization that operated through democratic assembly.

RAM's reaction to this potpourri of letters was the following communication mailed to CSICOP's sponsors in December, 1981.

* * * * *

Dear ______

It had been my intention to reply individually to each of the Fellows and Consultants of CSICOP who were kind enough to respond to my September letter asking about their interest in the Rawlins sTARBABY report published in Fate, October, 1981. I regret that, to save time, I must send this same reply to all of you.

Those of you who know me may have noticed that my inquiry letter was not in my customary, reserved style. The purpose of that letter was to encourage a thorough ventilation of the Rawlins affair and to learn a little about the people who lend their names to the CSICOP enterprise. I assumed that busy persons would not likely respond to an inquiry from a stranger about a matter peripheral to their daily activities unless they felt challenged. I tried to phrase my letter to interest the widest possible spectrum of CSICOP sponsors....

I am not moved by sympathy for Rawlins as a former Council member of CSICOP. He describes himself as "unsuspicious." Given the history of the leadership of that organization, he might have expected what he got. (See "Marcello Truzzi Talks about the Crusade Against the Paranormal. Parts 1 and 2": Fate, 1979, September,70-76; October, 87-94.) I accept Rawlins' article as an honest expression of his perception of a series of psychologically complex situations. I am sure that some others who were involved must have perceived matters differently.

Several of you expressed indignation that I should have relayed Rawlins' accusations to you and given my opinion without waiting for the Executive Council of the Committee to present their side of the story in The Skeptical Inquirer. Such indignation seems to me unwarranted. The sponsors of CSICOP are not outsiders. Their reputations are at risk through CSICOP. They are judge, jury, and defendants in this case. The Fellows and Consultants have a right to become acquainted with every point of view as quickly as possible. Only in this way will they know where and how far to press for evidence while action is still possible.

Some of you expressed regret because you thought I had made up my mind about this case solely on the basis of Rawlins' indictment. Nothing could be further from the truth. In a controversy of this kind one begins by examining the public, printed statements. The heart of Rawlins' accusations lies in that section of his paper in Fate (October, 1981) starting at the middle of the second column on page 76 ("In the report,...") and ending at the bottom of the first column of page 78 ("...were statistically significant.") and deals with a paper by M. Zelen, P. Kurtz, and G. Abell, titled "Is There a Mars Effect?" which appeared in The Humanist (November/December, 1977, pp. 36-39). The latter should be read in conjunction with the paper that immediately preceded it: "The Zelen Test of the Mars Effect" By M. and F. Gauquelin (pp.30-35) and an editorial introduction (p. 29). The rest of the Rawlins paper might be regarded as mere supporting background. (Page numbers in Rawlins reprints = Fate numbers minus 66.)

In view of the fact that, as described in The Humanist, the so-called Mars effect in the Zelen subsample (303 champion athletes) matched in strength (22%) the effect in Gauquelin's entire data sample (2088 champions), whereas the newly gathered control sample (16,756 nonathletes) showed only the theoretically expected effect (17%), the subdivision and criticism by Zelen, Kurtz, and Abell of the 303-champion subsample was certain to confuse the unsophisticated or casual reader. For this reason, in my judgment, this Humanist article is intellectually dishonest. It is good magicianship but bad science, and it is especially reprehensible because of the antecedent relationship of these authors to the so-called "Zelen test." To make matters worse, after two years in which to reconsider their misrepresentation, the same authors reviewed their 1977 Humanist article and repeated the same inappropriate criticism in the Skeptical Inquirer (Kurtz, Zelen, and Abell, Vol. 4, No. 2 [Winter, 1979-80], Page 20).

If, as some of you have written, Rawlins' accusations are trivial, this is true only in the sense that they relate to a scientifically trivial topic--a topic blown out of proportion to make it of interest to the subscribers of The Humanist and The Skeptical Inquirer.

One of the most valuable aspects of Rawlins' paper as a sociohistorical document is the way in which he reveals the personal nastiness that often accompanies the inner dealings of an organization that works for the destruction of beliefs--be they true or false. I suppose this reflects the kind of persons who are attracted to an essentially negative activity when they have no redeeming devotion to truth. We should condemn Rawlins' errors and distortions, if such there be, but if we want to know reality, I do not see how we can object merely because he speaks with the charming candor of a small child.

I have received assurances from several of you that the Rawlins paper is fallacious. Others have said only that it may contain errors and that I should have waited. Despite my inquiries, I have learned of no serious factual discrepancies. Several of you relayed the criticism that Rawlins thought he was an Associate Editor of The Skeptical Inquirer, when, in fact, he was only a member of the Editorial Board. From others I have learned that the distinction was never clear. In any case, the matter is hardly worth mentioning. I have no reason to change what I believed when I wrote my letter of inquiry to you, namely, that Rawlins' letter reveals how CSICOP operates. Since then, unsolicited documents sent to me by concerned persons have privately confirmed what was proved by the published papers.

It is evident that CSICOP is interested in saleable advocacy and not in scientific truth per se. In our society, there is nothing wrong with journalism for personal aggrandizement. In the case of CSICOP, what is intellectually dishonest is not only, specifically, the articles previously cited, but more generally, CSICOP's pretence to scientific authority and the tacit endorsement of its advocacy by sponsors who have no control over CSICOP and who, for the most part, have not taken the time to inform themselves as to the nature of its management.

In Toynbean terms, our civilization has suffered "breakdown," in which anti-rationalism by the masses and intellectual dishonesty by the ruling elite are complementary symptoms. The people no longer trust scientists. Could it be that scientists are "part of the problem"?

Having said the foregoing in all seriousness, I must add in all honesty that, like Martin Gardner, I have found this to be an "incredibly hilarious" affair. My feeling is one of gratitude toward Rawlins and the others who have provided comic relief in a time of tragedy. Moreover, I am grateful to all the valiant protagonists (whether amusing or not) who have provided me a reason to become acquainted with the Fellows and Consultants of the Committee, among whom I have discovered some admirable persons whom I would be pleased to have as friends.

For the several who asked, my credentials may be found in American Men and Women of Science. You will find listed there my interest in parapsychology--which accounts for my contacts with present and past members of CSICOP's Executive Council. I welcomed the present controversy as a means to explore the nature of CSICOP in circumstances where parapsychology was not involved.

Several of you referred to my interest in parapsychology--in some cases evidently without knowledge of that interest. I am enclosing a paper [McConnell, 1978] showing that I have not hesitated to attack some of the leaders of that field when I thought they had lost sight of their scientific goal.

For skeptics who would like to know the best and worst about parapsychology, I suggest the three books listed below.

Sincerely yours,

* * * * *

While any final conclusions concerning the impact of the sTARBABY affair must await a count of disappearing sponsoring names from The Skeptical Inquirer, it would appear from the evidence presented here that only a few of the scientists and scholars involved have a deep concern for the honesty of CSICOP and its journal.

This may reflect the zeitgeist. One is reminded of the competent and respected scientists at Yale, Cornell, and Harvard who have recently allowed their reputations to be tarnished by lack of vigilance for fraud among those for whom they had assumed scientific responsibility (Broad, 1980; 1981; 1982; Kolata, 1981).

References

Abell, G. & Kurtz, P. Response to Rawlins. Skeptical Inquirer, 6(2) (Winter 1981-82), 67.

Broad, W.J. Imbroglio at Yale: II. A top job lost. Science, 210 (10 October 1980), 171-173.

Broad, W.J. Team research: Responsibility at the top. Science, 213 (3 July 1981), 114-115.

Broad, W.J. Harvard delays in reporting fraud. Science, 215 (29 January 1982), 478-482.

CSICOP Council. Statement in response to Rawlins. Skeptical Inquirer, 6(2) (Winter 1981-82), 66.

Curry, P. Research on the Mars effect. Zetetic Scholar, No. 9 (1982), 34-53. (a)

Curry, P. Replies to his commentators. Zetetic Scholar, No. 9 (1982), 78-83. (b)

De Marré, L. Comment. Zetetic Scholar, No. 9 (1982), 71-72.

Dommanget, J. Comment. Zetetic Scholar, No. 9 (1982), 73-74.

Eysenck, H.J. Comment. Zetetic Scholar, No. 9 (1982), 61-63.

Gauquelin, M. Comment. Zetetic Scholar, No. 9 (1982), 54-61, 75-77.

Good, I.J. Is the Mars effect an artifact? Zetetic Scholar, No. 9 (1982), 65-69.

Hoebens, P.H. Comment. Zetetic Scholar, No. 9 (1982), 70-71.

Kolata, G.B. Re-evaluation of cancer data eagerly awaited. Science, 214 (16 October 1981), 316-318.

Kreps, H. Comment. Zetetic Scholar, No. 9 (1982), 63-64.

McConnell, R.A. On the distinction between science and nonscience in a pretheoretical field. (Privately printed, 1978. Single copies upon request.)

McConnell, R.A. (Editor and publisher). Encounters with Parapsychology (1981).

McConnell, R.A. (Editor and publisher). Parapsychology and Self-Deception in Science. (in press).

McConnell, R.A. (Author and publisher). An Introduction to Parapsychology in the Context of Science. (in press).

Rawlins, D. sTARBABY, Fate 34(10) (October 1981), 67-98. (a)

Rawlins, D. Remus Extremus. Skeptical Inquirer, 6(2) (Winter 1981-82), 58-63. (b)

Truzzi, M. Introduction. Zetetic Scholar, No. 9 (1982), 33.

TABLE

The Sponsors of CSICOP and Their Reactions to sTARBABY

	Total	Writers	Psychol-ogists	Philos-ophers	Astron-omers	Miscel-laneous
Listed in *Skeptical Inquirer*	89	18	13	11	7	40
Were sent letter of inquiry	75	15	12	10	7	31
Responded	39	9	6	4	3	17
Criticised						
sTARBABY	10	2	1	2	1	4
Rawlins otherwise	7	3	3	0	0	1
CSICOP Council	3	1	0	0	0	2
McConnell	14	4	2	1	1	6
No criticism	10	2	1	1	1	5
Judgmental action						
CSICOP condemned	1	0	0	0	0	1
Will actively investigate	6	1	2	0	0	3
Will reserve judgment	20	3	2	3	3	9
Satisfied with CSICOP	12	5	2	1	0	4
Substantive action						
Disaffiliating	1	0	0	0	0	1
"Appropriate"	7	1	2	1	1	2
Will not resign	17	6	2	0	2	7

THE TRUE DISBELIEVERS:
Mars Effect Drives Skeptics to Irrationality
(Part I)

RICHARD KAMMANN

(What follows is a two-part analysis of the Mars Effect Controversy surrounding the tests conducted by prominent Fellows of the Committee for the Scientific Investigation of Claims of the Paranormal. An advance copy of this article was sent to the Executive Council of that Committee, and a letter announcing the article and offering a free copy was sent to all of the Fellows and Scientific Consultants of the organization. Thus, all persons associated with the Committee have been urged to comment on this paper or to persuade others representing the Committee to reply. We hope that this will elicit responses to Dr. Kammann's critique (we have thus far received no responses to Patrick Curry's critique in ZS#9) that might appear in ZS#11. But if replies appear elsewhere (e.g., in THE SKEPTICAL INQUIRER), we will so inform ZS readers in ZS#11. Of course, as with all ZS materials, readers are welcome to enter these dialogues.

Perhaps it should be mentioned that the first part of this article by Dr. Kammann was originally submitted for possible publication to THE HUMANIST, PSYCHOLOGY TODAY, and THE SKEPTICAL INQUIRER, all of whom declined to publish it. I mention this not to impugn any of these publications or their policies -- there are many good reasons why a publication may reject an excellent article. Rather, I mention it to point out that this article was not solicited by ZS and was not originally written for ZS. -- M. Truzzi)

In recent years psychologists have become increasingly fascinated by the versatility of the true believer in finding reasons to go on believing in spite of clear evidence to the contrary. These belief-preserving maneuvers are most readily seen in everyday aberrations like racial prejudice, superstition, religion, and the slogans of politicians, but it is now recognized that they also occur in the very halls of science from which "truth" is supposed to be broadcast with dispassionate, value-free objectivity. Kuhn has written about the "paradigms" that organize scientific thought in each field of study at each point in time, while psychologist Michael Mahoney (The Scientist as Subject) has documented a number of signs of fallible logic and irrational conviction among scientists, especially social scientists.

This article is a case study in which a small group of antipseudoscience skeptics fall back on a remarkable line of illogic and defensiveness when confronted with intractible data suggesting that the position of Mars in the sky when one is born has an effect on the likelihood of becoming a sports champion. In the July 1982 issue of Psychology Today, UCLA astronomer George Abell reviews the data on the Mars effect, a quasi-astrological claim by French scientists Michel and Francoise Gauquelin and presents his reasons for disbelieving the claim. Again in the August 30, 1982, Newsweek, Abell says, "The Gauquelins have no way of proving they did not cheat." Of course, such a statement can be made against any scientific claim. Such a slur may only be raised properly where there are positive reasons for suspicion as there is in this case --not by the Gauquelins, but by Abell and his collaborators!

Abell's ultimate line of attack against the Mars correlation is his argument that the Gauquelins' data may not be trustworthy. To support this case, Abell points to data produced by the Gauquelins in response to a control group challenge issued by Abell's collaborator Marvin Zelen in The Humanist (Jan./Feb. 1976). The essence of the challenge was for Michel Gauquelin to produce the Mars data on ordinary people for comparison with the Mars effect on sports champions. Incidental to this test, the Gauquelins extracted a subsample of 303 of their original 2088 European champions. According to Abell, these champion data have two anomalies in them. First, there were "disparities" among the three regional samples from Paris, the rest of France, and Belgium. Second, the fact that the size of the Mars effect in this 303 subsample happens to match almost perfectly the size of the effect in the original sample of 2088 champions is seen by Abell as "improbably good," meaning that it was too good to have plausibly occurred by chance alone.

It is incredible that Abell should have produced these erroneous arguments for the 4th time in spite of repeated warnings by many critics over four years that they are incorrect. Nevertheless, having entered them (once again) into the public record, he says "Although we suspect that the Gauquelins' sample was not random, we can imagine ways that bias

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Cast of Characters

I CSICOP

Chairman: Paul Kurtz
9 members of Council (formerly including Dennis Rawlins, replaced in 1980 by George Abell
50 Fellows (e.g. B.F. Skinner, Isaac Asimov, Carl Sagan, including Marvin Zelen).
Journal: The Skeptical Inquirer.

II The Trio (who committed the errors)

Paul Kurtz, Professor of Philosophy, University of Buffalo (N.Y.)
George Abell, Professor of Astronomy, UCLA
Marvin Zelen, Professor of Biostatistics(!), Harvard University (formerly at University of Buffalo with Kurtz).

III The Man who Complained

Dennis Rawlins, Astronomer, and free-lance science writer in San Diego, California

IV The Victims of the Errors

Michel and Francoise Gauquelin, chronobiologists in Paris, France, who discovered the "Mars effect" among other correlations between planets and success in professions. (Data examined by H.J. Eysenck.)

Brief Chronology

1976 M. Gauquelin reports planetary effects in The Humanist magazine.
M. Zelen issues his control group challenge in The Humanist.

1977 CSICOP is formed with Kurtz as Chairman.
Gauquelins report data on Zelen challenge in The Humanist.
Zelen, Kurtz, Abell reply, committing 6 statistical errors.

1978 Dennis Rawlins raises a fuss in CSICOP about Z-K-A errors.

1979 Z-K-A repeat some errors in article in The Skeptical Inquirer.

1980 CSICOP drops Dennis Rawlins from Council and as Fellow.

1981 Rawlins publishes "sTarbaby" in Fate magazine accusing CSICOP of a "cover-up" of the errors.

1982 CSICOP launches campaign against Rawlins' character and truthfulness could have entered without intentional cheating." He then provides an analogy that makes the Gauquelins look grossly incompetent if they are not to be accused of cheating. Not satisfied with the damage done by this innuendo, Abell adds the thought, "I find it hard to believe that they [the Gauquelins] would intentionally falsify the data, but of course personal feelings are irrelevant in the scientific evaluation of a claim."

Following Abell's dictum, I here put aside my personal loyalties to him, to the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) which he represents as a member of its Council, and to those other Councilors of CSICOP whose talents and purposes I have always admired. In Part 1 of this two-part paper I shall show that Abell, along with Professor Paul Kurtz, the Chairman of CSICOP and former editor of The Humanist, and Professor Marvin Zelen, statistician at Harvard University and Fellow of CSICOP, have persisted in offering to the public a set of demonstrably false statistical arguments against the Mars effect in spite of four years of continuous and steadily mounting criticism of their illogic.

Also in this Part, I report a hypothesis that I worked out to explain away some of these errors, and even their unsinkability, as non-malicious acts of blind prejudice, and will explain why I was unable to extend that scenario to cover all the errors committed. Many of these errors were initially exposed by Dennis Rawlins in a smashing attack on CSICOP in the October 1981 issue of Fate magazine. Readers who are not aware of this whole controversy, and the vehement behind-the-scenes controversy it has created for CSICOP, can refer to the Chart for the cast of characters and the chronology of main events.

The Zelen Challenge

Readers who have followed the Mars feud already know that Michel and Francoise Gauquelin found that European sports champions were born in Mars sectors 1 and 4 at a rate of 22% instead of the 17% expected by chance. However, the definition of "chance" requires making assumptions, for example, about the times of day that people are born which are not

truly random. While one can try to take all such factors into account to calculate a chance baseline, Marvin Zelen proposed a shortcut in 1976 by looking at the Mars sectors of ordinary people to see how often they are born in sectors 1 and 4. As a method of finding the right baseline, the Zelen challenge is a definitive test.

The first error by the skeptics occurs in the funny way Zelen designed this challenge. Quite logically he said that the control group should be born at the same times and places as the champions. He suggested using 100 or 200 of the original champions to locate the matched control group. Practically nobody noticed in the fine print of Zelen's statistical design that he planned to see if the Mars effect in these 100 or 200 champions was above the baseline effect in their birth mates.

The catch-22 is the small sample size Zelen suggested. If there really is a Mars effect of 22% above 17%, a sample of 100 champions is far too small to detect the effect reliably. The Gauquelins not only spotted the error, but presented Zelen with a mathematical proof of it. As far as I know, Zelen has never admitted the point.

Taking up a corrected version of the Zelen challenge, the Gauquelins deleted a part of their champions group because it would be too difficult to get their control data, and used as many of the remaining champions as they could. Since the local French birth records offices would not always supply the data, the champions group dwindled to 303, but through them, a large control group of 16,756 non-athletes was located. When Zelen analyzed the data, the control group baseline came in almost perfectly, at 16.4%, and the 303 champions incidentally came in with a Mars effect of 21.8%, both as Michel Gauquelin had predicted.

Even the observed Mars effect in the 303 champions subsample was significant at the .02 level. This says that if there really is no Mars effect, and we ran the experiment 100 times, always using 303 champions, we should only observe a value as high as 21.8% in a mere 2 experiments. By a scientific rule of thumb, the investigator is allowed to claim a real effect whenever the "chance probability" of the observed result falls below 5 in 100 experiments. (This arbitrary .05 threshold will appear over and over again here as the "litmus test of truth.")

The Skeptics' Reply to the Gauquelins

Let us be clear about the Gauquelins unquestionable victory for the Mars effect--among 16,756 ordinary people, Mars was in sectors 1 and 4 for 16.4% of their births, just as expected, while for 2,088 European sports champions it was in these sectors for 21.6% of their births, a difference that is totally outside the realm of mere chance. The control group result also eliminates a number of statistical doubts raised by the skeptical Belgian Para Committee in their attempt to dis-own their own positive verification of the Mars claim.

Nevertheless, this strong evidence in favor of planetary influences de-materialised after Zelen, Kurtz and Abell performed their statistical numerology on the data in the November/December 1977 issue of The Humanist alongside the "good news" report from the Gauquelins. To accomplish this, the trio completely ignored the original sample of 2,088 champions and proceeded to bludgeon the subsample of 303 champions that had merely been used to locate the matched control group of non-athletes.

Their first method was sample-splitting. They divided the 303 champions data into three geographical regions, or alternatively split them into two parts according to sources of the data. The Mars-positive percentages were: 32% (Paris), 21% (France-minus-Paris), 15% (Belgium) and again, 21% (Gauquelin data), and 22% (Para Committee data). Out of these five small sub-sub-samples, only the 32% for Paris was statistically "significant," that is, reliably above the baseline level of 16.4%. This was good news for the skeptical trio since a private line from Mars to Paris should seem absurd to the most starry-eyed believer.

They failed to mention, however, that non-significance in the sub-sub-groups should occur automatically by the reduction in the size of the samples. (Small samples have large fluctuation zones or standard errors; to achieve the .05 level, the observed Mars effect needs to be only 20.3% among five hundred people, but must climb to 26.7% among fifty people.) The absurdity of all this sample-splitting was clearly demonstrated six months prior to the trio's article by Michel Gauquelin who pointedly showed Zelen how to break the Paris data into seven smaller samples to get rid of the Mars effect altogether!

A here-and-there Mars effect, however, led the skeptics to hint darkly about possible flaws in the data collection. With a Mars effect occurring only in Paris, they referred to "possible irregularities," "striking differences," or now to "disparities" in the subsets of data. In his first private analysis of the data, Zelen concluded his memo with the bald statement, "There is not enough information to verify how the sample was drawn," in spite of the fact that Michel Gauquelin had long before sent him three detailed descriptions of the sampling procedures which were entirely straightforward and barred Gauquelin himself from influencing the data.

The claim of disparities among the sub-sub-groups is simply incorrect statistics. The correct method for such a claim is not to compare each group with the 16.4% baseline, but to compare them directly with each other, as any statistics professor can tell you. Although the trio never did this, Eric Tarkington and Dennis Rawlins both did it independently and reported NO significant differences among the groups. Therefore there is no basis whatsoever to say that there is a bigger Mars effect in one place (not even Paris) than there is in another place (not even Belgium). The different sub-sub-group percentages could all result normally from a constant Mars effect of 22% in all categories. There are no anomalies. There are no disparities. There is nothing special about Paris. There is no evidence that the Gauquelins' data are in any way unusual, except for the Mars effect itself.

When more criticisms mounted after the trio published the 1977 paper, they persisted doggedly. They said it is only proper to explore the effect in "recognizable subsets" of the data (such as that well-known place "France-minus-Paris"???). After all, they said, the Gauquelins themselves had looked at sub-sub-groups. This was technically true, but with a major difference. The Gauquelins' breakdowns were scientifically more rational, such as the one between larger and smaller localities. More importantly, the Gauquelins did not try to run statistical tests on these small samples. Their purpose was only to show that there was at least a sign of the Mars effect in the different categories.

The next error, removal of the females, requires a background fact first. In Zelen's first and private analysis of the 303 champions, Paris was highly significant, France-minus-Paris was "marginally significant" at the .06 level, and Belgium was not significant. Coming into print, the trio noted that there were too few female champions for a proper analysis and dropped all 9 of them from the calculations, along with the control-group women. A small side-effect was that the significance level in France-minus-Paris happened to slip from .06 to .09, now called "not significant," while the whole sample slipped from .02 to .04. (The number 303 should actually read 294 in the preceding paragraphs.)

Of course, the .04 results was also doomed. In the bizarrest maneuver of all, the trio argued that the overall Mars effect now depended merely on the results of a single champion since, if only one more champion had been born outside a key sector, all results would fade into non-significance! Even Randi the magician could hardly match such a comprehensive vanishing trick!

This reasoning violates the basic principles of statistical analysis. A significance test only makes sense when applied to the actual data, not the data upped or downed a few notches to suit the researcher's personal prejudices. The main result did not depend on a single champion, but on all 63 of the key-sector athletes needed to beat the chance prediction of about 51. The trio's illogic invites any number of games with the data. The Gauquelins could ask, but what if there was one more champion born inside a key sector, or for that matter, ten more? And does the result only depend on one champion considering that three out of the nine deleted females were also Mars-positive?

Meanwhile, everybody but Dennis Rawlins had forgotten about the original hugh sample of 2,088 champions. In a letter to Paul Kurtz in 1978, Rawlins roughly estimated the significance level in that group, that is, the odds against there being no real effect behind the 22% result, at 1 in 10 million. It was fully four years later in the Winter 1981-82 issue of The Skeptical Inquirer before Abell and Kurtz (sans Zelen) acknowledged that the data from the full 2,088-champions sample "would seem to be statistically significant" and went on to say, "It was not a conscious omission. Indeed, the point was made very clearly in the Gauquelins' companion paper published in the same issue of The Humanist (Nov/Dec 1977)." That sounds better--until you read the Gauquelins' paper and discover that the alleged point isn't there!

The striking feature of all these fallacies was not just their unmooring from the anchors of logic but their unsinkability in four years of competent statistical bombardment by Michel Gauquelin, Elizabeth Scott, Dennis Rawlins and Ray Hyman. Indeed, the trio's reply to Rawlins' "sTarbaby" bomb, where the errors were laid out in blunt English, was to refurbish and re-circulate the whole lot in a paper sent privately by Abell and Kurtz to the Fellows of CSICOP, presumably to ward off a possible stampede of resignations. Remarkably enough--it even worked.

A Scenario for Innocence?

After seven months of investigation which included extensive correspondence with members of CSICOP's Executive Council, I developed a theory which classified the errors -- even their indestructability in the face of criticism--out of the arena of deliberate distortion, and into the theatre of blind prejudice. After receiving a thought-provoking letter from CSICOP Councilor Ray Hyman, I re-studied the documents from the point of view of innocent errors, in which case Rawlins' suggestion of cynical manipulation might go away. This interpretation is, I still believe, correct--as far as it goes.

With the extra help of unpublished background papers and letters, here is what I imagine took place. When the control group data unexpectedly came in at 17%, an exasperated Marvin Zelen immediately turned a skeptical eye back on the 303 subsample of champions and quickly made two erroneous discoveries. Analyzing the champions data a page at a time (which he did) the results bounced around--in the Belgium pages there was no Mars effect at all, while in the Paris pages it was highly significant. (A third set of pages created the arbitrary region "France-minus-Paris.") Without thinking twice about what he was doing, he incorrectly ran significance tests on these subsets against the baseline to confirm his suspicion of "disparities."

Another thing he happened across at this stage was a "striking similarity" between the Mars effect of 21.8% in the sample of 303 champions, and the 21.6% level in the parent group of 2,088 champions. These were obviously too close to be an accident, he thought, and with the versatility of a professional statistician he demonstrated a significant non-difference(!) at the .049 level. This appears in his original memorandum, but was not published in the 1977 paper. It only surfaced in the private 1981 memorandum to CSICOP's Fellows, and of course, as Abell's "improbably good" argument in the July 1982 issue of Psychology Today.

In spite of its professional flourish, this analysis was faulty because Zelen did not take into account the different pairs of data points in the whole set of results. There were probably several ways in which an unusually large or small difference could occur among the Mars sector percentages by chance, any one of which would seem suspicious when looked at by itself. Only if such possible pairs were listed in advance could Zelen make a correct test, in which case his "finding" would have disappeared.

The illusion that the Mars effect occurs only in Paris and that two sub-samples had a "striking similarity" convinced Zelen that the Gauquelins had somehow produced a biased sample. He was immediately so suspicious that he closed his memorandum with his unjustified complaint on the lack of sampling information from the Gauquelins.

At this point a process of subjective validation took over which I have outlined in The Psychology of the Psychic (Marks and Kammann, 1980) to account for the persistence of false beliefs in the face of contrary evidence. The model says that once a belief or expectation is found, especially one that resolves uncomfortable uncertainty, it biases the observor to notice new information that confirms the belief, and to discount evidence to the contrary. This self-perpetuating mechanism consolidates the original error and builds up an overconfidence in which the arguments of opponents are seen as too fragmentary to undo the adopted belief.

As soon as Zelen's suspicion about a biased sample was shared among the trio, it was quickly noted that the Belgium data, where there was no Mars result, was the only subset not collected by the Gauquelins; furthermore, Paul Kurtz then remembered some anomalies that he had glossed over as unimportant when he had previously spot-checked the Gauquelins' records in France. With all of such factors pointing so "clearly" to untrustworthy data, the answer was apparently in hand, even if not provable or fully publishable.

Partly because the minor pieces of this argument were not published, including the "striking similarity" effect, critics like Dennis Rawlins, Elizabeth Scott and Ray Hyman were seen as nit picking and lacking of the big picture. (They were also not offering any good alternatives to demolish the so-called Mars effect.) For example, when it was later shown that the differences among the three regions were not significant, the trio would not be impressed--such a test cannot prove that there are NO differences either, and the (imagined) negative result from Belgium was felt to be too "revealing" to be given up.

But It Doesn't Cover the Territory

Although the evidence seems strong to me that the preceding scenario correctly describes how the trio jumped into the stew, it does not cover several other errors. For these I had to construct a separate mini-scenario for each case. Although I was not at all confident about these, it seemed worthwhile to pursue an emerging innocence theory as far as it would go.

In this direction, the trio's removal of the females might have been, as they eventually claimed, the simplest way to balance the sexes between the control group and the 303 champions group. (This also required them to drop the female half of the control group, but it was already so large that this had no effect.)

The nonsensical one-single-champion argument was possibly a didactic device to explain to nonstatistical readers how really weak is the evidence for a Mars effect conveyed by a .04 significance level.

The reason that the trio overlooked the massive significance in the full 2,088 champions sample was the result of Zelen's pre-occupation with a perfect statistical design--in a technical sense, only the 303 champions subsample was precisely matched with the non-champions control group.

Why then did Abell and Kurtz later claim that this "2,088 error" had been separately covered by the Gauquelins when it had not been?

Perhaps they were under so much pressure from "sTarbaby" they rushed into print with a statement based on memory rather than re-reading. But this seems odd because that argument is the centerpiece of their entire one-page public defense against "sTarbaby."

How viable, then, is a comprehensive innocence theory? While each mini-scenario is plausible enough in isolation, their summation with the main scenario is hardly reassuring and to be accepted requires a conclusion of pervasive ineptitude. Unfortunately, the trio have never bothered to comment one way or the other. But perhaps the only important scientific point is that the errors exist and are still being repeated.

What About the Mars Effect?

The bottom line is that an apology is owed the Gauquelins for the mis-treatment of their data, and the aspersions cast on their authenticity. I don't wish to convey that I'm a believer, because I also have skeptical reservations about the Mars effect. What makes this claim suspect is the scientific perversity of the proposition that the location of Mars in the sky at the time a person is born has some effect on that person's athletic performance 30 or 40 years later. It has been repeatedly noted that the natural forces that emanate from Mars, such things as gravitational pull, electromagnetic radiation, and so on, are infinitesimally tiny and must be effectively zero when compared with such familiar earth-bound objects as hills, buildings and even furniture.

The sector locations of Mars do not even reflect its distance from earth, nor are the key sectors simply above and below the horizon in some sensible way that could produce an earth-shadow effect. Even worse, the Gauquelins believe the effect does not appear in merely good athletes, but only shows up in top-top champions. There is no precedent in biology or psychology for a performance factor, such as motivation or temperament, to become suddenly operative when skill reaches an extraordinary level.

Nevertheless, the Mars effect has been once replicated by the skeptics of the Belgian Para Committee (whose gyrations in disclaiming it would make another interesting case study) and once not replicated on U.S. champions by Rawlins, Kurtz, Abell and Zelen. It has, therefore, a residual prima facie case as a valid scientific anomaly. But let us be clear--nobody has produced more massive evidence AGAINST the claims of traditional astrology than Michel Gauguelin, himself (e.g., in The Skeptical Inquirer, Spring 1982, pp. 57-65.

In the long-run, the skeptics' worthy battle against superstition and pseudoscience must rely on trustworthy evidence, rational self-correcting debate, and their ability to supply normal explanations for paranormal claims. It is not the belief in astrology, ESP, UFOs or mythical monsters that is the real problem of our times, but the possibility that rationality itself will be submerged under social dis-locations arising from the economic and technological pressures that interfere with our collective will to live out our human potential in peace and without poverty. Thus, I believe that the method of debate is more important to the advancement of rationality than the specific debunking of popular fantasies. I object to Abell's interpretation of the Mars effect, not because we differ widely in the conclusion, but rather in the mode of argument.

None of which is to retract my general agreement with the goals of CSICOP or my admiration for the many enlightening articles that regularly appear in its journal, The Skeptical Inquirer. Knowing as I do how unpopular the skeptic can be in a society of believers, I appreciate the skeptics' need for social support by like-minded thinkers, and the urge to produce a definitive rebuttal to every paranormal claim. Over simplifications and errors are inevitable not only in debunking, but in all exercises in rationality and science, and no harm is done as long as they are amenable to open debate and correction. Unfortunately, CSICOP, as the self-avowed champion of rationality, refuses to admit publicly that the errors occurred and refuses to take any action to stop their endless repetition by Abell in his public attack on the Gauquelins' reputations.

FOOTNOTE

1. The sources for this paper are available in the unpublished manuscript "Statistical Numerology in the Skeptics' Response to the Mars Effect" by R. Kammann, available from CSICOP, 1203 Kensington Ave., Buffalo, N.Y. 14215. An excellent review of the larger controversy which also considers the disputes arising from the later Mars test on U.S. champions is found in Patrick Curry's "Research on the Mars Effect," Zetetic Scholar, 1982, #9, 34-53, along with 9 additional commentaries on pages 54-83. The author's subjective validation concept is presented in chapters 11-13 of The Psychology of the Psychic by D. Marks and R. Kammann (Buffalo: Prometheus Books, 1980). Thanks are due to Philip Klass, Paul Kurtz, George Abell and Ray Hyman for their patience in responding to my correspondence and to Dennis Rawlins, Marcello Truzzi, Piet Hein Hoebens, and Michel Gauquelin for supplying background documents.

(Part II)

Recap of Part 1: *Faced with unfaultable evidence of a connection between the position of planet Mars at birth and success in sports, skeptical Professors Paul Kurtz, George Abell and Marvin Zelen repeatedly offered fallacious statistics to deny astrology's only ray of hope. Focussing only on a small section of the Mars data, deleting the favorable results for females, dividing the sub-sample into tiny bits and applying the wrong statistical tests, the trio still could not get rid of the Mars effect. They ultimately argued that it was based on faulty data, due either to incompetence or cheating by Michel Gauquelin of France, who produced the original finding.*

"Our society has opted for a complete free-for-all of conflicting theories. But if it is this chaotic, who will ensure that there is law and order? Who will guard the truth? The answer is: CSICOP will!" With these words, Douglas R. Hofstadter began his glowing account of the Committee for the Scientific Investigation of Claims of the Paranormal in the February 1982 issue of Scientific American. In Hofstadter's account, CSICOP is a small and heroic band of nonsense fighters providing a steady buoy of rationality in a vast sea of public superstition.

It is remarkable that Hofstadter's piece appeared four months after Dennis Rawlins published "sTarbaby" in the October issue of Fate. In Rawlins' detailed account, the Council of CSICOP had covered up a scandalous demonstration of irrationality by three of their most prominent members, including the Chairman, Paul Kurtz. If Hofstadter did not know about "sTarbaby" then--proof of the pudding--CSICOP hadn't told him, but it is more likely he accepted the CSICOP party line that Rawlins was just a raving malcontent.

On the surface, this is plausible. The trouble with "sTarbaby" on first reading is that the case is too strong, and the cover-up too deep to be entirely believable. Like the other Fellows of CSICOP, I couldn't accept that Dennis Rawlins was the single honest and correct person on a nine-man Council consisting of men of such stature and reputation as Martin Gardner (whose mathematical games column in Scientific American had just been taken over by Hofstatdter), Professor Ray Hyman, the Amazing Randi and Kendrick Frazier. In fact, Rawlins seemed to grasp at straws to include these bystanders in the conspiracy plot. It seemed more likely that Rawlins had let his anger get out of control and was seeing connivance in the most innocent remarks. This attitude might then explain why his analysis of the Mars effect had been ignored, and why he was eventually voted off the Council and out of CSICOP. Undoubtedly Rawlins was making a mountain out of a molehill.

After seven months of research, I have come to the opposite conclusion. CSICOP has no good defense of the trio's Mars fiasco and has progressively trapped itself, degree by irreversible degree, into an anti-Rawlins propanganda campaign, into suppression of his evidence, and into stonewalling against other critics. In short, progressively stuck on the trio's tarbaby.

It is now vital to understand where CSICOP lost its bearings and how far off course it has drifted. I shall briefly review the essential events leading up to "sTarbaby" as I have confirmed them since, after which I present my personal experience of Council's modus operandi in the months following.

How Did the Trio Go Wrong?

Michel Gauquelin had already run into one group of irrational skeptics in the Belgian Para Committee who, upon unexpectedly confirming the Mars effect, dismissed their results. They fastened on the fact that babies are not born equally often during the 24 hours of the day and supposed that this could produce a spurious Mars effect. In effect, they suggested that everybody, not just sports champions, has a Mars effect.

Dennis Rawlins, then on the Council of CSICOP and its astrology subcommittee, checked this argument out mathematically and found it to be irrelevant. Nevertheless, Zelen, Kurtz and Abell grabbed the Belgian theory and publicly challenged Gauquelin to produce a control group of nonchampions. Michel and Francoise Gauquelin promptly accepted this "definitive test" as the trio called it and, as Rawlins predicted, won hands down. There was no Mars effect for ordinary people.

George Abell sensibly wrote Paul Kurtz saying the Gauquelins had won that round, and he suggested getting on with the new test on American athletes. Rawlins used this "smoking gun" letter as proof that the trio knew the true situation right from the start, but the case is not strong. Abell specifically asks in the letter what Zelen saw in the data. Meanwhile, as I described in Part 1, Zelen fancied he found two anomalies in the data that suggested a biased sample. In my "subjective validation" scenario, Zelen's erroneous statistics became the starting point for the trio's private belief that the Gauquelins had probably cheated. By the time the paper got to print, Zelen's skeptical approach had replaced Abell's; although the trio did not openly accuse the Gauquelins of fraud, they smothered the victory under a blanket of bogus side issues, partly achieved by deleting the favorable Mars results for female champions.

Against an "innocent goofs" theory, the trio was warned before publishing that their statistics were wrong, once by Michel Gauquelin and once by Elizabeth Scott, Professor of Statistics at Stanford University. (Rawlins was not consulted.) Even worse, after the paper came out, neither Scott nor Gauquelin could get space in The Humanist for a reply.

How Did CSICOP Go Wrong?

After Rawlins read the trio's 1977 paper, he set out with documented good will to educate Chairman Kurtz in statistical reasoning. This seemed to go well during 1978, especially when Kurtz called upon Rawlins to analyze the data for the American sports champions. Rawlins had every reason to believe that the Zelen-Kurtz-Abell errors would fade into history.

He was in for a shock. After Rawlins completed all the computer runs on the U.S. data (no Mars effect), Kurtz announced the trio would present Rawlins' results in a major CSICOP press conference in December 1978. When Kurtz refused to budge on this, Rawlins appealed to the other Councilors for help. It was at this crucial point that Council took its first and fatal wrong turn and embarked on a course they could not subsequently reverse. They ignored Rawlins' complaints. To avoid an embarrassing public split, Rawlins was promised a debate with Zelen and Abell in front of Council. After Rawlins did not blow the whistle with the press, this debate evaporated. When he finally got the floor in Council meeting, he met a wall of resistance.

We can only guess the thoughts of the Councilors. If the idea had been planted that Rawlins was jealous over getting a back seat at the press conference, his anger was explained, but only if Councilors missed the merits of his case. Alternatively, perhaps Paul Kurtz was so indispensable as the group's leader that a reprimand was unthinkable. ("sTarbaby" focuses on Council's obsession with its public relations image.)

After all this flak, it is only reasonable that Council would at least stop any repetition of the trio's past errors, but just the opposite occurred. The Zelen-Kurtz-Abell analysis was re-published a year later in CSICOP's own journal, The Skeptical Inquirer, overriding severe criticisms by one referee, statistician and Councilor Ray Hyman, which confirmed Rawlins. Rawlins was to cover only the technical aspects of the U.S. test, and claimed in "sTarbaby" that editor Ken Frazier censored his full protest of the trio's errors. Meanwhile, he sent a memorandum out to most of the Fellows but to no useful effect.

A year later, Rawlins was voted off the Council and soon after was quietly dropped from the list of Fellows. "sTarbaby" was his reply.

Council's Response to "sTarbaby"

After reading the Rawlins expose in Fate magazine, I was only sure of one thing--if any part of his story were true, I could count on Gardner, Hyman, Randi and Frazier to set the record straight. It was a very long time before I gave up that belief.

CSICOP's first reply was to circulate some photocopied old letters to show (ad hominem) that Rawlins was a habitual troublemaker. Two months later, CSICOP mailed out two privately authored white papers, without taking an official stance.

In "The Status of the Mars Effect" Abell, Kurtz and Zelen simply re-hashed all the statistical errors that Rawlins (Gauquelin, Scott, Hyman, Tarkington) had protested. I did not see this, however, until I had spent hours analyzing four years of published statistics--the errors were even worse than Rawlins had stated, but most Fellows would never learn this.

"Crybaby" was written by Councilor Philip Klass. Although it offered to refute the cover-up charge, it ignored practically every specific point that Rawlins had made. Instead it offered a blatant ad hominem attack on Rawlins' motives and personality, bolstered with rhetorical ploys--including crude mis-quotation.

Believing that a full understanding would still get this fiasco straightened out, I sent in a 28-page report called "Personal Assessment of the Mars Controversy." I came to three conclusions: (a) the scientific errors were gross, (b) Paul Kurtz was not guilty of a cover-up on grounds of lack of statistical understanding, (c) CSICOP was guilty of a cover-up by not taking Rawlins seriously, while "Crybaby" was a disgrace.

This report went to Council in December 1981, underlined by my resignation as a Fellow, and my request that it be circulated to all the Fellows. This was not done. Two months later it was casually described as a "lengthy letter" from me along with other routine news in a general CSICOP bulletin. The ho-hum context was so effective I yawned myself.

As my report was going in, the next issue of The Skeptical Inquirer (Winter 1981-82) was coming out with one good move and two bad ones. The good move was to give Rawlins space for a completely uncensored final article, which Rawlins unfortunately wasted on an unreadable script.

The first bad move was a boxed one-page Statement signed by the nine members of Council, with George Abell now in Rawlins' vacant seat. It asserted starkly that there was nothing to hide and no cover-up. Without giving any useful evidence, it declared that Rawlins' entire jam-packed article in the same issue "contains many demonstrably false and defamatory claims." (Name them!) It referred to all of Rawlins' "assertions and innuendoes" as being based on "half-truth and distortion." (The scientific errors alone disprove this claim.) Worst of all, Council offered for sale the hopeless "Status" and "Crybaby" papers, now officially endorsed by CSICOP. In a flurry of group-think, the whole Council lunged at the tarbaby.

On the facing page, George Abell and Paul Kurtz (sans Zelen) acknowledged only one of the major science errors (while repeating another one). They now claimed their failure to analyze the full sample of 2088 champions, rather than the small sub-sample of 303 champions, was merely an oversight and blandly understated that the total Mars effect "would seem to be statistically significant." With unconscionable bravado they falsely declared that the Gauquelins had already covered this point in their 1977 companion paper.

I still doggedly believed that the Trustworthy Four would come forth as soon as "Personal Assessment" had been digested, but the response over several months was dead silence. I racked my brain for an explanation--was my analysis wrong, had I spoken too harshly, did they still not understand?

Meanwhile, Paul Kurtz and Councilor Philip Klass each sent me a long letter which I naively took to be personal correspondence. (Kurtz' letter was marked CONFIDENTIAL.) Although I eventually disputed both letters, especially the nonsense by Klass, I only learned much later that both letters had been distributed to counter "Personal Assessment" for the few members who had asked to see it (my "lengthy letter"). Kurtz also refused my request to send his letter to Rawlins. The control on information was pervasive.

The Klass letter started a long and exasperating exchange in which he talked about everything but the statistical errors and the real cover-up. He kept me busy for a while answering irrelevant questions, while periodically attacking my objectivity, intelligence or integrity. From time to time, he threatened to expose MY cover-up of scientific evidence he imagined he had uncovered. After he regularly ignored all my serious answers and questions, I nicknamed him T.B. Diago -- the best defense is a good offense. He eventually fell back on the traditional Council stance -- he didn't understand statistics.

Around March, Zetetic Scholar featured a review of Mars and CSICOP with a lead article by Patrick Curry who not only agreed with Rawlins and me about the Zelen test fiasco, but presented a good case for more bungling in the U.S. champions test. But Council had already adopted the line that ZS editor Marcello Truzzi was on a "vendetta" kick. *Ad hominem* be thy name.

Fleeting Rays of Sunshine

Still getting no response from the 4 stony faces on CSICOP's Mt. Rushmore, I submitted a completely new paper fully documenting all the scientific errors with sources and omitting all charges of a cover-up by Council. Called "Statistical Numerology in the Skeptics' Response to the Mars Effect," and strictly limited to a small circle of addressees, this paper finally got some results.

George Abell produced 71 pages of explanations and apologies, accepting "Numerology" with two minor disclaimers (both wrong). Ray Hyman concurred on the errors but saw them as ordinary slip-ups in the process of science. Many scientists, he argued, try to publish nonsense but are blocked by a strong system of peer reviews and editorial control. Of course, there were no such controls for The Humanist or The Skeptical Inquirer, especially since Paul Kurtz had ultimate control on both. Ken Frazier agreed that a shorter and softer version of "Numerology" could be published in The Skeptical Inquirer but emphasized that nobody was interested in this dull old topic.

My faith in the goodness of CSICOP now flowering, I set to work on a readable third version of the paper. With Hyman's case for ordinary human errors humming in my head, I hit upon the subjective validation scenario for some of the errors (see Part 1) and even convinced myself that the whole cover-up was merely selective perception by Rawlins. The happy ending was in full sight.

The glow didn't last long. Frazier cabled that the editorial board was split and to shorten it severely. Meanwhile, my innocence theary was cracking under the strain to cover all the errors, and I sensed that no version I could write would be acceptable. Strong letters from Martin Gardner and Philip Klass now defined the situation as "resolved" by the Abell and Hyman letters. I was now exhausted and feeling the pressure to pronounce the benediction.

A reply to Hyman from parapsychologist R.A. McConnell said, "Nonsense. What we are talking about is elementary statistics--Abell and Zelen's specialty--and a third professor who is enhancing his status by lending his name in a field in which he presumably has no competence whatsoever.

Of course, I'll buy your claim of no conscious dishonesty. Neither was it a chance occurrence. Unexamined dishonesty is rampant in this world. I don't see how you can excuse scientists' publicly trading upon their professional reputations when they are not willing to exert self-discipline." I tried to ignore McConnell, but that phrase "unexamined dishonesty" kept haunting me. The happy ending was slipping from my grasp.

My new revision made a desperate attempt--to the point of bias--to classify the whole fiasco as a set of silly mistakes. But now the trio looked so pathetically inept that I foresaw another wall of resistance and resentment. Just as I sent it in, I learned that Abell was working on his own version, a confessional piece to be cosigned by Paul Kurtz if not by the now hibernating Marvin Zelen. Aha, this is an even better ending-- I withdrew my paper from The Skeptical Inquirer.

But this happy ending didn't occur either. Abell now echoed Frazier that nobody was interested in this topic any more. (Given the massive and malicious attack on Rawlins and the massive information control afterwards, this was obnoxiously true.) He now hinted that a short note in Marcello Truzzi's Zetetic Scholar might be sufficient. Next, his first draft of the piece actually repeated more errors than it corrected and continued the innuendoes about the Gauquelins' honesty. There was no more talk of even Kurtz co-signing; and, soon after, Abell said he was too busy with other work to get to the Mars paper very soon. Numbly I urged him not to let it fade away.

I did not know, of course, that Abell was at the same moment hitting the streets with a new round of the old arguments in the July 1982 issue of Psychology Today. He even worked in a new red-herring -- that only Gauquelin's own data showed the Mars Effect, thus dropping the Para Committee's replication. Still fixated on the final chorus of joy, I desperately tried to dismiss this new folly as a hapless hold-over from the bad old days. At least some errors had disappeared. But this denial mechanism couldn't hold. Abell had now launched the trio's fourth round of slurs against Gauquelin's integrity in five years, in spite of a relentless barrage of strong criticism against the statistical nonsense being used. This censure now included "sTarbaby" in October, "Personal Assessment" in December, Patrick Curry's paper in March, and "Numerology" in April, none of which deflected Abell in July. Thus CSICOP had chosen for its inner circle a habitually erroneous skeptic to replace Dennis Rawlins whose competence and integrity had proved to be exemplary in the Mars debate.

When the whole record is examined over five years, there is almost no instance in which merit wins out over self-serving bias. The one clear exception was providing Rawlins a carte blanche space in The Skeptical Inquirer, and even this was undermined by a flurry of simultaneous misstatements. Not only is the trio, in spite of all private admissions, publicly unstoppable, but Council backs them every inch of the way and gives Paul Kurtz almost total control over CSICOP's information flow. If the Fellows and Scientific Consultants of CSICOP do not put a stop to this, who do they think will?

ON THE MARS EFFECT

A LAST ANSWER TO M. GAUQUELIN

COMITÉ BELGE POUR L'INVESTIGATION SCIENTIFIQUES
DES PHÉNOMÈNES RÉPUTÉS PARANORMAUX

The answer of M. M. GAUQUELIN to the recent statement of the Belgian Committee PARA about the so-called Mars-effect has been carefully examined at the last meetings of the Committee. Its reactions are expressed hereunder:

Everyday, among the numerous scientific papers that are published in the world, one may find some new theories or results at the frontier of knowledge, that deny many others previously proposed. They sometimes lead to hard controversy for the greatest benefit of Science. But it is very rare that such situations evolve into personal antiscientific attacks as the ones already observed in the case of the Mars-effect claimed by M. GAUQUELIN. This never elevates the protagonists and does not lead to any scientific progress which finally is the only useful interest of such debates.

Therefore, once again, the Committee repeats that it will not follow M. GAUQUELIN in the way he has engaged himself since the publication of issue n 43 of the Committee's Nouvelles Breves in Septembre 1976 and that consists in mixing up arguments based on his own views of the problem and personal attacks. This does not help for a better mutual understanding.

To the contrary of M. GAUQUELIN - who apparently seems not willing to accept a fair discussion that could not lead to an approval of his views - the Committee is only interested in seeing Science progressing on a firm, stable and rocky basis, whoever could be right: the Committee or M. GAUQUELIN. The life of the Committee as well as the national or international good fame of its members are in no way related to the existence or the non-existence of a so-called Mars-effect. They are only related to the quality of the scientific investigations that the Committee wants to conduct as perfectly as possible. Therefore, the Committee has always been willing to answer to any question regarding the Mars-effect problem but - in order to avoid any misunderstanding and to clarify the debate - on the very fundamental condition that the discussion be conducted on the basis of a common adopted methodology. May we recall that the controversy lies in the mathematical formula used by M. GAUQUELIN for computing the theoretical diagramme and the one proposed by the Committee on the basis of a full analysis that has never been scientifically denied by anyone on any specific point? What make M. GAUQUELIN so reluctant since 1976 for any clear answer on such a simple point? Such an answer would certainly open the way for a new clear dialogue!

The Committee wishes to take this opportunity to state again that to its present knowledge, no other such full analysis (leading to a mathematical "model" of the problem) has been done by any other scientist. This of course does not mean that no other interesting comments, researches and computations have been made in that field by eminent scientists!

A REPLY

MICHEL GAUQUELIN RESPONDS THE STATEMENT OF THE COMMITTEE PARA:

"A last answer to M. Gauquelin" is a remarkably vague one. The Committee Para and its president, astronomer Dommanget, refuse to comply with the four requests I made at the end of my comments on the last Committee Para's statement (see ZETETIC SCHOLAR, No. 9, page 77). My requests are called "personal antiscientific attacks," and I am described as "not willing to accept a fair discussion."

"Personal antiscientific attacks" indeed to ask Dommanget: (1) to publish his own theoretical (expected) distribution of Mars in sectors at the birth of athletes; (2) to publish the outcomes of the counter-experiments he undertook; (3) to demonstrate on which precise point Rawlins' analysis and Abell's analysis of the problem--both of which are in agreement with my analysis--are "erroneous"; (4) to explain why the results of the Zelen test also vindicate my computations if I am wrong on this point! Here are four perfectly decent and scientifically crucial requests, I think. I am sure that ZETETIC SCHOLAR readers would have been curious, like myself, to know Dommanget's answers. But there is no answer at all in "A LAST ANSWER TO M. GAUQUELIN"! Actually, and despite its vagueness, this "last answer" is highly misleading. I would like to raise some points for the information of ZETETIC SCHOLAR readers.

The "common adopted methodology"

Dommanget says: "The Committee has always been willing to answer to any question regarding the Mars-effect problem...at the very fundamental condition that the discussion be conducted on the basis of a common adopted methodology" (Dommanget's emphasis). Note in passing how comical such a claim is. It simply means: "If you are not in agreement with me, I refuse to answer your question." A very childish rationale!

Actually, the control and replication of the Mars effect was conducted on the basis of a common adopted methodology between the Committee Para and me from the beginning. Definitive evidence is provided by the letter of January 28, 1962 sent to me by Prof. Jean Dath, former president of the Committee Para, who, according to Dommanget "made the fundamental statistical research on the problem." And what does this letter say? Here is the translation into English (please write me if you want a photocopy of the original in French):

> "I personally verified some of your results and I did not find anything which could be criticized from the statistical point of view. Of course, this verification supposes a priori that the data, i.e. your dates and hours of birth, are accurately gathered. What we hoped to do was, unfortunately, impossible: to replicate your work on a new sample of 500 Belgian athletes and the Mars positions. In spite of our efforts, we have not yet succeeded in gathering the necessary data. Please be sure that, as soon as M. Dommanget and myself have the data, the work will be done in minimum time. Hoping to give you our results soon...sincerely."

Never mind that the results were given to me only six years later. This document is very important. It shows that the analysis of my methodology, published in 1957 in our book METHODES POUR ETUDIER LA REPARTITION DES ASTRES DANS LE MOUVEMENT DIURNE, was done as early as 1962 by Dath and Dommanget and that it received their complete agreement. It is also clearly stated, in Dath's letter, that he and Dommanget were ready to gather a new sample of data and that the results of this sample will have full value since neither Dath nor Dommanget questioned my methodology. Therefore , the above letter-statement written by Dath in 1962 shows without any doubt that the test on a new sample of 535 athletes carried out in 1967-1968 by the Committee Para was conducted "on the basis of a common adopted methodology" (It would have been bizarre, by the way, to see the Committee undertaking a painful new test on athletes before being sure that I was using an accurate methodology!). Then, the awful thing happened: MM. Dath & Dommanget replicated the Mars effect...

The "full analysis leading to a mathematical model"

Dommanget also says: "The Committee wishes to take this opportunity to state again that at its present knowledge, no other such full analysis (leading to a mathematical model of the problem) has been done by any other scientist. This of course does not mean that no other interesting comments, researches and computations have been made in that field by eminent scientists!" Abell, Rawlins, etc., are called "eminent" and their computations "interesting." They found the same theoretical (expected) distribution of Mars in sectors that I found. But my computations are called "erroneous" by Dommanget, and I am accused of "personal antiscientific attacks." The dialectic of Dommanget is rather surprising.

Far more surprising is Dommanget's claim that "no other such full analysis (Dommanget's emphasis) leading to a mathematical model of the problem has been done by any other scientist.

And now, dear ZETETIC SCHOLAR reader, what do you think about a "mathematical model" built up after a "full analysis" which does not allow Dommanget to publish any theoretical (expected) frequencies of Mars??? What kind of sick science is that? What kind of trick, I should say? Come on M. Dommanget! In your sample of 535 athletes, and according to your calculations, Mars is found 68 times in the key sector N°1 (rising sector). Since there are 12 sectors, the average frequency for sector N°1 is 535/12 = 44.58. The actual excess of Mars in sector N°1 is +23.42; that is an excess of 52 percent above the average frequency (52 percent!). M. Dommanget, please, help us with your "mathematical model." Did you find an expected value of 70 for sector N°1, or, maybe, only 65? It should have been something very much like that to show that the Mars effect you found is an artifact and my analysis "erroneous." Actually, Rawlins, Abell and I, all independently, found almost the same expected value for sector N°1: around 48; and the Zelen test gave a very similar value.

Why did Dommanget never published his own expected Mars frequencies? I asked him several times through the years. Many people asked him, too. We all invariably got the same incredible answer: "such calculations cannot be published because they never were done..it is impossible"(!!) The beautiful "mathematical model" of Dath & Dommanget is very helpful indeed! It only means: "we refuse to believe in what we found." Their so-called "mathematical model" is a fine example of pseudo-science created to hide the Mars effect and to mislead the general reader.

The counter-experiments which vanished

But, if the beautiful "mathematical model" cannot help even the Committee Para itself, we are not in a desperate situation. There are several indirect procedures which allow us to see if the Mars effect replicated by the Committee Para is an artifact or not. The best solution is to conduct some counter-experiments with control groups. The Zelen test for instance, was one of these possible counter-experiments.*

To its credit, the Committee did carry out a series of crucial counter-experiments all of which vindicated our methodology. To its discredit, it did not publish its results. The 1976 Committee report is completely mute on this important point, and, for his part, president Dommanget suffers a strange loss of memory about it in all his public and private correspondance. For the benefit of Dommanget's memory and, more likely, for the information of ZETETIC SCHOLAR readers, I would like to describe the most effective counter-experiment carried out by Dommanget in 1970 (I briefly presented an account of this experiment in the International Journal of Interdisciplinary Cycle Research, 1972, pp. 381-389, but I think it worthwhile to give full information here about the test.)

Design of the counter-experiment:

A crucial test for judging any hypothetical demographic or astronomical bias is to use the same distribution as that of the champions' births (i.e. same year, month, day, place and time of birth), but to shuffle the hours of birth: each champion keeps his real birth date and place, but is given the birth hour of the preceding champion according to the alphabetical order. Thus the new group has exactly the same demographic and astronomical conditions as the champions' group with real birth hours.

The Committee Para repeated this test nine times, each time shifting the birth hour from one champion to the next one. For example, in the first control test, the champion number 4 keeps his birth date and place, but receives the birth hour of champion number 3; champion number 3 receives the birth hour of champion number 2, and so on. In the second control test, champion number 4 receives the birth hour of champion number

*It is worth recalling that the Zelen test was designed to judge the accuracy of the Committee Para objections against my methodology (THE HUMANIST, Jan/Feb 1976). In private correspondance, Dommanget has said that the Zelen test may contain "some error of the same type as the one made by Gauquelin." What kind of error? Why did Dommanget neglect to publicly point out this "error" in due time to all interested parties? Kurtz would have been more than happy to publish Dommanget's reservations in THE HUMANIST in 1976. In fact, the Committee Para rightly estimated that the results of the Zelen test would kill its alibi (see above and again its admirable "mathematical model"). Today, Dommanget is going to be more isolated among his colleagues. Even the highly skeptical French Committee for the study of paranormal phenomena (CFEPP) considers tests of the same kind as Zelen's the best procedure to provide empirically expected frequencies of Mars in sectors.

2, champion number 3 receives the birth hour of champion number 1, and so on. In the third control test, champion number 4 receives the birth hour of champion number 1, champion number 3 receives the birth hour of champion number 535, and so on.

Results of the counter-experiment:

They are given in Table 1 below, taken from an unpublished preliminary report written by Dath & Dommanget in 1970. The Committee Para suppressed these results from its published final report (NOUVELLES BREVES, N°43, 1976) without warning me and without any justification at all: the counter-experiment was simply ignored.

Classement alphabetique

cl	f_0	f_1	f_2	f_3	f_4	f_5	f_6	f_7	f_8	f_9	$f_{1,9}$
1	68	45	55	44	44	56	38	47	50	40	46,6
2	47	50	43	38	46	37	52	49	45	56	46,2
3	36	46	47	52	46	43	45	51	45	42	46,3
4	51	58	44	50	45	54	49	32	53	42	47,4
5	36	35	42	40	42	31	54	44	44	50	42,4
6	30	38	35	50	41	41	31	43	43	46	40,9
7	36	31	48	34	37	44	33	50	37	36	38,9
8	51	36	34	40	52	46	40	44	50	39	42,3
9	53	48	51	52	48	51	46	38	42	40	46,2
10	53	48	45	48	38	40	53	53	40	39	44,9
11	40	54	48	34	49	46	49	42	37	41	44,4
12	34	46	43	53	47	46	45	42	49	64	48,3
	X^2	33,0	24,9	36,1	32,2	21,6	40,8	43,1	25,8	60,4	25,4
	P	-	0,8%	-	-	3%	-	-	0,6%	-	0,7%

Explanations of and comments on the Table

"Classement alphabetique" means "alphabetical order."

From left to right, columns mean:

cl = sectors of Mars;
fo = actual distribution of Mars at the birth of the champions;
f1 to f9 = distributions of Mars for the nine counter-experiments;
f1, 9 = the nine counter-experiments taken together (each value is the average of the nine frequencies obtained for each of the 12 sectors).

At the bottom of the table, the two lines marked X^2 and P refer to chi square and probability. The values are obtained by comparing the actual distribution fo with each counter-experiment distribution f1, f2,... f9. The differences are all significant. Those between fo and f1, f3, f4, f6, f7, and f9 are significant at the .001 level. The comparison between fo and f1,9 (last column) is significant at .007.

So the Mars effect, replicated by the Committee Para, cannot be considered as an artifact or a demographic error.

Moreover, the values found here in the column f1,9 are very close to the theoretical (expected) values I calculated by my methodology (which received Rawlins's & Abell's agreement and were vindicated by the results of the Zelen test).

In the "key sector" N^{o}1 (rising sector) for instance, the Belgian Committee Para found 68 champions born with Mars in it (column fo). The combined value of the control-experiments gives 46.6 for this sector (column f1,9). I estimated, according to my analysis of the problem, the most probable value to be 47.6. In each case, the difference between 68 and 46.6 or 47.6 is highly significant.

Conclusion

It is fully demonstrated, I think, using Committee Para's own evidence, that (1) it successfully replicated the Mars effect on a new sample of athletes; (2) it found theoretically (1962) and by using counter-experiments (1970) that my methodology is accurate and the Mars effect is not due to an artifact.

The so-called "mathematical model" of the Committee Para is a trick, a useful trick, since it allows skeptical people to publicly claim with "good conscience," as Kurtz did recently, that "the Mars effect has not been replicated independently of Gauquelin's own data" (NEW SCIENTIST, 11 February 1982). A useful trick, maybe, but also a very bad one. The behavior of the Belgian Committee Para and its president, astronomer Dommanget, is a perfect example of pathological science.

POSTSCRIPT BY THE EDITOR

The above reply by Mr. Gauquelin was sent to Dr. J. Dommanget of the Belgian Para Committee indicating that ZS would publish any response that Committee might wish to make. A letter to ZS was received dated September 20, 1982 in which Dr. Dommanget stated that the Para Committee had taken note of Gauquelin's paper and had decided (1) not to answer any more papers by Mr. Gauquelin since it believes that Gauquelin "refuses" ("since 1976") to show where the Committee has made any error in its full analysis of the so-called Mars-effect, and (2) to propose that a full and correct translation into English be made of both Gauquelin's and the Committee's methodologies so that the fundamental points of difference might be clarified. (ZS is now pursuing the possibilities for obtaining such translations --and their approvals by their authors-- and welcomes suggestions or offers of help from our readers.)

In addition to the above, I proposed a possibly constructive meeting between Dr. Dommanget (for the Para Committee) and Mr. Gauquelin with mediation by Mr. Piet Hein Hoebens (the Dutch contact for the Committee for the Scientific Investigation of Claims of the Paranormal), that could take place in Brussells. It was hoped that such a meeting might help clarify the methodology used by the Para Committee for those of us who still fail to understand exactly why Gauquelin's objections might be inappropriate. Both the Para Committee and Dr. Dommanget rejected the proposed meeting on the grounds that it would prove useless in light of past discussions. A second letter was sent asking for reconsideration of this negative decision. A letter from Dr. Dommanget dated November 4, 1982, again declined the proposed meeting on the grounds that he felt it would be unlikely to lead to any scientific and fair discussion. He cited the above reply by Mr. Gauquelin as evidence of Gauquelin's improper "attitude." -- M.TRUZZI

A PROPOSAL BY MICHEL GAUQUELIN

After my reply to Lawrence Jerome's attack on my work in The Humanist (Sept/Oct 1975), Paul Kurtz wrote me a letter on October 13, 1975. The letter said: "A group of scientists in the USA led by Professor George Abell would like to review your research findings. Could you pease send us the following by air mail" (followed by a list of six volumes of data published by my laboratory, all of which I immediately sent to Paul Kurtz and to George Abell). Seven years have passed, and the "group of scientists" (CSICOP members) have failed to properly check my work. Several authors rightly consider that unjustice was done to me. This feeling is well expressed by Marcello Truzzi in his open letter on May 7, 1982, to George Abell who apparently has not yet publicly answered that letter.

Ideally, the proper way for this "group of scientists led by Professor George Abell" to restore the situation would simply be to keep its promise and to check the accuracy and the objectivity of my samples with the help of the volumes of data I sent it on its request. Unfortunately these scientists do not intend to do so. George Abell discovered that I was able to handle the technical problems of the research with accuracy (statistical treatment, actual and expected calculations, etc.), but he remains skeptical. He says that my findings will very probably turn out to be spurious because several biases could have entered into my samples. What can I do against such vague insinuations?

Granted, this attitude is not shared by all the scientists who know my work well. For instance, Prof. H. Eysenck and Dr. D. Nias, both from London University, write in their recent book Astrology: Science or Superstition? (1982, page 209): "Because Gauquelin, has, all along, published full details of his research in a series of documents, it is possible to evaluate independently the design and methods used in the research. This we have done, and we have been unable to find anything seriously wrong. On the contrary we have been impressed by the meticulous care with which the data have been set out and analysed."* For a large part, however, the scientific community, because of its strong prejudice against my findings, retains the impression that there is something fishy in my samples of birth data. I would like to give to all interested parties full opportunity to discover the truth on this crucial point.

I am eager to submit to a thorough "cross examination" about the way I collected my samples. All the data which support my findings are

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*"Meticulous care": exactly the same expression which was used by Paul Kurtz himself in The Humanist (Nov/Dec 1977) when he stated that he "inspected the Gauquelin's archives and was impressed by the meticulous care which the data had been collected." Why did Kurtz change his mind since? A mystery.

open to inspection from A to Z. I have in my laboratory the reference books (directories,etc,) I used in the research. I also keep all the birth information which I obtained through the years by writing directly to the birth registries of the cities listed as birth places of the subjects of my samples. Accordingly, here is my offer. I am willing to pay for all the expenses needed by any scientifically trained people (preferably representatives from some scientific group) who agree to perform this "cross examination." If this examination is made by mail, I offer to pay for the postage, photocopies of documents and the time consumed. If the examination is conducted in my laboratory, I will pay for all accomodations in Paris for so long as the investigation may last. My investigators may also want to write personnally to some birth registries in order to directly control the accuracy of some times of birth. I would also cover those expenses. My only condition is that these investigators agree to publish their conclusions in an appropriate medium, Zetetic Scholar for instance, or any other journal of similar kind.

I take the opportunity of this offer to remind all interested parties that our work on the Mars effect and sports champions occupies a very tiny part of all the investigations I have carried out over thirty years. In fact, there are three large series of experiments which could be easily checked. Studies on:

(1) successful professionals (15,000 birth data from several countries, distributed in ten different professions and published in full by my laboratory (Series A, Volumes 1 to 6, 1970-1971);

(2) Experiment on heredity (60,000 birth data of ordinary people - parents and their children - published by my laboratory (Series B, Volumes 1 to 6, 1970-1971);

(3) character-traits (the full catalogue of 50,000 trait-units systematically compiled from the biographies of 2,000 successful people is published by my laboratory in five large volumes with full details: Series C, 1973-1977).*

There are plenty of opportunities to investigate my findings. I really do hope that some trained scientists, CSAR members or others, will seriously consider my present proposal. Please contact: Michel Gauquelin, Laboratoire d'Etude des Relations entre Rythmes Cosmiques et Psychophysiologiques, 8, rue Amyot, 75005 Paris, France (phone: 535-17-20). Thank you!

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*Please note that the "top top" professional argument against my findings does not apply for either the (2) or (3) series of investigations. On the contrary.

PERSONAL REFLECTIONS ON THE MARS EFFECT CONTROVERSY

MARCELLO TRUZZI

The editorial position of ZETETIC SCHOLAR (ZS) remains open and seeks to be fair-minded towards all parties in the Mars Effect Controversy. And as the editor of ZS, I have tried to avoid direct involvement. But because of my past association with the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), because I have published two major critiques of the CSICOP involvement in this controversy (Patrick Curry's article in ZS#9 and Richard Kammann's article in this issue), and because of my heavy involvement with the extensive correspondences that have privately taken place among most of the parties involved, many now perceive me as an advocate rather than a mere mediator (the role I prefer) in this controversy. It should prove useful, therefore, for me to put forward some of my opinions (perhaps biases) into the public record so that ZS readers can recognize them clearly. Whatever my own views, of course, these should not be confused with the position of ZS. I speak only for myself and not for others on the editorial staff of ZS. This journal will continue to urge and encourage public dialogue between all viewpoints on these matters.

The Mars Effect Controversy really has two separate elements to it which are unfortunately often confused. The first element is the question of whether the claimed Mars Effect is scientifically valid. The second, perhaps the more important element, concerns the way in which this extraordinary claim has been challenged by its critics, especially the Belgian Comité Para (CP) & the U.S.-based CSICOP. Many, including myself, believe the Mars Effect is invalid while we also believe that the critical (so-called "skeptical") reaction to the claim has been scientifically erroneous and sometimes even reprehensible. I will examine each of these elements in turn.

THE "MARS EFFECT" CLAIM

Mistaken Escalation

The very name "Mars Effect" is a misnomer. The controversy centrally surrounds data purporting to show evidence for a statistically significant and non-chance correlation between persons emerging as sports champions and having Mars in certain positions at the time of birth. But, alas, both the Gauquelins and their critics have treated this correlation as though it demonstrated a causal relationship. Such a causal relationship would indeed be highly extraordinary and at least to some degree lend support to the notion of planetary "influence" as found in astrology. Thus, such a causal claim is not merely implausible to most scientists, it represents a kind of aid and comfort to what most scientists consider to be a pseudoscience that was supposedly discredited by astronomy. Gauquelin in effect reopened an old wound from an old battle which the astronomers thought they won long ago. Yet, in fact, this greatly exaggerates the evidence Gauquelin has actually put forward. It is fundamental that a correlation may be valid while due to any number of third factors; and Gauquelin has merely demonstrated (at best) the existence of a Mars Correlation (rather than Effect). Seen in this light, his evidence is really not that extraordinary at all. It does, of course, remain an anomaly, and it may be worthwhile to pursue its causes; but the evidence claimed really generates great excitement and passion if we prematurely leap to the conclusion that its validity demonstrates a causal connection supportive of astrology.

Why, then, has this anomalous correlation been persistently presented as though it necessarily was linked to astrology? In large part, of course, it is due to Gauquelin's work having originally centered around his empirical investigations into the claims of traditional astrology. The Gauquelins themselves do think that they are dealing with some sort of mysterious causal connection and the possible birth of a new science they term neo-astrology. However, Gauquelin has consistently acknowledged the he might be quite wrong, that the correlations he finds may be due to some mysterious third factor we have not yet been able to isolate and establish. But, perhaps more significantly, some (but by no means all) astrologers have greeted Gauquelin's claim of a Mars Effect with great joy and have offered him support for his research (e.g., the use of computers), while his fellow "normal" scientists have met his claims with either indifference or severe antagonism (far beyond what one normally obtains from critical peers in science). So, Gauquelin's alliance with some astrologers is understandable and largely the result of his having to find support for his work where he can. But the great irony in all this is that the vast majority of Gauquelin's work is severely damaging to traditional astrology, and he has always made this quite clear. If Gauquelin had not had the "misfortune" to stumble on a few anomalous correlations during his research into the claims of the astrologers, he would today be hailed by his critics as the greatest debunker of astrology science has so far produced. Even his critics today must admit that his empirical research constitutes the best body of evidence against traditional astrology to be found anywhere. (And, of course, Gauquelin's vast evidence against astrology has not received anything like the criticism levelled against the similarly derived evidence for the Mars Effect.)

Although we can readily understand Gauquelin's framing his anomalous finding in the context of astrology, the more interesting question for the sociologist of science is: Why have the critics inflated this "mere correlation" into an "effect" which thereby increases the extraordinariness of the claim rather than diminishes it? Certainly, two functions emerge from such an escalation. (1) By increasing the extraordinariness of the claim, one can call for stronger evidence for it than Gauquelin has to offer; if degree of proof must be commensurate with the extraordinariness of the claim, this places a greater burden of proof on Gauquelin. (2) The importance of the critics' challenge is increased if the object being attacked is more important. This has two components. (a) There is little point in attacking a mere correlation which results only in discrediting a minor anomaly. The Mars Correlation is only significant as an issue when it is tied to the claims of the astrologers, the real targets of the critics who have rallied to "fight pseudoscience." And (b) by attacking astrology and tapping the hostility against that "irrational superstition," the critics make themselves newsworthy and will be viewed as "heroes" by those want to see such "nonsense" crushed and the public "educated." But such escalation of the basic claim is really inexcusable for scientific critics, whose first order of business in dealing with an extraordinary claim should be to seek to minimize its extraordinary character wherever that can reasonably be done. A proponent of an anomaly may have good reasons (both practical and theoretical) for interpreting his anomaly as of maximum significance; but the critics of an anomaly have an obligation to "cut it down to size" so as not to exaggerate its importance. Here the opposite seems to have happened, and this has resulted in charges that the data must be spurious because the claim was painted as incredible. By inflating the claim, it becomes all the more important, too, to discredit it. This seems to have resulted in irresponsible attempts to debunk the claim.

The Status of the Claim

The current evidence strongly indicates that (a) a Mars Correlation was validly found by the Gauquelins, (b) a correlation was found in several replications by the Gauquelins using different samples, (c) a similar correlation was found in replications by the CP and the first study conducted by Kurtz-Zelen-Abell (KZA). In regard to (a) and (b), the key question concerns the validity of the Gauquelins' data. It has repeatedly been incorrectly stated that there is no way to check this data. Not only have the Gauquelins published all their data (so computations can easily be checked), they have kept all original records from the birth registries, and these have been made available to any serious researchers. In fact, the Gauquelins have urged critics to check this data (see Gauquelin's proposal in this issue of ZS). It should also be noted that checking the data would not necessitate checking every single case; one needs to look only at the sports champions for whom there was a Mars Correlation (only about 23% of the sports champions). However, this is largely a false issue. Based only on Gauquelin's work, a critic can properly say that the evidence is simply not strong enough until there are independent replications, and this does not necessitate calling the Gauquelins' data into question. In science, we can take an agnostic, wait-and-see position and speak of evidence as "unconvincing" rather than leap to the conclusion that something spurious is present. If you don't believe that what someone tells you he saw actually happened, you may think him mistaken and insist on further witnesses without calling the original narrator a liar. Once a critic claims Gauquelin's work is spurious, then that critic has the burden of proof on him to show how it is spurious. That simply has not yet been done successfully, so we must proceed on the presumption that the Gauquelins' data is legitimate. The claims put forward by the Gauquelins are falsifiable, but some critics' vague and unsupported accusations of spurious data are not. All statements claiming scientific status must be falsifiable, whether made by proponents or critics.

Although the CP and KZA may wish to reanalyze their data in a way that questions the existence of a Mars Correlation, the fact remains that both their studies replicated Gauquelin's own work. That is, if the analysis applied to Gauquelin's studies is applied to the data used by the CP and by KZA, those results support the claim of a Mars Correlation. In the case of the CP, they questioned the theoretical chance expectation level used by Gauquelin in his own work and concluded that their study did not therefore show evidence for a Mars Effect. But it is apparent that this is post hoc reasoning on their part since the same reasoning could have been applied to Gauquelin's own work in the first place. If they had originally reasoned this way, they would never have felt it necessary to replicate Gauquelin's work at all. They could have explained his results away the same way they explained away their own. (Of course, this does not prove that the CP criticism of the chance level used is incorrect; but it does demonstrate that it was a criticism that must have been "realized" only after they got results that otherwise would force them to admit support for the Mars Effect.)

In the first KZA test, it is clear that the total sample they used did show the Mars Effect. Only through reanalysis of that data (sample splitting,etc.,) can that data be made to fall short of a demonstration of the claimed correlation (a reanalysis that Kammann, Dennis Rawlins, and others have criticized). Though KZA reach different conclusions from Gauquelin (as did the CP), their data results unequivocally constitute a replication of Gauquelin's own finding. They did not replicate his analysis, but they certainly did replicate his own data pattern. (We should not confuse replication of a study with replication of the data results, and we should not confuse replication of data results with replication of the analysis of such data.)

So, whether or not such replications have faults similar to those that might be present in Gauquelin's early studies, they do indeed constitute replications of his work. All these replications may be flawed just as Gauquelin's work may be flawed, but the critics who have repeatedly asserted that Gauquelin's work has not been replicated by others simply misstate the facts. And the fact that supposed "flaws" found during these replications --resulting in new analyses to discount the Mars Correlation -- follow rather than precede the data these critics have produced but which support the Mars Correlation, suggests strongly that the critics are really rationalizing away embarrassing findings. Clearly these replications were initiated by the critics with the expectation that no significant Mars Correlation would emerge in their studies; for until the results came in which supported Gauquelin, these "flaws" had gone unnoticed.

What, then,can we conclude from all this? First, that the Mars Effect is really a claimed correlation, an anomaly far less extraordinary -- and thus requiring far less evidence -- than a claim for an incredible causal relationship. Second, what evidence exists for the Mars Correlation does persistently show up if we accept the basic methodology proposed by Gauquelin. Third, if there is something fundamentally wrong with Gauquelin's methodology, that error has not been clearly stated or demonstrated in such a way as to convince skeptics toward such a claimed error (e.g., even critics of the Mars Effect like George Abell, Dennis Rawlins, and Richard Kammann have said that they do not understand the reasoning put forward by the CP, and Ray Hyman has raised criticisms of the KZA study rather similar to those put forward by Richard Kammann). Those independent investigations that have been conducted into Gauquelin's data (e.g., by Hans J. Eysenck, but we should recall that checks were also conducted by the CP and by Paul Kurtz) indicate that no irregularities are present there. (It might also be mentioned that privately circulated "critiques" of Gauquelin's analyses have been seen by many of us, but such critiques --e.g., those by Lawrence Jerome and Colin James -- have not been endorsed by their fellow critics who have found them unconvincing or in error.) Fourth, Gauquelin's own work has withstood critical appraisal thus far levied against it by responsible critics. And, fifth, following the normal procedures of science, we should accept the evidence for the Mars Correlation while recognizing that this evidence may yet be superseded by new research and data analysis that may establish it is invalid. But the burden for such replications and reanalyses must now fall upon the critics of the Mars Correlation. The Mars Correlation may or may not really be present, but the case ultimately must rest on the reasons given for any conclusion. So far, the case against the Mars Effect rests largely on spurious reasoning. The burden of proof is on the claimant, and Gauquelin has accepted that burden by conducting his studies. And if extraordinary claims require extraordinary proof, Gauquelin's meticulous research (which goes well beyond the scientific norm of openness since all his data is publicly available), plus the replications by his critics, surely constitutes such extraordinary evidence (especially when we recognize that the claim of a correlation rather than a causality is simply not that extraordinary).

The CP's Study

The Belgian CP, as I have noted above, replicated Gauquelin's experiment and his results but have argued that these do not support his conclusions because the CP questions the validity of the theoretical chance expectation level that Gauquelin used to compare with his (and their) group of sports champions. It is essential to note that, first of all, the CP unequivocally has accepted the data base of athletes they used. It has been pointed out that Gauquelin was asked to gather the data for them on these sports champions., and this has been brought up to question the authenticity of the sample. But (1) the CP chose the athletes, (2) Gauquelin merely acted in a secretarial capacity, at their request, in writing to the birth registries, (3) the data from the registries was directly transmitted to the CP, (4) these documents remain available for checks, and (5) the CP almost certainly checked this documentation since they flatly endorse the data and explicitly have exonerated Gauquelin of any charges related to error on his part regarding that data. Thus, questions raised about this data by Paul Kurtz and George Abell are clearly irrelevant unless they are really challenging the claims of the CP which sides with Gauquelin on this specific issue.

The main reason the CP has given for discounting the pro-Mars Effect interpretation of their own study concerns the matter of the theoretical chance expectation level that Gauquelin has used with which to compare the sports champion sample. This explanation is at this point still unclear to almost all critics of the Mars Effect outside of those connected with the CP. This may be due to problems of translation. and this will eventually be clarified once an authorized translation becomes available (ZS is now seeking to obtain such a translation in English). However, in principal, the original CP argument had merit since we had no empirical basis*for knowing what the chance rate for non-athletes (the general population) should really be. What has been largely overlooked is that the Zelen test (the KZA study) was constructed to avoid this very problem since the Zelen approach used an empirical (rather than theoretical) control group of non-champions. In the KZA test, the non-champions empirically demonstrated a Mars Effect of about 17%, the very level that Gauquelin and others had theoretically expected. Thus, the KZA test would seem --if nothing else-- to demonstrate that the CP test really did show evidence for the Mars Effect and that the CP interpretation is probably wrong. In light of the KZA test, the CP test clearly corroborates Gauquelin's interpretation. In this sense, the CP test and the KZA test are contradictory. Kurtz and Abell have chosen to remain silent about this fact and have misled their readers into thinking the CP test conclusions are congruent with their own negative conclusions about the Mars Effect. (For details about the CP test, I refer you to the exchanges between Gauquelin and J. Dommanget in ZS#9 and this issue.)

The CSICOP Connection

KZA conducted two separate studies, the first was published in THE HUMANIST (the KZA or Zelen test), the second was published in THE SKEPTICAL INQUIRER (the U.S. test). When Dennis Rawlins first brought charges against these studies in his FATE article ("sTarbaby"), the initial reac-

* independent of Gauquelin's own data on non-athletes which supported his theoretical expectation.

tion of CSICOP's Councilors was that these simply were not CSICOP studies at all, merely studies conducted by three members of CSICOP, one of whom happened to be its chairman and one other a Councilor. It was quickly pointed out that this defense was completely wrong as far as the second test was concerned for CSICOP clearly sponsored the U.S. test, paid for it, and took credit for it via public statements at the time it was released. No one has claimed that all the members of CSICOP should be held responsible for the CSICOP related tests. The charges have been made against the CSICOP leadership, its Executive Council, and mainly by ex-Fellows of the CSICOP who wished to bring these faults to the attention of the CSICOP general membership. The CSICOP leadership has consistently tried to make it appear that the critics were attacking CSICOP rather than them. They have tried to make it appear that we critics have been out to destroy rather than reform CSICOP, a thoroughly untrue picture of the motives of the critics. Unfortunately, this picture of the critics has apparently been accepted by most of the CSICOP Fellows since they thus far have been remarkably indifferent to the whole affair. (And this indifference has actually been cited by Paul Kurtz and Kendrick Frazier as good reason for why further material on this controversy should not appear in THE SKEPTICAL INQUIRER. CSICOP is interested in the education of the general public but perhaps less interested in the education of its members.) The fact that the CSICOP Council recently placed an announcement in THE SKEPTICAL INQUIRER (following Rawlins' public critique) that CSICOP would no longer conduct or endorse research, is easily read as an admission, in fact, that they had sponsored the second KZA study (the U.S. test), and that this mistake would not be made again. (An unfortunate way of handling this embarrassment since one of the original reasons for forming CSICOP was so that they could conduct research and not merely encourage its being done by others.)

If the connection between the CSICOP and the U.S. test is clear, the connection between it and the first KZA study is far less so. It has been repeatedly pointed out by the Councilors that this first study was not even sponsored by CSICOP. That defense is not congruent with the facts. It is certainly true that the KZA study was published in THE HUMANIST and not even in THE SKEPTICAL INQUIRER. But what is overlooked is that in those early days of CSICOP (when in fact I was co-chairman of CSICOP and on the Council) there was no clear distinction at all between the American Humanist Association and CSICOP. The AHA was the sponsor and creator of CSICOP (funding to initiate CSICOP came from the AHA). Paul Kurtz was the editor of THE HUMANIST as well as chairman of the CSICOP. Funding seems to have been totally intertwined. CSICOP only became a separate organization after the first test was initiated, mainly because Paul Kurtz left his position as editor of THE HUMANIST. This blending of the AHA and CSICOP matters was well known and a major reason for my own early problems with CSICOP since I had from the start disapproved what I saw as the authoritarian manifesto published by Kurtz against astrology in THE HUMANIST, publicity for which actually promoted the initiation of CSICOP. Certainly, to now claim no serious connection existed between CSICOP and the original KZA test is remarkable post hoc reasoning that is more convenient than accurate.

But whatever the exact relationship between CSICOP and these anti-Mars Effect experiments, the clear fact remains that the Council of CSICOP has generally allowed the public to believe these studies are competent and that the criticisms levelled by Rawlins and others have been met. Their silence has generally been interpreted as consent, and that has been intentional. In other words, in my view, the CSICOP leadership has engaged in stonewalling their critics' charges while also resorting to ad hominem attacks (especially against Rawlins) and seeking to make it appear the CSICOP "mission" and not their own bungling is under attack. This is a tragedy since the major criticism against the CSICOP's handling of this affair has come not from allies of Gauquelin but from fellow rationalists who are in fact skeptical about the validity of the Mars Effect. In short, the CSICOP has been --in my view -- guilty of the very pathological science that they were set up to attack. Instead of exemplifying a rational approach to an anomalous claim, CSICOP has descended into protecting orthodoxy and its own reputation as a goal more important than finding the truth. The inquirers have indeed become the Inquisitors that some feared they might. This is a great loss since the world truly needs a responsible group of critics to challenge the real nonsense and pseudoscience that competes with science.

All of this is not to say that the CSICOP has not otherwise done some valuable work. Nor do I suggest that most of the errors committed by its Council have been intentional. The sad part of all this is that honorable men could make such mistakes, often probably with good intentions and perhaps short-sighted high motives. Seeing their efforts as a Great Crusade Against the Irrational, I think they just got carried away, found themselves on the defensive, and then mistakenly tried to rationalize or ignore the errors -- all in the name of the Good Fight and for the Just Cause. Alas, as Goya put it so well: "The sleep of reason produces monsters."

Finally, on this issue, it should be noted that a number of Fellows of CSICOP were outraged by the behavior of the Council, and these Fellows resigned. A few others have expressed their concern. A few of the Technical Consultants to the CSICOP also resigned. But the vast majority of the Fellows remain apathetic or support the CSICOP (on this, see the early poll by Robert McConnell, whose results are published in this issue). So far there is little sign of reform demands from the CSICOP membership.

WHAT'S NEXT?

The CSICOP has recently altered the membership of its Executive Council. Perhaps that will produce changes. George Abell has circulated a private memo in which he acknowledges many of the errors charged by Kammann. Perhaps that may yet take the form of a public document. Perhaps, as I have urged, such a public admission of errors may be jointly signed by Abell, Kurtz, and Zelen. That would do much to clear the air.

A new test of the Mars Correlation has been initiated, this time to be conducted by his French critics. They will gather all data and will, we are told, follow the guidelines for inclusion put forward by Gauquelin. If they get positive results, none should be able to blame Gauquelin. But if their results are negative, I trust they will make all their records available to skeptics towards their work in the same way that Gauquelin has done for his critics.

To a degree, the sloppiness of the critics in handling the Mars Effect claim has led to Gauquelin's being considered somewhat of a martyr by some.

A similar counter-reaction followed the vigilante-like response of scientists to the extraordinary claims of Immanuel Velikovsky. Science does not need these sorts of heroes, either martyrs or knights. The best antidote to bad science, an eminent CSICOP Fellow has said, is good science. If in fact Gauquelin represents a case of bad science, if we seek to invalidate the Mars Effect, let us tackle the problem using good science. And if we somehow find that the Mars Effect is real, let us rejoice rather than weep; for we will have found something new and challenging in nature, something that might help us better reshape our incomplete map of it. Isn't that what good science is really all about?

The Mars Effect controversy remains unresolved. Though this essay has tried to bring some clarification into matters, it may fail to do so. But I retain my faith in science as a self-correcting system, and I urge and invite advocates on all sides to present their arguments and evidence in public forums. Much of the controversy has taken place in the form of semi-private letters and memoranda circulated in selective fashion. Science demands openness, so I hope we shall soon see full exchange of opinions in public forums. I have urged the CSICOP leadership to reply to the charges that have been made and have offered them space in ZS for that purpose. Similarly, I have urged all the Fellows to participate in the dialogue and have informed them about it. Perhaps the next issue of ZS will contain their replies. Whatever my own views, I believe that the readers of ZS (many of who staunchly support the CSICOP otherwise) can fairly reach their own conclusions. Even those of us critical of the CSICOP do not entirely agree about all the issues that have been raised. Similarly, I do not believe all the defenders (or even all the Councilors) of CSICOP are of one mind. But it is only through rational and public dialogue that we can approximate a true picture of what has been happening. Whether we be "zetetics" or "skeptical inquirers," can we not cooperate as true scientists and emphasize the norms of openness and disinterestedness? We must at least try to do so.

A POSTSCRIPT (11/28/82)

In re-examining the above, I see that little mention has been made of the second CSICOP-associated study, the U.S. test. It must be clearly noted that this U.S. test, if accepted at face value, does not demonstrate the Mars Effect. However, as Patrick Curry has argued in ZS#9, this study has serious methodological problems because of controversy over its data sources and the selection processes used. At the date of this writing, KZA have not replied to Curry's (and Gauquelin's) criticisms. Until such responses are forthcoming, or no response appears certain, I simply refer reader's to Curry's article for independent evaluation. For myself, I found Curry's general criticisms reasonable and convincing.

Throughout this whole affair, information has reached me that KZA have been preparing a possible reply to their critics which may be published soon. I hope this information proves correct, and such a reply remains most welcome.

RANDOM BIBLIOGRAPHY ON THE OCCULT AND THE PARANORMAL

Abell, George O., "The Mars Effect," Psychology Today, July 1982, pp. 8-13.

Anonymous, "License to Misinform," Discover, April 1982, p.12. (On the "In Search Of" TV series.)

Anonymous, "Living Human Fossils in Outer Mongolia?" New Scientist, March 25, 1982, p. 778.

Awanbor, David, "The Healing Process in African Psychotherapy," American Journal of Psychotherapy, 36, 2 (1982), 206-213.

Baker, R. Robin, "The Human Magnetic Sense," Theoria to Theory, 14 (1981), 241-246.

Barber, Simon, "The Boss Don't Like Robbery, Make It Swindle: Inside the National Enquirer," Washington Journalism Review, July/Aug. 1982, pp. 46-49.

Barone, Nancy C., "How Foregone Conclusions about the Mind-Body Relation Inhibit Research," Psychological Reports, 49 (1981), 812-814.

Bartley, S. Howard, "Magic: A Forerunner of Science," Perceptual and Motor Skills, 54 (1982), 1264.

Beaumont, Roger A., "C^{nth}? On the Strategic Potential of ESP," Signal (Journal of the Armed Forces Communications and Electronics Association), 36, 5 (Jan. 1982, 39-45.

Becker, Carl Bradley, Survival: Death and Afterlife in Christianity, Buddhism, and Modern Science. Doctoral dissertation (Philosophy), University of Hawaii, 1981. University Microfilms #8129384. 621pp.

Bellezza, Frances S., "Updating Memory Using Mnemonic Devices," Cognitive Psychology, 14 (1982), 301-327.

Beloff, John, "Is Normal Memory a 'Paranormal' Phenomenon?" Theoria to Theory, 14 (1980), 145-162.

Beloff, John, et al., "Discussion: Memory," Theoria to Theory, 14 (1981), 187-203.

Berman, David, "Hume and Collins on Miracles," Hume Studies, 6 (1980), 150-154.

Bernstein, Ralph, "Wallace: The Man Who Almost Pipped Darwin," New Scientist, June 3, 1982, pp. 652-655.

Brackenridge, J. Bruce, "Kepler, Elliptical Orbits, and Celestial Circularity: A Study in the Persistence of Metaphysical Commitment," Annals of Science, 39 (1982), 117-143 and 265-295.

Brackenridge, J. Bruce, "The Signs of Success," Times Literary Supplement, July 9, 1982, pp. 731-732.

Brady, Ivan, "The Myth-Eating Man," American Anthropologist, 84 3 (1982), 595-611.

Burton, Muarice, "The Loch Ness Saga," New Scientist, June 24, 1982, p. 872; July 1, 1982, pp. 41-42; and July 8, 1982, pp. 112-113.

Byrne, Patrick H., "God and the Statistical Universe," Zygon, 16, 4 (1981), 345-363.

Carrington, Richard, "The Natural History of the Mermaid," The Saturday Book, 19 (1959), 292-301.

Carrington, Richard, "The Natural History of the Unicorn," The Saturday Book, 20 (1960), 272-285.

Chandrasena, Ranjith, "Hypnosis in the Treatment of Viral Warts," Psychiatric Journal of the University of Ottawa, 7, 2 (1982), 135-137.

Cherfas, Jeremy, "Horoscope Horror Story," New Scientist, April 22, 1982, pp. 245-246.

Cherfas, Jeremy, "Mind-Bending Research," New Scientist, Aug. 12, 1982, pp. 444-445. Letter of comment by Julian Isaacs in New Scientist, Aug. 26, 1982, p. 579.

Clube, Victor, and Bill Napier, "Close Encounters with a Million Comets," New Scientist, July 15, 1982, pp. 149-151.

Cohan, Jonathan L., "What Is Necessary for Testimonial Corroboration?" British Journal for the Philosophy of Science, 33 (1981).

Cohen, David, "Paranormal London," Time Out, August 13-19, 1982, pp. 2-15.

Craig, Robert P., "Loch Ness: the Monster Unveiled," New Scientist, Aug. 5, 1982, pp. 354-357.

Degh, Linda, "UFO's and How Folklorists Should Look at Them," Fabula, 18, 3/4 (1977), 242-248.

de Mille, Richard, "And God Created Evooution," National Review, March 19, 1982, pp. 228-289.

Dettling, J. Ray, "Time Travel: The Ultimate Trip," Science Digest, Sept. 1982, pp. 81-88 & 97.

Doherty, Jim, "Hot Feat: Firewalkers of the World," Science Digest, August 1982, pp. 66-71.

Drab, Kevin J., "The Tunnel Experience: Reality or Hallucination?" Anabiosis, 1, 2 (1981), 126-152.

Draper, Thomas W., and Milagros M. Munoz, "Minor Physical Anomalies, Footprints, and Behavior: Was the Buddha Right?" Perceptual and Motor Skills, 54 (1982), 455-459.

Du Boulay, Juliet, "The Greek Vampire: A Study of Cyclic Symbolism in Marriage and Death," Man, N.S. 17 (1982), 219-238.

Eastwell, Harry D., "Voodoo Death and the Mechanism for Dispatch of the Dying in East Arnhem, Australia," American Anthropologist, 84, 1 (1982), 5-18.

Eccles, Sir John, "Beyond the Brain," Omni, Oct. 1982, pp. 56-58 &62.

Ericson, Les, "Butterflies in the Dark," Omni, July 1982, pp. 46-48, 50-51, 96 & 98.

Feyerabend, Paul, "More Clothes from the Emperor's Bargain Basement," British Journal of Philosophy of Science, 32 (1981), 57-94.

Fields, Karen E., "Charismatic Religion as Popular Protest: The Ordinary and the Extraordinary in Social Movements," Theory and Society, 11 (1982), 321-361.

Finkler, Kaja, "A Comparative Study of Health Seekers: Or, Why Do Some People Go to Doctors Rather than to Spiritualist Healers?" Medical Anthropology, 5, 4, 383-424.

Force, James E., "Hume and Johnson on Prophecy and Miracles: Historical Context," Journal of the History of Ideas, 43, 3 (1982), 463-475.

Franklin, R.L., "On Taking New Beliefs Seriously: A Case Study," Theoria to Theory, 14 (1980), 43-64.

Fraser, Alex, and Kimerly J. Wilcox, "Perception of Illusory Movement," Nature, 281, 5732 (Oct. 18, 1979), 565-566.

Fraser, Alex, Kimerly Wilcox and Stephanie Storgion, "The Filling-In Illusion and Moving Visual Phantoms," Perceptual and Motor Skills, 54 (1982), 543-555.

Frazier, Kendrick, "How to Cover 'Psychics' and the Paranormal," Bulletin of the American Society of Newspaper Editors, April 1982, pp. 16-19.

Gabriel, Chester E., Communications of the Spirit: Umbanda, Regional Cults in Manaus and the Dynamics of Mediumistic Trance, Doctoral dissertation (Anthropology), McGill University, 1981.

Gaither, Douglas M., Jr., and Leonard Zusne, "Simulation of the Telephathy Experiment with Zener Cards," Behavior Research Methods & Instrumentation, 10 (1978), 78-80.

Galanti, Geri-Ann, The Psychic Reader as Shaman and Psychotherapist: The Interface between Clients and Practitioners' Belief Systems in Los Angeles. Doctoral dissertation (Anthropology), University of California at Los Angeles, 1981. University Microfilms #8120960. 342pp.

Gardner, Martin, "According to Hoyle," Discover, March 1982, p. 12.

Gardner, Martin, "Eysenck's Folly," Discover, October, 1982, p. 12.

Gardner, Martin, "Quantum Weirdness," Discover, October, 1982, pp. 69-75.

Garrison, Jim, "Meeting a Firewalker," Theoria to Theory, 13 (1979), 191-195.

Gauquelin, Michel, Report on American Data, With the Publication of 1400 Birthdata and 5400 Personality Traits of Successful American. Paris: Laboratoire D'Etude Des Relations Entre Rythmes Cosmiques Et Psychophysiologiques (Scientific Documents #10), 1982.

Gibson, H.B., "The Use of Hypnosis in Police Investigations," Bulletin of the British Psychological Society, 35 (1982), 138-142.

Gliedman, John, "Interview: Brian Josephson," Omni, July 1982, pp. 86-89 114 & 116.

Goldberg, Alan Bruce, Commercial Folklore and Voodoo in Haiti: International Tourism and the Sale of Culture. Doctoral dissertation (Anthropology), Indiana University, 1981. University Microfilms #8119054. 318 pp.

Goldin, Greg, "That's (Not So) Incredible!"TV Guide, May 15, 1982, pp. 4-8.

Goldman, Michael, "Against Feyerabend: The Meaning of Progress in Science," Research in Philosophy and Technology, (1981), 28-38.

Gordon, Michael, "How Socially Distinctive is Cognitive Deviance in an Emergent Science? The Case of Parapsychology," Social Studies of Science, 12 (1982), 151-166.

Gray, J. Patrick, and Linda D. Wolfe, "Sociobiology and Creationism: Two Ethnosociologies of American Culture," American Anthropologist, 84, 3 (1982), 580-594.

Gunter, Barrie, "Can Astrology Predict Personality?" Bulletin of the British Psychological Society, 33 (1980), 155-157.

Hacking, Ian, "Do We See Through a Microscope?" Pacific Philosphical Quarterly, 62 (1981), 305-322.

Hadfield, Mike, "A Convocation of Dragons," The Saturday Book, 13 (1953), 115-127.

Hambourger, Robert, "Belief in Miracles and Hume's Essay," Nous, 14 (1980), 587-604.

Hand, Wayland D., "Will-of-the-Wisps, Jack-o'-Lanterns and Their Congeners: A Consideration of the Fiery and Luminous Creatures of Lower Mythology," Fabula, 18, 3/4 (1977), 226-233.

Harrison, Michael, Vanishings. London: New English Library, 1981. 190 pp. 1.25 pounds. An entertaining popular account of alleged abnormal or paranormal dissappearances. The cases described need to be contrasted with alternative accounts put forward by critics of the paranormal explantions, before serious conclusions can be reached. But a good general survey to introduce the topic.

Hart, James J., "Psychology of the Scientist: XLVI: Correlation between Theoretical Orientation in Psychology and Personality Type," Psychological Reports, 50 (1982), 795-801.

Hirsch, Jerry, "To'Unfrock the Charlatans.'" Sage Race Relations Abstracts, 6, 2 (1981), 1-66. (re racism and pseudoscience)

Hufford, David, "Christian Religious Healing," Journal of Operational Psychiatry, 8, 2 (1977), 22-27.

Hufford, David, "Humanoids and Anomalous Lights: Taxonomic and Epistemological Problems," Fabula, 18, 3/4 (1977), 234-241.

Jahn, Robert G., "The Persistent Paradox of Psychic Phenomena: An Engineering Perspective," Proceedings of the IEEE, 70, #2 (Feb. 1982), 136-170.

Johnson, Oliver A., "Hume's 'True' Scepticism," Pacific Philosophical Quarterly, 62, 403- 410.

Jones, Funmilayo M., Strategies and Techniques Used in Occasion Maintenance: An Examination of Reader-Client Relationships in a Tearoom. Doctoral dissertation (Sociology), Boston University Graduate School, 1981. University Microfilms #8126798. 302pp.

Jurma, William E., "Evaluations of a Credibility of the Source of a Message," Psychological Reports, 49, (1981), 778.

Kenny, Michael G., "Multiple Personality and Spirit Possession," Psychiatry, 44, 4 (1981), 337-358.

Kiline, Kenneth S., E.M. Docherty, and H. Farley, "Transcendental Meditation:, Self/Actualization, and Global Personality, "Journal of Psychiatry, 106, (1982), 3-8.

Koumjian, Kevin, "The Use of Valium as a Form of Social Control," Social Science and Medicine, 15E (1981), 245-249.

Kramer, Milton, "The Psychology of the Dream: Art or Science?" Psychiatric Journal of the University of Ottawa, 7, 2 (1982), 87-100.

Kronefeld, Jennie J., and Cody Wasner, "The Use of Unorthodox Therapies and Marginal Practitioners," Social Science and Medicine, 16 (1982), 1119-1125.

Lester, David, "Astrologers and Psychics as Therapists," American Journal of Psychotherapy, 36, 1 (1982), 56-66.

Liffman, Paul, "Vampires of the Andes," Michigan Discussions in Anthropology, 2, (Winter 1977), 205-226.

Lofland, John, "Conversion Motifs," Journal for the Scientific Study of Religion, 20, 4 (1981), 373-385.

Lofland, John, "Crowd Joys," Urban Life, 10, 4 (1982), 355-381.

Lowe, Walter L., "Bad Dreams in the Future Tense," Playboy, March 1980.

Mackkenzie, Brian, "Joseph Banks Rhine: 1895-1980," American Journal of Psychology, 94 (1981), 649-653.

Markoff, John, and Daniel Regan, "The Rise and Fall of Civil Religion: Comparative Perspectives," Sociological Analysis, 42, 4 (1982), 333-352.

Mathes, Eugene, "Mystical Experiences, Romantic Love, and Hypnotic Susceptibility," Psychological Reports, 50 (1982), 701-702.

McLachlan, Hugh, and J.K. Swales, "Tibbett's Theory of Rationality and Scottish Witchcraft," Philosophy of the Social Sciences, 12 (1982), 75-79.

McMullen, Anna, "A Census of Sea-Serpents," The Saturday Book, 13 (1953), 104-114.

Meyer, Alfred, "Do Lie Detectors Lie?" Science 82, June, 1982, pp. 24-27.

Moor, James, "Split Brains and Atomic Persons," Philosophy of Science, 49 (1982), 91-106.

Motz, Lotte, "Giants in Folklore and Mythology: A New Approach," Folklore, 93, (1982), 70-84.

Mulkay, Michael, and G. Nigel Gilbert, "Accounting for Error: How Scientists Construct Their Social World When They Account for Correct and Incorrect Belief," Sociology, 16, 2 (1982), 165-183.

Mulkay, Michael, and C. Nigel Gilbert, "Putting Plilosophy to Work: Karl Popper's Influence on Scientific Practice," Philosophy of the Social Sciences, 11 (1981), 389-407.

Naughton, John, "Revolution in Science: 20 Years On," New Scientist, August 5, 1982, pp. 372-375.

Nederman, Cary J., and J.W. Goulding, "Popular Occultism and Critical Social Theory: Exploring Some Themes in Adorno's Critique of Astrology and the Occult," Sociological Analysis, 42 (1982), 325-332.

Newland, G. Anthony, "Differences Between Left- and Right-Handers on a Measure of Creativity," Perceptual and Motor Skills, 53 (1981), 787-792.

O'Connell, M.C., Spirit Possession and Role Stress Among the Xesibe of Eastern Transkei," Ethnology, 21, 1 (1982), 21-37.

O'Neill, L.J., "Corroboration Testimonies," British Journal for the Philosophy of Science, 33 (1982), 60-63.

Otubanjo, Femi, "Wittgenseinianism and Magico-Religious Beliefs," Theoria to Theory, 13 (1979), 149-162.

Owens, R. Glynn, and Stuart B. Linke, "Psychology and Astrology," New Humanist, March 1980, pp. 142-143.

Peters, Larry G., "Trance, Initiation, and Psychotherapy in Tamang Shamanism," American Ethnologist, (1982), 21-46.

Plummer, Mark, "The Commonwealth Professional Interviews James Randi--Sceptic," Commonwealth Professional, Oct. 1980. pp. 16-18 & 43-44.

Rawlins, Dennis, "An Investigation of the Ancient Star Catalog," Publications of the Astronomical Society of the Pacific, 94 (1982), 359-373.

Rawlins, Dennis, "Eratosthenes' Geodesy Unraveled: Was There a High-Accuracy Hellenistic Astronomy?" Isis, 73 (1982), 259-265.

Rawlins, Dennis, "External Tides: Rigorous Computation for the Many-Body Case," Geophysical Journal of the Royal Astronomical Society, 68 (1982), 265-271.

Restak, Richard, "Islands of Genius," Science 82, May 1982, pp. 62-67.

Robbins, Thimas, and Dick Anthony, "Deprogramming, Brainwashing, and the Medicalization of Deviant Religious Groups," Social Problems, 29 (1982). 283-297.

Robinson, Daniel N., "Cerebral Plurality and the Unity of Self," American Psychologist, 37 (1982), 904-910.

Rodgers, Joann Ellison, "The Malleable Memory of Eyewitnesses," Science 82, June 1982, pp. 32-35.

Rosen, Edward, "In Defense of Tycho Brahe," Archives for History of Exact Sciences, 24, 4 (1981), 257-265.

Routley, Richard, "Alleged Problems in Attributing Beliefs, and Intentionality, to Animals," Inquiry, 24 (198), 385-417.

Ruse, Michael, "Creation Science: The Ulitmate Fraud," New Scientist, May 27, 1982, pp. 586-591.

Saklofske, D.H., I.W. Kelly, and D.W. McKerracher, "An Empirical Study of Personality and Astrological Factors," Journal of Psychology, 110 (1981), 275-280.

Schallenberg, Richard H., "The Anomalous Storage Battery: An American Lag in Early Electrical Engineering," Technology and Culture, 22, 4 (1981), 725-752.

Schmaus, Warren, "Fraud and Sloppiness in Science," Ethical & Policy Issues Perspectives on the Professions, 1, #3/4 (1981), 1-4.

Schwartz, Stephan A., and Rand De Mattei, "Psi-Q I Report," Omni, November 1982, pp. 24 & 160-161.

Schwartz, Stephan A., and Rand De Mattei, "Psi-Q Test II: Remote Viewing," Omni, Oct. 1982, pp. 136-138, 141-142, & 182.

Scott, Amoret and Christopher, "The Scoial History of the Quack," The Saturday Book, 25 (1965), 184-191.

Sebeok, Thomas A., "Dialogue about Signs with a Nobel Laureate," American Journal of Semiotics, 1, 3 (1982), 35- 57. (Re the Elberfeld horses.)

Sebeok, Thomas A., "The Not So Sedulous Ape," Times Literary Supplement, Sept. 10, 1982, p. 976.

Silberner, Joanne, "Cheating in the Labs," Science Digest, August 1982, pp. 38 & 40-41.

Simkins, Lawrence, "Biofeedback: Clinically Valid or Oversold?" Psychological Record, 32 (1982). 3-17.

Singh, Chandralkha, Hare Krishnas: A Study of the Deviant Career of Krishna Devotees. Doctoral dissertation (Sociology), New York University, 1981. University Microfilms #8127967. 390pp.

Smith, Bob, with S. Conradie and B. Spear, "Discussion: Levitation," Theoria to Theory, 13 (1979), 177-189.

Smith, Daniel S., and J. Roland Fleck, "Personality Correlates of Conventional and Unvonventional Glossolalis," Journal of Social Psychology, 114, (1981), 209-217.

Sparkes, John, "Can We Extend the Scope of Science," New Scientist, July 8, 1982, pp. 97-99.

Stebbins, Robert A., "Amateur and Professional Astronomers: A Study of Their Interrelationships," Urban Life, 10, 4 (1982), 433-454.

Stebbins, Robert, A., "Avocational Science: The Amateur Routine in Archaeology and Astronomy," International Journal of Comparative Sociology, 21, 1-2 (1980), 34-48.

Stebbins, Robert A., "Science Amators? Rewards and Costs in Amateur Astronomy and Archaeology," Journal of Leisure Research, 13, 4 (1981), 289-304.

Style, Alec, and Carolyn Briggs Style, "Once Upon A Paradigm: An Inquiry into the State of Medical Knowledge," Journal of Social and Biological Structure, 5 (1982), 203-212.

Swartz, Paul, and Lise Seginer, "Response to Body Rotation and Tendency to Mystical Experience," Perceptual and Motor Skills, 53 (1981), 683-688.

Tilley, Nicholas, "Rational Scientific Development," Knowledge: Creation, Diffusion, Utilization, 3, 4 (1982), 453-464.

Tilley, Nicholas, "The Logic of Laboratory Life," Sociology, 15, 1 (1981), 117-126.

Tipler, Frank J., "We Are Alone In Our Galaxy," New Scientist, Oct. 7, 1982, pp. 33-35.

Tobacyk, Jerome, "Halloween and Paranormal Belief," Psychological Reports, 50 (1982), 1006.

Tushnet, Mark V., "Deviant Science in Constitutional Law," Texas Law Review, 59, 5 (1981), 815-827.

Tyson, G.A., "Why People PERCEIVE Horoscopes as Being True: A review," Bulletin of the British Psychological Society, 35 (1982), 186-188.

Uennell, Peter, "The Invisible People," The Saturday Book, 17 (1957), 253-259.

Umiker-Sebeok, Jean, and Thomas A. Sebeok, "Rejoinder to the Rumbaughs," Anthropos, 77 (1982), 574-578.

Waid, William M., and M.T. Orne, "Cognitive, Social, and Personality Processes in the Physiological Detecti on of Deception," in L. Berkowitx (ed.,) Advances in Experimental Social Psychology, 14 (N.Y. Academic Press, 1981), 61-106.

Wallis, Roy, "The Social Construction of Charisma," Social Compass, 29, 1 (1982), 25-39.

Ward, David, "The Little Man Who Wasn't There: Encounters with the Supranormal," Fabula, 18, 3/4 (1977), 212-225.

Weinstein, Deena, "On the Importance of Fraud in Science," Ethical & Policy Issues Perspectives on the Professions, 1, #3/4 (1981), 4-6.

Weinberg, Howard I., and Robert S. Baron, "The Discredible Eyewitness," Personality and Social Psychology Bulletin, 8, 1 (1982), 60-67.

Westrum, Ron, "Social Intelligence about Hidden Events." Knowledge, Creation, Diffusion, Utilization, 3, #3 (1982), 381-400.

Wiley, John P., Jr., "Phenomena, Comment and Notes: For Scientists Who Want to Study Anomalous Phenomena, New Society Offers a Respectable Framework," Smithsonian, August 1982, pp.

Winkelman., Michael, "Magic: A Theoretical Reassessment," Current Anthropology, 23, 1 (1982), 37- 66.

Young, Robert, "Miracles and Credibility," Religious Studies, 16 (1980), 465-468.

Zelig, Mark, and William B. Beidleman, "The Investigative Use of Hyponosis: A Word of Caution," International Journal of Clinical and Experimental Hypnosis, 29, 4 (1981), 401- 412.

Zenker, S., et al., "The Sequence of Outcomes and ESP -- More Evidence for a Primacy Effect," Personality and Social Psychology Bulletin, 8, 2 (1982), 233-238.

Zuckerman, Miron, B.M. DeOayki, and R. Rosenthal, "Verbal and Nonverbal Communication of Deception," in L. Berkowitz (ed.), Advances in Experimental Social Psychology, 14 (N.Y.: Academic Press, 1981), 2-59.

Zusne, Leonard, and Barbara Allen, "Magnetic Sense in Humans," Perceptual and Motor Skills, 52 (1981), 910.

Zusne, Leonard, "Metaphysical Parallels of the Study of Values," Psychological Record, 15 (1965), 537-543.

Zusne, Leonard, "Teaching Anomalistic Psychology," Teaching of Psychology, 8, 2 (1981), 78-82.

Zusne, Leonard, "Contributions to the History of Psychology: XXXII. On Living With a Specter: The Story of Anomalistic Psychology," Perceptual and Motor Skills, 55 (1982), 683-694.

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CRYPTO-SCIENCE AND SOCIAL INTELLIGENCE ABOUT ANOMALIES

RON WESTRUM

The subject of this paper is the nature of research in what have been called the "cryptosciences."[1] Some years ago Irving Langmuir delivered a paper on "pathological science, the science of things that aren't there."[2] What I propose to treat here is a related topic, the science of "things that might be there." The cryptosciences deal with those objects and events whose existence is so far not acknowledged by science, but for which there is nonetheless other evidence, such as human testimony. UFOs, ghosts, bigfoot, the Loch Ness monsters, spontaneous human combustion, and numerous other hypothetical anomalies would fall within the domain of the cryptosciences. The aim of cryptoscience is to collect and analyze information about such anomalies so that (1) if they exist, they may be brought within the domain of science or (2) if they turn out not to exist, to explain why there nonetheless appears to be evidence for them.

The value of such activities seems obvious, yet those who engage in cryptoscience are hardly well received by the scientific community. That macroscopic anomalies unknown to science exist, most scientists would acknowledge; That cryptoscientists can aid in discovering them is another matter entirely. The zoologist interested in the existence of "bigfoot" is lumped together with astrologers and psychics under the label "pseudoscientist."[3] The UFO researcher (UFOlogist) is often considered to be a contemporary flat-earther and UFOlogy is generally ignored by scientists interested in exobiology and interstellar communication.[4] Indeed, the scientific community's general feeling about such "pseudoscience" is something like this:

> "If science is the competent study of what is real, then pseudoscience is the incompetent study of what is not real."

Or, more simply:

> "Pseudo-science is the study of the non-existent by the incompetent."

Some critics have gone even further and suggested that cryptoscientists are delusional.[5] While a good deal of thoughtful and elegant writing has been devoted to what pseudo-science really means,[6] I think it is important to confront this cruder (but operational) definition head-on. The issues of "competence" and "reality" frequently arise, and they need serious discussion. As a UFO field investigator and consultant to several UFO research organizations, I feel that I am in a particularly strategic position to assess certain aspects of cryptoscience, and I would like to share some of my insights with the reader.

First, though, I would like to clarify what I mean by "competence" and by "reality." By "competence" I mean proper use of that system of inquiry, experiment, and reasoning which best reflects the state of the art of science. In school most of us were taught that there is a single "scientific method" used in resolving scientific questions. Working scientists, however, understand, and ethnographic studies of research have confirmed, that exactly what constitutes "scientific method" is a flexible matter depending on the problem at hand and the resources which can be brought to bear on it.

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There appears to be a whole spectrum of procedures which are used by scientists to develop and test ideas about the way things are.[7] Some of the operations and some of the practitioners are perceived as being "better" than others.[8] The highest level of practice is called the "state of the art." It is this socially constructed standard of competence in research which defines what is really "scientific" and what is not.[9]

By "reality" I mean that state of affairs which a given social group agrees is the case. What is "real," then, is something agreed upon by a given group and therefore may be different for different groups. This reality is constructed through mutual interaction, and more advanced societies tend to have highly specialized roles (such as that of "scientist") whose incumbents are given special duties in the construction process.[10] The maintenance of this reality is just as important as its construction, and a number of social institutions are likely to take part in it. Every such reality necessarily brings with it a set of events which do not fit within the framework, and therefore are "unreal." How experiences with such "unreal" events are handled is obviously critical for the maintenance of the reality for that society.[11]

With these basic definitions out of the way, I would like to address my main subject, which is how cryptoscientific work is actually carried out, and in what ways it differs from ordinary scientific research. To make this discussion concrete, I would like to share some of my own experiences from UFO research, and then suggest some of the wider ramifications of these observations.

A Day in the Life of a UFOlogist

It is Tuesday, a day on which (fortunately) I have no teaching responsibilities, and therefore I can look for a hypnotist. The reason that I am looking for a hypnotist is that I suspect that a UFO case I am investigating is an "abduction" case. To understand the full complexity of abduction cases one has to be a UFOlogist! But, briefly, an abduction case is one in which someone "remembers" under hypnosis that he or she was kidnapped by beings from a UFO. How would one know that such persons should be placed under hypnosis in the first place? Basically, UFOlogists have learned from experience that when "missing time"---say a period of two hours---is associated with a UFO experience, what the subjects will usually "recall" under hypnosis is that they had been abducted by UFO beings. Actually the case I am investigating does not have any missing time associated with it. But there are some other puzzling aspects which my UFOlogical intuition tells me might well be due to an abduction experience which has been repressed.

If this sounds bizarre, imagine how difficult it is to try to interest a clinical hypnotist, who already has professional credibility problems, in working on this case, perhaps for free. In this case I talk to two people, one a department head, the other a social worker, both of whom turn me down but suggest other names to try. While I speak with the social worker, he mentions in passing that "we would probably have to get medical clearance on some of these cases. After all, we are probably dealing with psychotic or near-psychotic people." I am not sure exactly what he means by this statement, but since I am eager to get his cooperation, I do not challenge it. But I get the names he suggests that I call, and I leave.

There are several observations to make about this kind of encounter which, as a UFOlogist, I am likely to have. I need the cooperation of other specialists. While I could carry out the hypnosis myself, I would prefer, and it is much more sensible, to have the job done by a professional; someone, in other words, who is competent at the state of the art level. Yet I lack a basic resource---legitimacy---in securing such cooperation. If I do not find a hynotist, I will not be able to test my intuitions about this case and will thus have to substitute supposition for experiment. Hence I am keenly aware that while from my point of view the experiment is important, from the point of view of established psychology, I am dealing with a species of psychopathology. Intellectually, it may be reassurring to know that resistance to new concept-systems is a routine process in science,[12] and that I share similar problems with Galileo and Semmelweiss, but I would prefer to know the result of the proposed experiment.

In considering such UFO abduction cases in general, the question of the researcher's competence is inescapable. I know several other UFOlogists who have investigated such cases, and I am aware of the problems that surround the use of hypnosis as a research instrument.[13] In some cases I think the investigation has been competent; in other cases I have strong reason to believe that it has not. My opinions about the competence and incompetence of my fellow investigators are gained in ways not dissimilar from the ways in which ordinary physical and biological scientists form assessments of each others' competence.[14] I do know, however, that in most cases the investigator is not a hypnotist, and I hence must seek outside help. Again, however, this is no different than in ordinary science, in which persons from different specialties work together in research.

So am I, as a UFOlogist, doing science? In a strict sense, yes. The logic of what I am doing is (I would argue) within the basic logic of science. But in a practical sense, I am not doing science, since the resources which would make my research competent are simply not present. Good research takes proper training, it takes time, and it takes money. I am trained as a sociologist, and yet the nature of my work frequently requires judgments that a psychologist should make, or in other cases, a physicist or chemist. And then there is the matter of time. Investigation of cases is extremely time-consuming, to say nothing of writing the cases up. The relevant time-frame for investigation of an interesting UFO case is probably about the same as that for a homicide case. Such investigations should be pursued on a full-time basis; yet ordinarily they are pursued after-hours, on nights, weekends, and vacations. And finally there is the problem of money. A good physical trace case (and there are many) should probably cost about $25,000 to investigate. In actual practice, it is rare that over $100 is spent on such a case. Because cryptoscience is not perceived as a legitimate scientific activity, it is not properly staffed and funded. This makes a large impact on the value of the results obtained.

There are other concerns. I am a social scientist, with a career hardly likely to be advanced by my interest in UFOs. The time spent on UFOlogy must compete with time given to other projects, many of which are more respectable and therefore more beneficial to my reputation than my UFO work. I ask myself if I am not wasting my time chasing phantoms. What if, as is certainly possible, there are no "real" UFOs, and all reports are the result of misidentification and fraud? I think wistfully of what my career might be if I had devoted it to, say, research on complex orgainizations.

Yet at the same time, I feel pangs of guilt. Perhaps I should be spending more time on UFO research. What if there really are manifestations of alien technology in our atmosphere? Perhaps someone will come to me some day and say "Westrum how could you have let all this be ignored? You knew something was going on; why didn't you make a fuss about it?"[15] No one likes to be considered a coward. So I am torn between spending more time on UFO research and spending less.

I think of the average UFO investigator, who is not a professor, and who cannot command even the modest resources I have. I know time, money, and marital peace are often sacrificed by the serious investigator, the same investigator who may get chopped up on the local radio talk show the next week by an astronomer from the university; or whom professionals like the social worker I mentioned are not required to treat with the same courtesy with which they treat me. Being a UFOlogist is not good for family life, it is not good for careers, and it is definitely not good for one's piece of mind.[16]

Why must UFO research be a spare-time, avocational activity, and therefore, in a practical sense, scientifically incompetent? Fundamentally, the nature of such work is determined by society's attitude (and especially that of science) that UFOs do not exist, and therefore time spent on them is time wasted. Scientists who would otherwise be interested in doing research on UFOs are discouraged by the aura of ridicule which surrounds the subject.[17] Even if such interest should become overt, the funds for doing research may have to be "borrowed" from other projects, a risky business at best.[18] It is interesting to note which scientists become active in UFO research. Generally they are those whose careers have already peaked or those who are willing to risk a "long shot" because they have less to lose if it fails. The ambitious "comers" avoid UFO research like the plague. If interested in extraterrestrial intelligence, they will safely pursue their interest in the manner approved by the tribe: by building or operating radio-telescopes to detect signals from distant galaxies. The lack of interest in UFOs by exobiologists and CETI-oriented astronomers is a fascinating example of the social factors directing scientific research.

So the nature of the conditions under which cryptoscientific work is done tends to work against it being done at the state of the art level in science. Perhaps, given the nature of science, we should expect cryptoscientific work to have a low priority and therefore be given few resources. Yet it is not a question of science simply ignoring cryptoscientific work. On the contrary, the label "pseudo-science" should attract our attention to the facts that such work is actually taboo and that those who practice it are viewed as pariahs by the scientific community. What is it about the anomalous event which makes its study a taboo activity? To answer this we must understand how our society constructs and protects its sense of reality.

Anomalies and the Social Sense of Reality

In most societies there is some kind of official consensus about what is real and what is not. This official consensus is a basis for the society's administrative and intellectual activities and also plays a certain role in the individual's sense of security.[19] Often there is a special corps of people, scientists, philosophers, or priests, whose duty it is to construct and maintain this reality. In our society, of course, this duty is largely in the hands of the scientific community. The consensus of the scientific community is very important to our own society's sense of reality, although it is not decisive on all matters.

This consensus has a strong tendency toward closure, in what John Dewey called "the quest for certainty."[20] While current knowledge is the basis for further research, and for scientific progress, it can also be the basis for dogma; and progress must constantly fight against a premature closure of the system against new and discrepant principles. Present knowledge, particularly knowledge sanctified by constant usage---and therefore familiarity---can thus be a major barrier to acquisition of new knowledge. It is difficult to improve on the formulation of this problem by Ludwik Fleck many years ago. Discussing the development of ideas about syphillis, he noted that any accepted system of ideas tends to display certain features:

(1) a contradiction to the system appears unthinkable.
(2) what does not fit into the system remains unseen.
(3) alternatively, if it is noticed, either it is kept secret, or
(4) laborious efforts are made to explain an exception in terms that do not contradict the system.
(5) despite the legitimate claims of contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them substance.[21]

The sociology of anomalous events gives ample testimony to the existence of all these intellectual and social phenomena. I did not come across Fleck's writings until quite recently, but it is astonishing how accurate this formulation is in relation to events as diverse as meteorites, UFOs, and the battered child syndrome.[22] A more judicious and balanced assessment of Fleck's view must await further comparative study, but certainly finding examples to illustrate his arguments is not difficult.

The point, then, is that any strong sense of reality shared by a social group is likely to demonstrate an equally strong resistance to discrepant information. Persons who take an interest in developing or studying these discrepant phenomena will then be seen by the larger group as misguided or insane, no matter how "scientifically" this interest is pursued. This means that, in some sense, whether one studies "pseudo-scientific" topics with state-of-the-art methods or not, one cannot be doing "science," since, as by the earlier definition I noted, science is the study of real things. What is not there cannot be studied, much less scientifically.

It is this definition of science as the study of the "real" which is responsible for many of the intolerant and often questionable attacks on "pseudoscience." The methods used to undermine the public credibility of pseudo-science are often extreme, but such methods (as in the Velikovsky affair) are felt to be justified by the threat that pseudo-science poses to the current public consensus on what is real. Anything that poses the danger of erasing the boundary between standard, formally recognized "reality" and other possibilities is violently resisted, not so much because it is dangerous in itself, but because of its subversive implications. In some cases the "nonsense" is lampooned by skillful wits; in others, solemn and even collective pronouncements may be used to banish the offending ideas.[23] Frequently such tasks are undertaken not by scientists themselves, but by popular science writers who act as the publicists and "secular arm" of the scientific community. The effect, however, is usually the same: anything not within the pale is treated not with amused tolerance or critical reasoning but with hatred and ridicule.[24]

In this way, some ideas become seen as intrinsically "wrong" or "crazy." Experiences or even experiments which evoke them are accordingly suppressed, not only by society in general, but also by those who make the anomalous observations.[25] These observations may then become "hidden events" in terms of social intelligence. I have devoted several papers to studies of such events, and I have found that not only will society fail to hear of these events through ordinary social intelligence processes, but special efforts to find out about them may prove futile unless such efforts are carefully designed.[26] If seemingly indisputable evidence does surface, it may cause intense psychological problems for the discoverer. J.L.B. Smith, who first discovered and described a recently living Coelacanth, found he had difficulty believing in his own experiences. In his book Old Fourlegs, he relates:

> I was quite irrationally still fearful, because although my intellect was completely satisfied with the irrefutable evidence my eyes had seen, completely satisfied that the fish was indeed a true Coelacanth, it seemed too impossible, too fantastic, that this could have happened. A coelacanth. Alive! Every night I had a nightmare, dreaming that I had found a Coelacanth, and it was confused and troublesome because I realized it was impossible. Then I would wake and ponder on this curious dream until suddenly I would realize that it wasn't a dream, but true. I had that happen to me hundreds of nights in the years that followed. Sometimes it got all mixed up, for I would dream I had dreamt it, and when I did wake up it took a long time to sort it all out. This sounds fantastic, and it was.[27]

Smith was courageous, and he was lucky because he did have irrefutable evidence of his claims. I leave you to imagine his fate as an ichthyologist if he had only observed the fish, and had not had an actual specimen to back him up. Actually he was fairly roughly treated as it was; long-time friends avoided him on the street and several people questioned his sanity.[28]

If we switch from the individual level to that of the community, even more interesting phenomena present themselves. Since anomalous events (or stimuli which give rise to the impression that such events have occurred) may be relatively circumscribed in time and space, it is possible that only a small group of people may perceive them. Observations of events of an "unacceptable" nature can then pose a problem, not only to the individual but for the community as a whole. Consider, for instance, the following remarks which greeted a legally certified observation of a meteorite fall witnessed by an entire community in 1790:

> If the readers have already had occasion to deplore the error of some individuals, how much more will they be appalled today seeing a whole municipality attest to, consecrate, by a legal protocol in good form, these same popular sensations, which can only excite the

> pity, not only of physicists, but of all reasonable people...What can we add here to such a affadavit? All the reflections which it suggests will present themselves to the philosophical reader in reading this authentic attestation of an obviously wrong fact, of a phenomenon physically impossible.[29]

I wish to draw attention to the phrase "obviously wrong fact," since on this point many 20th century attitudes are not very far removed from 18th century ones.

It is worth observing that the features of an observation that make it "impossible" and therefore unreal are often implicit rather than spelled out in detail. Very frequently the assertion that something is "impossible" rests either upon the unfamiliarity of the event in question or upon a rather hastily put-together proof which will not stand detailed scrutiny.[30] Often also the reaction to an event as "just crazy" is more emotional than intellectual, and suggestions to discuss the event's possibility will be rejected as a waste of time. Use of the phrase "nonsense" also suggests that the reality of the alleged event is undiscussable due to its violent contradiction of the ordinary criteria of reality. Such refusals can sometimes be amusingly juxtaposed with later recognition that some of the events so treated were, after all, real enough.

But let me return to the community and the possibility that its own lived reality may be different from that of the society as a whole. As a contemporary case study, let us take the observation of UFOs on the Yakima Indian Reservation in Washington State.[31] Although published accounts of these observations are only beginning to appear, they have some sociological features which are extremely interesting. This reservation is located in the south-central area of Washington and is several hundred square miles in size. A large variety of airborne and other anomalous events have been observed in this area by fire lookouts, who evidently are quite familiar with most of the usual phenomena in it, and by the Yakima Indians. The extent of the phenomena which were taking place in the area only became known, however, when the senior Fire Control Officer photographed one of the events which was visible for at least ten minutes.[32]

Previous to this officer's own experience, he took those reports which came in "with the proverbial grain of salt and some question." When it became known that his attitude had changed, however, "observation and reporting of UFO activity became a part of almost everyone's daily work pattern." From 1972-1980, furthermore, all reports which were made received at least a cursory investigation, the results of which have now reached the Center for UFO Studies in Evanston, Illinois. But in spite of the voluminous documentation of these events (which fills a three-inch-thick looseleaf binder), at least in one sense, they are not "real"; they simply cannot be acceptable to society at large unless they can all be explained as being frauds, optical illusions, or whatever. Furthermore, if these events, which involve an entire community, have such difficulty in coming to light (the documentation would probably not have been done had the Center for UFO Studies not suggested that records be kept), what of the experiences of isolated individuals or small groups? I am reminded of a prudent cleric in the 18th century who received an affidavit of a meteorite fall, as well as one of the stones. He stated, some thirty-nine years after finding out about the event:

> Since that time, the Prince of San Severo and the Marquis Mauri often tried to persuade me to publish all the details; but other friends dissuaded me. They warned me that the savants and the half-savants (even more to be feared) would attack me on this subject or pretend to be gracious to me while treating me only with incredulity. These reasons decided me in favor of silence.[33]

What we learn from the study of social intelligence about anomalous events is, as Fleck noted, that the anomalous, if noticed, is kept secret. And it remains secret precisely because there are so many barriers to its emergence.

In practice, this means that the deviant reality does not get to confront the standard reality. This means not only that our standard reality may be incomplete but also that the deviant reality may appear more deviant than it is. For instance, in the course of UFO investigation, I have come across cases that would seem to violate standard reality and also cases which could be easily included in it. But the reporting of the genuinely deviant and the spuriously deviant cases alike seldom takes place. Hence the standard reality is not forced to grapple with contradictory evidence, but also (in other cases) people may inaccurately believe that they have had a deviant experience. (I will never forget the housewife whose "UFO" was the constellation Orion!) The deviant experience is thus forced into a separate world, where for the most part it can neither change nor be changed by standard reality. This separation impoverishes both perspectives and makes each less than it could be.[34] Hence what is not "real" according to standard reality remains foreign territory, and those who explore it can be treated as pariahs.

Conclusion

These observations call for an extended discussion, but to bring this paper to a close, let me make only two observations.

The first is that one can hardly expect marginal areas in science to be pursued in the same way and by the same persons as main-stream areas. The enormous pressure on scientists to pick projects whose success is assured means that long shots will seldom be attempted.[35] Hence the science done in "fringe" areas will usually be done with sub-standard resources and by sub-standard practitioners. If competent individuals enter these areas, they will be tarred with the same brush as the incompetent, and their careers may suffer.

Second, observations of a cryptoscientific nature will be extremely difficult to get since making such observations or even taking an interest in them is stigmatized. This means that one should not infer that infrequent reports mean infrequent observations. If social intelligence about such events is to be improved, then conscious and well-conceived efforts must be made to improve reporting channels. This has happened for some kinds of anomalous events (meteorites) but usually only after they had begun to seem less anomalous. Society seems to be able to find out only about what it is willing to accept.

Let me close with a speculation. I suspect that a society and a scientific community with a greater openness and tolerance would find less use for the epithet "pseudo-science." If the ordinary citizen were encouraged to understand and to participate in the process of science, much of what is now called "pseudo-science" would be replaced by the term "amateur science." The crypto-sciences, because of their great need for non-scientist observers, could be an excellent place to begin. The efforts of H.H. Nininger to educate the American people regarding meteorites is a worthy example and has been amply repaid.[36] I cannot help but feel that the issue here is larger than just what is to be included under the heading "science." The issue is fundamentally one of democracy as well. Wallace Stevens once wrote that "A very great order is a disorder." I wonder whether this pithy observation might not be advantageously applied to the understanding of the science/pseudo-science demarcation problem.

REFERENCES

1. Marcello Truzzi, "Parameters of the Paranormal," The Zetetic, Vol. 1, #2 (Spring/Summer 1977), pp. 4-8.

2. Irving Langmuir, "Pathological Science," General Electric Research and Development Center Report 86-C-035 (April 1968). Also see: Ray Hyman, "Pathological Science: Towards a Proper Diagnosis and Remedy," Zetetic Scholar, #6 (1980), 31-39, plus Commentaries and reply by Hyman in issues #6 and 7.

3. See, e.g. Douglas Hofstadter, "About Two Kinds of Inquiry: 'National Enquirer' and 'The Skeptical Inquirer,'" Scientific American (February 1982), pp. 18-26. As in the case of many articles on the Committee for Scientific Investigation of Claims of the Paranormal (CSICOP), little effort is made to separate scientific research on paranormal claims from the sensational approach of popular magazines. This distinction is made occasionally, however, e.g.: Paul Kurtz, "From the Chairman," The Skeptical Inquirer (Spring 1981), pp. 2-4.

   Richard Greenwell, who carried out a survey on physical anthropologists' opinions on Bigfoot, ("Attitudes of Physical Anthropologists Toward Reports of Bigfoot and Nessie," Current Anthropology, Vol. 22 #1, February 1981, pp. 79-80), tells me that one of the respondents, a well-known paleontologist, found it hard to believe that John Napier, a respected authority on primates, had written a book on Bigfoot; he felt this was "out of character" for his colleague. See also Ron Westrum, "Results of a Questionnaire on the Sasquatch," in Roderick Sprague and Grover Krantz, Editors, The Scientist Looks at the Sasquatch (Moscow, Idaho: University Press of Idaho, 2nd Edition, 1979), pp. 160-165. This study suggests that academics favorable to the existence of Sasquatch (Bigfoot) are not well received by their colleagues, while those undecided or negative were better received. Napier himself, not included in the study, states that (scientists') "reluctance to become actively involved in such matters has no sinister overtones, reflects no fear of ridicule from their peers, but expresses a wholly practical attitude of mind..." Bigfoot: The Yeti and Sasquatch in Myth and Reality (London: Jonathan Cape, 1972), p. 15. This assertion is, to say the least, surprising. A similar assertion regarding UFOs is made by Gerard Kuiper in University of Colorado, Scientific Study of Unidentified Flying Objects (New York: Bantam, 1969), at 839-840:

   > "I should correct a statement that has been made that scientists have shied away from UFO reports for fear of ridicule. As a

practicing scientist, I want to state categorically that this is nonsense..."

Kuiper's assertion, however, would seem to be rebutted by some statements by J. Allen Hynek, whose public posture regarding UFO research changed in the late 1960's from neutral or negative to positive. Reminiscing over his apparent failure to "speak out" in the middle 1950's, he stated:

> "But at that time I was either an assistant or an associate professor at Ohio State, and Ohio State is not Harvard, nor the University of Chicago; frankly, I was much more concerned with my own career at the time.
>
> I knew if I really came on waving my arms I would have been declared a nut and my services would no longer have been required. I don't think I would have done any good. Certainly any future effectiveness would have been shot down. I never would have been asked by Whipple to take charge of the satellite tracking program, I never would have gotten to Northwestern because I had a reputation in satellite tracking, with Sputnik and so forth. My temperament is to play the waiting game; I always have, maybe it's my Czech background. (From J.A. Hynek and J. Vallee, The Edge of Reality: A Progress Report on UFOs, Chicago: Henry Regnery, 1975, p. 193)

It is hard to believe that testimony so damaging could be made up, and it should be taken seriously. How many other potentially interested scientists would act as Hynek did, however, must remain unknown. Yet if Hynek's statements about career mobility are correct, then it would seem that young researchers who openly express such interests are committing professional suicide. It certainly appears to be the case that the meteorologist James McDonald's testimony on the Supersonic Transport (SST) was tainted by his association with UFOs.

McDonald appeared before the U.S. House Committee on Appropriations in March 1971. During his testimony, it was pointed out repeatedly that McDonald was interested in and believed in the existence of UFOs. He was laughed at by some of the congressmen during his testimony, in spite of his otherwise excellent reputation as a meteorologist. One congressman stated:

> "A man who comes here and tells me that the SST flying in the stratosphere is going to cause thousands of skin cancers has to back up his theory that there are little green men flying around in the sky. I think this is very important." (Quoted in David Jacobs, The UFO Controversy in America, New York: New American Library, 1976, p. 232)

How much this event contributed to McDonald's suicide three months later is unknown. It is worth considering, however, how many scientists might be willing to face the same kind of ridicule. My impression is, not many.

4. See the comments on James McDonald by George Gaylord Simpson "Added Comments on 'The Nonprevalence of Humanoids,'" in C. Sagan, Editor, Communication with Extraterrestrial Intelligence (Cambridge, Mass.: MIT Press, 1973), pp. 362-364.

5. Gary Posner, "Faulty Sense of Reality" (letter), The Skeptical Inquirer, Vol. 3, #2 (Winter 1978), p. 79.

6. E.g. Helga Nowotny and Hilary Rose, Editors, Counter-Movements in the Sciences (Dordrecht: D. Reidel, 1979); Marsha P. Hanen, M.J. Osler, R.G. Weyant, Editors, Science, Pseudo-Science, and Society (Waterloo, Ontario: Wilrid Laurier University Press, 1980); Seymour Mauskopf, Editor, The Reception of Unconventional Science (Washington: American Association for the Advancement of Science, 1979); Roy Wallis, Editor, "On the Margins of Science," Sociological Review Monographs, Vol. 27 (entire issue, 1979).

7. Cf. Ron Westrum, Book Review of B. Latour and S. Woolgar, Laboratory Life, in Knowledge: Creation, Diffusion, Utilization, Vol. 3, #3 (1982), pp. 437-439.

8. J.R. Ravetz, "Quality Control in Science," pp. 273-288 in his Scientific Knowledge and Its Social Problems (Harmondsworth: Penguin, 1973); Michael Polanyi, "The Republic of Science: Its Political and Economic Theory," Minerva, Vol. I, #1 (August 1962), pp. 54-73; John Ziman, Reliable Knowledge (Cambridge: Cambridge University Press, 1978), especially chapter 6. Social scientists who have not had a scientific background are less sensitive to the question of quality control than are philosophers of science who have such a background. For most practicing scientists, the question is not whether something is scientific or not, but whether it is "good" science.

9. Cf. Michael Polanyi on some of the "tacit components" of scientific practice: "The Growth of Science in Society," Minerva, Vol. V, #4 (Summer 1967), pp. 533-545.

10. See Harry M. Collins, Editor, special number of Social Studies of Science, Vol. 11 #1, February 1981, especially the article by Andrew Pickering; also Bruno Latour and Steve Woolgar, Laboratory Life (Beverly Hills: Sage, 1979), pp. 151-187.

11. Ray Fowler, The Andreasson Affair (Englewood Cliffs, New Jersey: Prentice-Hill, 1979); John Fuller, The Interrupted Journey (New York: Dell, 1967); Ann Druffel, and D. Scott Rogo, The Tujunga Canyon Contacts (Englewood Cliffs, New Jersey: Prentice-Hall, 1980); Budd Hopkins, Missing Time: A Documented Study of UFO Abductions (New York: Richard Marek, 1981). These four accounts provide documentation of a psychological phenomenon which, even if it has no connection with beings from outer space, ought to be carefully studied. The last, by Budd Hopkins, provides a useful overall introduction to the subject.

12. Cf. Ludwik Fleck, Genesis and Development of a Scientific Fact (Chicago: University of Chicago Press, 1979), at 27. Also see Bernard Barber, "Resistance by Scientists to Scientific Discovery, "Science, Vol. 134 #3479 (Sept. 1, 1961), pp. 596-602.

13. Richard Hall, "UFO Abduction Cases: A Mini-Symposium," MUFON UFO Journal (August 1980), pp. 9-12. This article contains a useful bibliography.

14. See Latour and Woolgar references, cited in Fn. 10, loc. cit.

15. J.A. Hynek was actually reproached by James McDonald for not being an earlier advocate of the reality of UFO sightings. See the Hynek reference in Fn. 3, loc. cit. It might be remarked that perhaps cowardice in science is not rare and only awaits a suitable study. Carl Friedrich Gauss was reluctant to publish his work on non-Eulidean geometry because he "feared the clamor of the Boeotians, a figurative reference to a dull-witted Greek tribe." (Morris Kline, Mathematical Thought from Ancient to Modern Times, New York: Oxford University Press, 1972, p. 871)

16. See Ray E. Fowler, UFOs: Interplanetary Visitors (Englewood Cliffs, New Jersey: Prentice-Hall, 1974) for the constant conflict between UFO investigation and family activities on the part of a serious and intelligent investigator. John Keel's comments in UFOs: Operation Trojan Horse (New York: Putnam's, 1970), pp. 291-307, are more extreme, but Keel could hardly be considered a representative UFOlogist.

17. See the remarks in Footnote 3.

18. This "borrowing" for purposes of otherwise unfundable innovation has been successful in some cases (Cf. Richard A. Muller, "Innovation and Scientific Funding," Science, Vol. 209, August 1980, pp. 880-883), but it is dangerous. When a UFO critic found that McDonald was bootlegging Office of Naval Research funds for his UFO researches, the critic (Philip Klass) did his best to get McDonald in trouble with ONR. Although it is not clear that Klass's action had the effect of terminating McDonald's contract, this is certainly one plausible interpretation. See Paul McCarthy, Politicking and Paradigm Shifting: James McDonald and the UFO Case Study (Doctoral Dissertation, Political Science, University of Hawaii, December 1975), pp.194-222.

19. Zevedei Barbu, Problems of Historical Psychology (New York: Grove Press, 1960), pp. 26-31.

20. John Dewey, The Quest for Certainty: A Study of the Relation of Knowledge and Action (New York: Minton, Balch & Co., 1929).

21. Ludwik Fleck, Genesis and Development of a Scientific Fact (Chicago: University of Chicago Press, 1979), p. 27.

22. Ron Westrum, "Social Intelligence About Hidden Events," Knowledge: Creation, Diffusion, Utilization, Vol. 3 #3 (March 1982), pp.381-400.

23. As examples of former, consider the general tenor of the articles in The Skeptical Inquirer. As an example of the latter, consider "Objections to Astrology: A Statement by 186 Leading Scientists," The Humanist, Vol. 35, #5 (September-October 1975), pp. 4-6. There is much evidence that "Objections" was largely signed by scientific "experts" who were far from current on recent studies in the astrobiological field. See the exchanges between myself and

P Kurtz & R.Nisbet in The Zetetic Vol. I, #1 (1976, pp. 34-52) and Vol. I #2 (1977, pp. 107-117).

24. There seems to be a sentiment on the part of some science writers that anything goes when dealing with certain claims of paranormal events. Martin Gardner states in the Introduction to his Science, Good, Bad and Bogus (Buffalo, NY: Prometheus Press, 1981, p. xv): "For these reasons, when writing about extreme eccentricities of science, I have adopted H.L. Mencken's sage advice that one horse-laugh is worth ten thousand syllogisms." One case which deserves such treatment, Gardner feels, is the "Mars Effect" work of Michel Gauguelin, based on painstaking research and replicated by independent and hostile critics. On the latter, see various articles in Zetetic Scholar, Vol. 9 (1982), pp. 33-83.

25. Ron Westrum, "Social Intelligence About Anomalies: The Case of UFOs," Social Studies of Science, Vol. 7 (1977), pp. 271-302; also "Sasquatch and Scientists: Reporting Scientific Anomalies," pp. 27-36 in M. Halpin and M.M. Ames, Manlike Monsters on Trial: Early Records and Modern Evidence (Vancouver: University of British Columbia Press, 1980); "UFO Reporting Dynamics," pp. 147-163 in Richard Haines, Editor, UFO Phenomena and the Behavioral Scientist (Metuchen, New Jersey: Scarecrow Press, 1979).

26. See Westrum article cited in footnote 22.

27. J.L.B. Smith, Old Fourlegs: The Story of the Coelacanth (London, Longmans, Green, 1957), pp. 47-48.

28. Ibid. and personal communication from Bernard Heuvelmans.

29. Pierre Bertholon, "Observation d'un Globe de Feu," Journal des Sciences Utiles, Vol. 4, #24 (1791) p. 226. Emphasis in original.

30. As an example of a hastily put together proof, see the article by William Markowitz, "The Physics and Metaphysics of Unidentified Flying Objects," Science, Vol. 157 (15 September 1967), pp. 1274-1279. Among the many careless arguments in this article, the author asserts that the take-off of advanced interstellar spacecraft would resemble a Saturn 5 launch. Pacific islanders unfamiliar with 20th century technology might with equal justice assert the impossibility of a Boeing 747, since it lacks sails or paddles.

31. Bill Vogel, "UFOs on the Yakima Indian Reservation," CUFOS Bulletin (Spring 1981), pp. 1, 6, 7; Greg Long, "Yakima Indian Reservation Sightings," MUFON UFO Journal, #166 (December 1981), pp. 3-7; Idem., "UFO 'Menagerie' on Yakima Indian Reservations," MUFON UFO Journal, #168 (February 1982), pp. 8-12; personal communication from J. Allen Hynek and Bill Vogel.

32. Bill Vogel, ibid., p. 1.

33. Domenico Tata, Memoria sulla Pioggia di Pietre (Naples: Nobile and Co., 1794), at 23.

34. Cf. Michel Foucault, Madness and Civilization (New York: New American Library, 1967), pp. ix-xi.

35. Richard A. Muller, cited in Footnote 18; consider also the concept of "normal science" of Thomas Kuhn, in his The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 2nd Edition 1970), pp. 23-34. Cf., John Ziman's observation that:

> "The average Apollonian Scientist tries to make a modest living by solving a succession of puzzles within a specialized disciplinary matrix. The questions to be answered are not necessarily trivial, but they are chosen because they are expected to give publishable answers within a reasonably short time...The majority of scientific careers give the impression that 'minimal risk' is their underlying policy."

(in "What Are the Options: Social Determinants of Personal Research Plans," presented at the Conference on Scientific Establishments and Hierarchies, 5 July 1980, Oxford, England, p. 15) Cf. also his observations in Chapter 1 of his forthcoming Puzzles, Problems, and Enigmas, on "enigmas."

36. The story of these efforts is chronicled in H.H. Nininger, Find a Falling Star (New York: Paul S. Eriksson, 1972). Cf. Ron Westrum, cited in Footnote #22.

CRITICAL COMMENTARIES:

COMMENTS BY ROY P. MACKAL:

I read the article "Crypto-Science and Social Intelligence About Anomalies" by Ron Westrum with great interest, and then reread it several times. The reason for rereading the article was not that it was unclear, or that I had not followed the arguments and implications. I went over the paper a number of times in an attempt to find something with which I might disagree or amplify. However, I was unable to do so, and find myself in agreement with Westrum on practically every point. Having served as the Scientific Director of the Loch Ness Investigation Bureau (1965-1975), and being the co-founder of the International Society of Cryptozoology, I can attest from personal experience, to the validity of Westrum's analysis, and to his perceptive understanding of the more subtle aspects.

I could provide concrete personal examples to illustate the situations so well presented, and discussed. Perhaps a few healthy open-minded skeptics of good will will read this article and as a result, take a more objective, not necessarily positive, position regarding the investigation of anomalous events.

COMMENTS BY ROBERT ROSENTHAL:

"Controversial Science, Crypto Science, and Taboo Science"

Ron Westrum's wonderfully candid article was most informative and most stimulating. It was informative in the insight it gave into the life of the cryptoscientist. It was stimulating in that it suggested certain parallels between cryptoscience and taboo science. Thus, while the scientific study of religion is not especially "crypto" (i.e., no one doubts that religion exists) it is to some degree taboo (Douglas, 1963).

Some 20 years ago Norman Farberow (1963) edited a volume on taboo science in which the topics described as taboo were relatively non-crypto. Nevertheless, investigators working in these areas had many experiences similar to those encountered by cryptoscientists. It would seem to be a useful step to disentangle the reactions experienced by cryptoscientists because they (1) study things that may not exist (cryptoness) or because (2) they study things they "should not study" (tabooness), or both. It should aid social scientists of science in defining their independent variables if these two dimensions can be separated. This separation is not easy because crypto topics tend also to be taboo. The table below shows nine research areas arranged into three levels of tabooness and three levels of cryptoness.

	Levels of Tabooness		
Cryptoness	Low	Medium	High
High	Language in Apes	Psychokinesis	UFO Abductions
Medium	Telepathy	Precognition	Reincarnation
Low	Self-Fulfilling Prophecy	Hypnosis	Religion

It should be noted that all of the entries of the table above are controversial topics but that they nevertheless vary in degree of cryptoness and degree of tabooness.

The particular placement of the topics above into their row and column is very provisional, designed to be illustrative rather than definitive. The placement was made on the basis of rankings of degree of cryptoness and degree of tabooness of 20 research topics by three informants. These informants were well-educated (beyond the AB degree) but were not trained as scientists. The three reliabilities of rankings of cryptoness were .40, .54, and .68; for tabooness they were .44, .58, and .73. The table below shows the 20 research areas arranged from most to least crypto, i.e., most to least unlikely to exist. After each topic the rank of tabooness is given where 1 means it is most taboo for a scientist to study that area and 20 means it is least taboo for a scientist to study.

	Topic	Rank of Tabooness
	Abduction by Spacecraft	2.5
	Psychokinesis	9
	Astrology	4
	Ghosts	1
	Loch Ness Monster	2.5
	Bigfoot	5
	Language in Apes	14
	Reincarnation	7
	Precognition	12
*	UFO's	8
*	Racial Differences in IQ	11
	Mental Telepathy	15
	Graphology	10
	Hypnosis	13
*	Homosexuality	18
*	Self-Fulfilling Prophecy	16
	Religion	6
	Suicide	17
	Heterosexual Behavior	20
	Death	19

*Tied ranks

Note: Topics are ranked from most to least "crypto" i.e., unlikely to exist.

For the 20 topics listed the rank correlation between cryptoness and tabooness was quite substantial (.78). Investigators of the social science of science would, therefore, have to work hard to find high crypto - low taboo and low crypto - high taboo topics, but the tables above suggest that such combinations may exist. Careful study of the lives and experiences of investigators working in those two types of areas would illuminate the antecedents and consequences of working in areas that are crypto vs those that are taboo. In addition, of course, we would want to apply our methods of inquiry to the two types of combinations studied more typically: the high crypto - high taboo and the low crypto - low taboo.

References

Douglas, W. Religion. In N. L. Farberow (Ed.), Taboo topics, New York: Atherton, 1963.

Farberow, N. L. (Ed.) Taboo topics. New York: Atherton, 1963.

COMMENTS BY H.J. EYSENCK:

The article by Ron Westrum on crypto-science presents a very true-to-life picture of what the researcher in these areas (or the writer about these areas) has to expect. From my own experience I can testify to the accuracy of his observations, and the truth of his statements. The premature crystallization of spurious orthodoxies, which is so typical of the scientific establishment, particularly in the social sciences, threatens and tends to overwhelm the potential rebel who dares to investigate, with however much scientific care, such areas as astrology, the paranormal, etc. A secure position, and a reputation for rigorous research, may save the investigator from the worst kind of attack, but he will always find his work and his words misrepresented, ridiculed, and abused; the only alternative to this is complete neglect.

However, I think Westrum is wrong in imagining that this fate is reserved entirely for advocates of what he calls the "crypto-sciences." Orthodoxy is far more far-reaching, and embraces areas which no one would consider to be "crypto-sciences" in any sense of the term. I think what Westrum has to say applies to a much broader spectrum of research investigations and beliefs, and might be generalised to apply to anything which goes counter to the Zeitgeist. I first came across this term in Boring's famous book on the History of Experimental Psychology, where he attributed a considerable degree of influence to this rather mysterious concept, which seemed to float above the doings and writings of experimentalists and theoreticians, and at first I was rather sceptical about its reality. A long life spent in battling against the Zeitgeist has taught me better.

As an example of the influence of the Zeitgeist, consider the conviction, apparent in American psychological, anthropological and sociological writings over the past 50 years, that individual differences in behavior, intelligence, personality, etc. are due entirely to environmental causes. This belief goes counter to all the scientific investigations that have been done on the genetics of intelligence and personality, and of such behaviours as criminal, neurotic and psychotic; yet the belief persists, is advocated with a great show of emotion, and has led to the branding of those not sharing it as being unscientific, racist, anti-feminist, etc. Research findings showing a correlation between parental and filial behaviour are unblushingly interpreted in environmental terms, and the possibility or even probability that the observed correlations may be due to genetic factors is completely neglected.

That such opposition to genetic views is an ideological child of the Zeitgeist, and not due to rational, scientific observation, is clearly shown by the fact that practically none of the people arguing in this fashion have any expert knowledge in the field, that psychologists are not normally taught anything about polygenic genetics, and that the arguments and debates found even in prestigious journals are of a scientific standard so low as to be laughable. Thus here we have the odd situation where environmentalist crypto-science constitutes the orthodoxy, and real science, i.e. behavioural genetics, is cast in the role of the villain, and treated in precisely the way so well described by Westrum.

Genetics is of course only one example; many others could be given. Thus when I pointed out in 1952 that there was no acceptable evidence to support the view that psychoanalytic treatment in fact cured neurotic patients, and that when compared with spontaneous remission there was no advantage for psychotherapy, there was a very furious response on the part of orthodoxy, characteristically attacking not what I said, but what the critics imagined I might have said, producing argumenta ad hominem, and generally showing an utter disregard for the most elementary traditions of scientific debate. Even now, after 30 years, orthodoxy is still putting forward completely discredited arguments and data, and refuses to look at the evidence in a calm, unimpassioned and objective way. If "pseudoscience is a study of the non-existent by the incompetent," then surely the theory and practice of psychotherapy must qualify; yet it is the opponents of psychotherapy who are treated in the way described by Westrum!

To give just one more example, I have criticised, in my book on The Causes and Effects of Smoking the well-known belief that smoking cigarettes causes lung cancer and cardiovascular disease, showing reasons why the alleged evidence in favour of these beliefs is in fact very weak, and subject to many very serious criticisms. I have also suggested an alternative hypothesis (or possibly a complementary one) which takes into account many facts disregarded by orthodoxy. As a result my contribution has treated very much in the way that according to Westrum crypto-science is treated, although there is nothing "crypto" about the arguments, all of which are derived from evidence published in the most prestigious medical, genetic and psychological journals. The Zeitgeist simply insists on gross over-simplifications of the kind embodied in the statement that: "smoking cigarettes causes cancer and cardiovascular disease," and any opposition to such statements, however scientifically justified, is treated aggressively and with contempt, even by people who have not read the original literature, and would not understand most of it if they were to read it.

I think one might say that by contrast with defending views which are entirely in line with all the available scientific evidence, but which go against the ideology of the Zeitgeist, doing research in crypto-science, and advocating its beliefs, may be the lesser of two evils. I do not recall people who believe in the existence of UFOs, or in astrology, or in paranormal psychology, being personally attacked and beaten up, as I was by students who objected to a lecture I gave on the psychophysiological measurement of intelligence. Similarly Arthur Jensen has been threatened, as has his family, by over-enthusiastic environmentalists who seemed ready to shoot him, burn down his house, and kill his family. Both he and I had to seek police protection, all for advocating facts which are universally agreed to be in line with the best available scientific evidence. I conclude that Westrum is correct in his description of what happens to advocates of crypto-science, but would like to suggest that what is important is not the crypto-science part, but the opposition to the Zeitgeist. Any belief, any research, and any theory which opposes the Zeitgeist is liable to be treated in much the same fashion, whether we are dealing with true crypto-science, or with what is universally regarded by the experts as true, mainstream science. Thus Westrum's article requires to be extended to areas other than crypto-science, and it would be very interesting to discover just how the Zeitgeist is created, why it acts in the fashion it does, and why scientists are so easily detached from the objectivity that ought to characterise their work, and become ideologists subservient to the Zeitgeist. But this clearly is a much wider topic, on which Westrum, perhaps wisely, says nothing. It would nevertheless be a worthwhile topic for a sociologist to investigate.

COMMENTS BY PATRICK GRIM:

The following line of argument is evident in Westrum's piece and in many a piece like it:

Let us suppose that we _are_ in fact being visited by extraterrestrials. Then there is a clear sociological explanation for why claims of extraterrestrial visitation are nonetheless so widely considered dubious within established scientific circles: "Anything that poses the danger of erasing the boundary between standard, formally recognized 'reality' and other possibilities is violently resisted . . . because of its subversive implications," etc. etc. Therefore we ought to be more tolerant and supportive of the work of UFOlogists and others like them.

My first difficulty with appeals of this familiar type is that I am not sure how good the sociological explanation offered really is. Here, at least, it comes dangerously close to the banal observation that areas of investigation which are considered marginal will be treated that way.

That difficulty aside, however, I am still at a loss as to what to make of the crucial "therefore." Let us grant that, given genuine extraterrestrial visitation, their neglect within establishment circles could be sociologically explained. By what means does it follow, without assuming the reality of such visitations, that standard treatment of UFOs and UFOlogists ought to be anything other than what it now is?

As far as I can see, any conclusion of this sort is a simple _non sequitur_. If we _are_ being visited, we will be able to give a sociological explanation for the fact that that supposition is nonetheless generally rejected within certain circles. But if we are _not_ being visited, we will also be able to give an equally compelling sociological or social psychological explanation for the fact that such a supposition is nonetheless _accepted_ within certain circles (see for example Leon Festinger, Henry W. Riecken, and Stanley Schachter's _When Prophecy Fails_ and Alan C. Kerckhoff and Kurt W. Back's _The June Bug_). Whichever side of the dispute turns out to be right, then, a sociological explanation will be available for the error of that side which happens to be in error. But it no more follows that establishment science ought to be softer on UFOlogists, in light of one possible explanation for possible error on one side, than that it ought to be _harder_ on UFOlogists, given the other possible explanation for possible error on the other side. From the possibility of such sociological explanations for error it does not follow that those on one side of the dispute _are_ in error, that they are _likely_ to be in error, or that they ought to be more open to or more tolerant of the opposition.

Let me try to avoid a misunderstanding at this point. I think Westrum may be absolutely right in thinking that UFOlogists and other 'cryptoscientists' are unjustifiably snubbed, slighted, and hampered in their work by the core scientific community. But I also think that the only legitimate _argument_ for such a claim would be a direct and unabashedly normative argument concerning justifiable and unjustifiable scientific procedure. General sociological scenarios of an establishment elite jealously guarding its supposed truths, like suggestive historical anecdotes of genius neglected or evidence ignored, are simply not enough.

There is in all this a quite general lesson regarding the limits of sociology of science. Despite its crucial importance, sociology of science cannot replace the science of which it is the sociology, and cannot generally even serve as a satisfactory basis for argument within that science. The existence of quarks, for example, is a question for physics, and no sociology--even a sociology of physics--can offer much that is of relevance to that question. Nor can sociology of science supplant *philosophy* of science, conceived of as addressing normative issues of proper scientific procedure--an enterprise evident in the work of both working scientists and idle philosophers. How physics *ought* to proceed is as much beyond the reach of a mere sociology of physics as are questions of quarks.

Let me finally add a note concerning what I regard as a substantial and important contribution of Westrum's paper: the initial outline and discussion of "cryptosciences," the sciences of "things that might be there."

"The aim of cryptoscience," Westrum says, "is to collect and analyze information about such anomalies UFOs, ghosts, bigfoot, etc. so that (1) if they exist, they may be brought within the domain of science, or (2) if they turn out not to exist, to explain why there nonetheless appears to be evidence for them."

What I want to point out is simply that the attempt to pursue any such "cryptoscience" will face an immediate and quite fundamental difficulty, at once both theoretical and practical. The difficulty is this: precisely what kind of investigation is called for, and even what form of 'collection and analysis' is called for, will crucially depend on whether the thing at issue in fact exists or not. If extraterrestrials *do* visit regularly, the scientific work to be done is the work of biology and astronomy. If they do not, the work to be done is the quite different work of sociology and psychology.

And what if extraterrestrials might be visiting or might not? All that we can say in that case, I think, is that it might be astronomy, or on the other hand it might be psychology, that is called for--we don't yet know which. But these are radically different disciplines, in terms of tools, training, forms of expertise, and methodology. Because of such radical differences, Westrum's notion of a single unified and coherent discipline which will in the meantime somehow straddle the two and might be astronomy, or might be psychology, but is not yet either, is simply boggling. Until we have some justifiable guess as to what certain apparent anomalies *really are*, then, we will not know *how* to study them, or analyze them, or even classify them. For that reason any genuine science of UFOs, or ghosts, or spontaneous human combustion, I'm afraid, will prove precisely as elusive as the things themselves.

COMMENTS BY HENRY H. BAUER:

Westrum's case studies are valuable; he detects and elucidates important themes, which must be considered by those who wish to study anomalies or hidden events. But I shall not take space to detail the many virtues in Westrum's work: it is more productive to argue than to agree. Though my disagreements with his paper are more of nuance than substance, I think the nuances are important.

In detailing societal barriers to crypto-scientific work, Westrum sounds rather pessimistic; and comes close to implying that those barriers might not exist in a more ideal society (more open, tolerant, democratic). I shall argue, however, that

1. The difficulties facing crypto-science are not arbitrarily imposed by society, but stem inevitably from the different natures of science and of crypto-science.

2. The difficulties are not so serious since excellent work in crypto-science can be done and has been done.

3. The difficulties present a welcome opportunity for disinterested intellectual endeavor.

1. I do not share Westrum's hope that understanding of, and participation in, science by the ordinary citizen would lead to healthy "amateur science." Science is not arbitrarily closed to the ordinary citizen -- it is closed because, as Westrum himself says, good research takes proper training as well as time and money. So the established disciplines are by definition closed, except in such specialized areas as some types of observational astronomy, field biology, or archaeology; and if we consider other than established disciplines, we arrive back at crypto-science.

But further, science or crypto-science or any form of intellectual activity is inevitably elitist rather than democratic. In the search for new knowledge, there is a premium on intelligence and on knowledge and depth of education and understanding. The larger the number of participants, the greater the noise and the mediocrity -- as evidenced by the so-called "information explosion" of the last couple of decades, which has been more an inflation in the amount of published material than an increase of gained understanding.

Westrum says, "The value of such [crypto-scientific] activities seems obvious". I would say, by and large only to those of intellectual bent, who form a very small minority in our society. The Loch Ness question may be relevant here: before the early 1930s, Loch Ness was rather isolated, and the locals could have kept watch for the creatures without knowing or caring that the scientific community might be critical. It seems, though, that the natives were simply not very interested: some believed that Nessies are real, natural animals, and no occasion for making a fuss; others mixed them with various mythical attributes and legends, and warned children not to go near the water, but were no more concerned about that than about fairies or spirits; others again regarded the whole affair as nonsense. Even now, after 50 years of world-wide publicity, many of the local residents display no obvious interest in the existence or nature of Nessie.

So science and crypto-science will always be specialized pursuits, carried on by minorities within society, and significantly different from one another because they are based in different ways on pre-existing knowledge.

2. The difficulties in pursuing crypto-science are real but by no means insurmountable. That some (even many) scientists and science writers and others are quick to label implausible quests as pseudo-science does not make the quests impossible. But I think it is important to distinguish, and then to keep separate, two quite different objectives: first, to carry on investigations; second, to convince others that the investigation is worthwhile. Westrum focuses on the second, I believe; and therefore emphasizes difficulties, which I agree may well be insurmountable. I would agree that the first objective, however, is attainable, and independent of the second.

Heuvelmans has surely accomplished much in cryptozoology: at the very least, his writings make available for all time a rational basis for many quests. Constance Whyte, in "More Than A Legend" (1957), gave the stimulus for the last 25 years of increasing successes at Loch Ness. And here one of Westrum's generalizations does not hold, that crypto-science will be left to sub-standard practioners with sub-standard resources: Tim Dinsdale and the team assembled by Robert Rines have done splendid work, with state-of-the-art (and even pioneering) equipment. Of course more could have been done if.... But most every scientist would say the same about his pet sub-specialty. Let me suggest that Westrum's pessimistic emphasis may stem in part from his taking ufology as an exemplar rather than Loch Ness, or acupuncture, or biological effects of electromagnetism, or hypnotism -- areas in which amateurs and maverick scientists have brought erstwhile pseudo-science or crypto-science or proto-science to acceptance, or close to acceptance, by the scientific community. We surely cannot ask for acceptance until enough facts are in; but even before that, one finds some scientists willing to help in a quiet way, as at Loch Ness.

There is of course important truth in Westrum's generalization that society is able to find out only about what it is willing to accept. But there are no two fixed categories of acceptable and unacceptable; subjects move from one to the other over various periods of time, and some of them lie between those two extremes. And a saving grace is that American society, for one, is pluralist in important practical ways: crypto-science may not be publicly funded, but is is not outlawed either. Rhine found support for work on ESP over decades.

The personal dilemma of the ufologist -- to spend more time on it, to spend less time on it -- is not just society's doing: it stems from the present and real uncertainty of the matter. Were the data incontrovertibly convincing to enough individuals, then those individuals would find a way to carry on. They would, in the future and by hindsight, be classed as cranks or as geniuses, depending on the eventual outcome; but history makes clear that sufficiently convinced people carry on the quest regardless -- and Dinsdale is a living exemplar; Heuvelmans another; and Mackal, Rines, and so forth. So it is not a question of cowardice as inhibiting factor, I suggest: it is that it would at present be too unreasonable not to hedge one's bets about UFOs.

It is important not to overgeneralize. Indeed, I see danger in lumping all unexplained matters together: that, after all, is what the confirmed debunkers do, asserting Loch Ness and the Bermuda Triangle and UFOs -- for example -- all guilty by association. When Westrum lumps together UFOs, ghosts, bigfoot, Loch Ness, SHC, etc. as subjects worthy of substantive objective study, he may play into the hands of such as CSICOP. We must be clear that some subjects are a priori more implausible than others, some currently more intractable than others; and above all we must be clear that some of these things will turn out to be genuinely without other substance than misreporting and the like.

Let me re-emphasize that I am responding to nuances in Westrum's paper, not contradicting. Certainly the prevailing consensus makes the quests quite difficult for the investigators. But let us recognize that the questing is feasible; not feasible is the trying to convince society that it should look with favor on the quest. Moreover, when we try so to convince others, we may do the quests a disservice: in attempting to make the case, we readily slide into maintaining that the evidence is stronger than it actually is. Instead of saying, "This phenomenon interests me, and is worth pursuing even if the cause turns out to be misperception", we tend to say, "There is so much testimony that something must be there" -- as unsupportable a stance as the opposing one taken by the perpetual debunkers.

3. So let me press optimism, and point to the intellectual pleasure that is ours for the taking if we make the quests primary, and forget about trying to make the quest seem respectable to the society or to the scientific community. Here again, Dinsdale is an exemplar: in 1960 he filmed a Loch Ness monster, made the film available to scientists, and found that incontrovertible piece of evidence to be insufficient to carry the day. Very quickly he realized that public arguing and berating of scientists' conservatism was pointless; for more than two decades he has carried on the search, scrupulously avoiding controversy under even severe provocation. He can testify -- has in his writings -- to the personal satisfaction he has gained, the widening of his perspectives, the gaining of new knowledge, the making of great friendships; and I believe the history books of the future will add objective vindication.

This "third way", it seems to me, is the one to press and proselytize. ZS represents one realization of this; the Center for Scientific Anomalies Research another. The new Society for Scientific Exploration offers opportunity, as does the International Society of Cryptozoology. The third way is to build a community dedicated to fostering opportunities for reporting on quests, for constructive analysis and criticism, for enlisting the like-minded, for setting an example of unaggressive but determined pursuit of oddities and enigmas, for satisfying our human curiosity. Rather than deploring that science is closed, let us consciously enjoy the advantages that amateur pursuits offer: for one, that the motives of curiosity and interest need not conflict with career-building and money-making quite as they do in science (or any other profession).

Of course, there is also room and need to educate the wider society and the media, to expose the misdeeds of the extremist debunkers, to correct misconceptions about science and its role in society. But thinking primarily of opportunities for crypto-science, I conclude that Westrum expressed himself too pessimistically; and neglected the important distinction between doing it and justifying it.

COMMENTS BY SUSAN SMITH-CUNNIEN AND GARY ALAN FINE:

Just as the examination of purported deviance allows us to realize the often unseen contours of "normal" behavior, so too does the examination of "deviant science" permit us to recognize the outlines of "normal science." Deviance and normative behavior are much closer in form and process than their contradictory stereotypes suggest. Likewise, science and cryptoscience frequently do not differ in terms of aims and methods. As Westrum suggests, issues such as work conditions, resources and professional legitimacy may be of greater relevance to the difision between these two forms of knowledge production.

Westrum makes an interesting comment in the first paragraph of his discussion of cryptoscience:

> The cryptosciences deal with those objects and events whose existence is so far not acknowledged by science, but for which there is nonetheless other evidence, such as human testimony (emphasis added).

First, we note the reification of the "scientific establishment" (itself a term with some measure of reification) as "science." The use of the term science -- implying a body of knowledge -- suggests that the body of knowledge rejects crytoscience. This contrasts with the saying that cryptoscience is rejected by the consensus of a set of "political" actors (using the word political broadly to refer to individuals who wish to control resources and to use power (see Cohen, 1977)), which is basically Westrum's argument. Second, we find it interesting that "other evidence" is contrasted with scientific acknowledgement. This implies that evidence for a phenomenon is the attitude of scientists. We don't intend to critique Westrum's article on the basis of specific words which he used. Surely it was not his intent to write with a primary concern for the nuances of language. But we do find it intriguing that it is difficult to prevent oneself from "buying into" the rhetoric of scientific objectivity, even though that may be the topic of the paper. Careful readers may note that, despite our best efforts, we may do the same ourselves.

We see Westrum's particular contribution to the literature on the sociology of science as his raising of several key issues in the sociology of work and occupations. Although he phrases his concerns somewhat differently, we see Westrum's discussion as revolving around how the structure of an occupation relates to its position in the stratification hierarchy of occupations. As Westrum demonstrates for UFOlogists, the organization and practice of work depends in part on the resources that workers can muster.

Although Westrum adds to our knowledge of the boundaries of the scientific enterprise, he focuses almost exclusively on the status quo. We wish to expand his analysis by addressing the related concepts of diversity and change. We hope to add a dynamic dimension to his analysis by following certain theoretical directions suggested by literature in the sociology of occupations (especially that of emerging professions) and social movements. In referring to this literature we do not mean to imply that science or cryptoscience is an emerging profession or a social movement. We merely wish to suggest some analogous ways in which diversity and dynamism can be incorporated in the analysis of the relationship between cryptoscience and orthodox science.

Diversity Within The Scientific Community

We agree with Westrum that at any point in time there exists a dominant normative system in the community of scientists regarding acceptable scientific questions and acceptable methods for obtaining answers to those questions. However, within the scientific community there is by no means total consensus. Often the view of science as a monolithic enterprise is more apparent than real. Westrum himself notes the diversity of methods that comprise the "scientific method." A diversity of substantive and political interests exist as well. Within the gross division of labor among scientists -- such as biology, physics, and chemistry -- there are those who are working on the fringes of their specialty area. Some of those on these frontiers may ask questions and obtain answers that eventually result in paradigm shifts and a new perspective on scientific investigation.

This diversity within the scientific community (or subcommunities) is not a phenomenon which simply occurs prior to a paradigmatic shift. Diversity is much more ubiquitous. For example, Bucher and Strauss (1961) note that definite "segments" develop within existing professions (and even within specialty areas of a profession). These segments have organized identities, values, "missions" and, as a result, different work activities. Moreover, they may be in conflict with one another for resources, including status and legitimacy. Bucher and Strauss suggest a process-oriented model in which these segments -- and the relationship among the segments -- are in a constant state of flux. As an illustration, these authors point to:

> the progenitors of the clinical pathologists, who today are a threat to the institutional position of research-oriented pathologists but who were considered the failures, or poor cousins, of the specialty thirty years ago (p. 333).

They recommend that sociologists examine these segments as social movements, looking at the recruitment strategies, the development of ideology, and the development of an organizational structure, which will in turn affect the success of the segment in establishing legitimacy and/or dominance. Scientific and professional enterprises are not identical, but the Bucher and Strauss model of professions in process may nonetheless be fruitfully applied to an analysis of diversity, conflict, and change in the scientific community.

Specifically, if we see cryptoscientists as part of the knowledge-generating industry, it becomes clear that they are in competition with those who are working on other ("acceptable") problems. These researchers are threatening to dislocate the balance of power and resources in the profession. If there is not undeniable and insistent evidence of a "new" phenomenon, scientists rely on the political balance of the status quo. This may be particularly true during periods in which resources for the field are not expanding or are diminishing.

The Effects Of External Changes

Changes that occur in the relationship between orthodox scientists and cryptoscientists may also be related to changes that take place in other parts of society. McCarthy and Zald (1977) outline an approach that they (and others such as Oberschall (1973)) have labelled the resource mobilization perspective. From this view the growth and decline of a social movement are not due primarily to preexisting strain or discontent, but to the group's mobilization of resources to create (or prevent) change. The ability of the leaders of a social movement to mobilize resources successfully depends on several factors, including the environmental context and the enlistment of outsiders in support of the movement.

These two features of the resource mobilization perspective -- the attention to environmental conditions surrounding the movement organization and the enlistment of outsiders in support of the movement -- provide some direction for thinking about the relationship between orthodox science and cryptoscience. We can think about cryptoscience as analogous to a social movement: "a set of opinions and beliefs in a population which represents preferences for changing some elements of the social structure and/or reward distribution of a society" (McCarthy and Zald, 1977: 1217-18). Orthodox science may be likened to a countermovement: "a set of opinions and beliefs in a population opposed to a social movement" (McCarthy and Zald, 1977: 1218). Each is supported by social movement organizations, such as The Center for UFO Studies or the Committee for the Scientific Investigation of Claims of the Paranormal. Environmental conditions, such as the increased availability of discretionary resources among a larger segment of the general population increases the possibility that competitive scientific social movements will arise. This availability of resources may explain the increase in organizations devoted to studying paranormal phenomena.

Another feature of the resource mobilization perspective of relevance to the relationship between orthodox science and cryptoscience is the role that outsiders play in the success or failure of a movement or countermovement. In this regard we refer to outsiders as those not directly engaged in the work of science or cryptoscience (and, hence, not directly affected by the dominance of either science or cryptoscience), but who nonetheless may have preference for and provided support for one enterprise or the other. McCarthy and Zald (1977) refer to these as conscience adherents or conscience constituents. Westrum (1976) provides an example of how outsiders can support a movement in his discussion of how scientists provide "expert testimony" regarding phenomena about which they have no special expertise. In this article, his discussion of the implicit division of labor in UFOlogy, whereby adequate research requires the aid of trained hypnotists to examine potential abduction cases, also illustrates the impact of outsiders.

In a somewhat similar vein, some research in the area of emerging occupations indicates that the role of the client may be quite influential in determining the success or failure of attempts by occupational groupings to maintain or enlarge occupational task boundaries. In her study of the relationship between physicians and pharmacists, Kronus concludes that:

> when the occupation was able to demonstrate that it controlled a sizeable portion of the market -- as indicated by the size, wealth, and loyalty of its clientele -- its task boundary efforts succeeded. (Kronus, 1976: 35)

Clearly a "client" is not identical to an "adherent." But we can nonetheless extend Kronus' findings to the relationship between science and cryptoscience for the purpose of generating ideas. Perhaps the lay audience of science -- in addition to the collegial audience -- may play a large role in determining the relationship between cryptoscience and orthodox science. In extreme form this is "science by democracy"; a less extreme view recognizes that the knowledge-consumer can influence the sort of knowledge that will be produced. An example of this process is in the area of the racial basis of intelligence. Without wishing to judge the substance of the debate, we note that most opinion-leaders believe in racial equality. Researchers who find small inherited racial differences in intelligence are thus accorded more respect than those who claim that there are substantial racial differences in intelligence -- even though there are flaws and ambiguities in both bodies of data. One wonders whether we would know anything if all scientific or social scientific research was subject to the same scrutiny to which research on cryptoscientific topics (e.g., extra-sensory perception) is subjected. The legitimacy of scientists as reality-constructors may be questioned by their various publics. Because of the choices that are made in all scientific research, every study can be questioned by those with a mind to do so.

This view of the power of clients is consistent with Haug and Sussman's (1969) analysis of the "revolution of the client." It is also consistent with developments in the legal legitimation of medical practices such as chiropractic, where grass roots politiking on the part of consumers appears to be at least in part responsible for the legal victories of chiropractic in the face of organized opposition by orthodox medicine.

Conclusions

Westrum suggests that the definition of cryptoscientific events as real or unreal is the key factor in the position of cryptoscience in the occupational hierarchy and the resultant organization of cryptoscientific research. While this is a significant advance over previous analyses which find the differences in the logic and rigor of inquiry or in personal characteristics of the investigator, we believe that it is necessary to add a dynamic component to the analysis which incorporates the diversity and conflict within the scientific community as well as between cryptoscience and orthodox science. The relationship between orthodox science and any type of cryptoscience is not static. Further, the similarities between these two "different" approaches are much greater than their differences. Although the resources and prestige differ, the goals, methods, reliance on others outside the field, and even the audiences are quite similar. There are scientists of all stripes who turn out to be wrong (even grossly in error), but often it is impossible to know this until much later. We do know that orthodox scientists have more "idiosyncrasy credits" than do their colleagues in cryptoscience, which means that their surprising statements are more likely to be given credence.

By the formal definition cryptoscience and orthodox science are much the same; what differs is the oddsmaker's evaluations of the unknown coming to be known. Yet, if oddsmakers could predict the future, then no one would go broke at the races or in the stock market.

References

Bucher, Rue and Anselm Strauss, "Professions in Process," American Journal of Sociology, 1961, 66, 325-334.

Cohen, Anthony P., "For a Political Ethnography of Everyday Life: Sketches from Whalsay, Shetland," Ethnos, 1977, 42, 180-205.

Haug, Marie R., and Marvin B. Sussman, "Professional Autonomy and the Revolt of the Client," Social Problems, 1969, 17, 153-161.

Kronus, Carol L., "The Evolution of Occupational Power: An Historical Study of Task Boundaries Between Physicians and Pharmacists," Sociology of Work and Occupations, 1976, 3, 3-37.

McCarthy, John D., and Mayer N. Zald, "Resource Mobilization and Social Movements: A Partial Theory," American Journal of Sociology, 1977, 82, 1212-1241.

Oberschall, Anthony, Social Conflict and Social Movements. Englewood Cliffs, N.J.: Prentice-Hall, 1973.

Westrum, Ron, "Scientists as Experts: Observations on 'Objections to Astrology,'" The Zetetic, 1976, 1, 34-36.

COMMENTS BY ANDREW NEHER:

Ron Westrum's article is a stimulating addition to the "Science is too conservative" literature, which by now is quite voluminous. Before I discuss his major thesis, however, I'll comment on several of Westrum's lesser points.

Westrum complains that "Being a UFO logist [or, by implication, a cryptoscientist of any kind] is not good for family life, it is not good for careers, and it is definitely not good for one's piece [peace?] of mind," that "making [cryptoscientific] observations or even taking an interest in them is stigmatized," and that "the reporting of...deviant cases ... seldom takes place." What he fails to stress is that this is true only within the scientific community. In the "pop-science" and non-scientific communities, cryptoscience often outdraws science; witness the popularity of crypto-writers and lecturers such as C. Norman Shealy, Fritjof Capra, and J. Allen Hynek. This, of course, is *itself* an anomaly if it is true, as Westrum maintains, that the duty of establishing "official consensus about what is real and what is not ... in our society ... is largely in the hands of the scientific community."

Westrum also states that "I am a social scientist, with a career hardly likely to be advanced by my interest in UFOs." My guess is that that would depend on how his interest is directed. If Westrum, as sociologist, produces valuable studies of the *sociology* of UFO groups and belief systems, that certainly will enhance his career. If, however, Westrum goes outside his area of professional competence -- to concern himself, for example, with the question of whether or not UFOs are "real"

-- his views are likely to be taken the way Shockley's views on race are taken, as dilettantism at best and misleading at worst.

Finally, Westrum bemoans the fact that mainstream scientists "if interested in extraterrestrial intelligence ... will safely pursue their interest in the manner approved by the tribe: by building or operating radio-telescopes to detect signals from distant galaxies." Perhaps Westrum hasn't considered that radio-telescopes -- in their promise of yielding a definitive answer, free from a thick overlay of psychological interpretation -- may be the more sensible approach to the study of extraterrestrial intelligence.

Now let's return to the central question that Westrum raises: Is science too conservative in its attitude towards research on anomalistic experience? This is at least a two-part question, in that it applies both to 1. funding and other support for cryptoscience, and 2. acceptance of extraordinary claims. Let's discuss these in turn.

Westrum objects to the low level of support provided for crypto-scientific research, but he is well aware of the reason; as he says, "I ask myself if I am not wasting my time chasing phantoms." How many resources should we devote to chasing phantoms? My answer is, probably some (the few phantoms we track down might be valuable indeed!) but probably not a lot. Whether our current backing of "long shots" is adequate, however, is a difficult question; I don't pretend to know the answer.

Westrum also feels that mainstream science is unjustifiably resistant to accepting extraordinary claims. The instances he cites -- e.g., of scientific skepticism regarding the existence of meteorites -- certainly strengthen his position. However, let's take a step back for a minute and view this issue in larger perspective.

Let's begin by admitting that we never know anything "for sure". Our beliefs about the world are just that, with probabilities we assign to them ranging from infinitesimal to almost a sure thing. Let's grant further that there is a certain level of proof we demand to back up our beliefs (obviously this doesn't apply in matters of faith). Now let's acknowledge that if we are satisfied with too little proof, we will end up accepting many claims that eventually prove to be false; statisticians call this a Type I error. Let's also recognize that if we demand too much proof, we will fail to accept some claims that are, in fact, true, making what is called a Type II error. Now here's the difficulty. Although our threshold of acceptance varies somewhat according to our personal preferences, in the long run there is no way to avoid these errors. And, a moment's thought will show that trying to eliminate one variety of error only increases the number of errors of the other variety! The best that can be hoped for is some kind of balance between the two varieties.

When Westrum says that science is too conservative in its acceptance of extraordinary claims, he means that science commits too many Type II errors. But, of course, if this is so, and science becomes more liberal, then it will end up committing more Type I errors. Furthermore, if Westrum is right (and he undoubtedly is) that certain accepted, but faulty, beliefs in science serve as barriers to extraordinary but valid claims, then these faulty beliefs are the direct consequence of Type I errors -- i.e., obviously they were accepted prematurely and without sufficient proof. In other words, Westrum seems to be saying that science is committing too many of both kinds of errors. But, of course, you can't have it both ways. And, although we can empathize with Westrum's frustrations, we must acknowledge that the Eastern sage is correct, that error is necessary. Of course, that is little consolation to one who is the "victim" of such errors.

Thus, the question is not simply "Is science too conservative in its attitude towards extraordinary claims?" but rather "Has current scientific practice struck a proper balance between two unavoidable sources of error?" Again, although I hope I have helped clarify the question, I unfortunately do not claim the wisdom to know the answer.

COMMENTS BY DANIEL COHEN:

I agree with a good deal of what Dr. Ron Westrum has to say. His point about the self-fulfilling prophecy nature of the incompetent investigation charge is very well taken. His hope that ordinary citizens can become more involved in the process of science is so democratic that it is clearly subversive, and I love it.

However, as a popular science writer, and sometime member of the "secular arm," I must take exception to some of his comments. Dr. Westrum gives the impression that there is pressure on us poor ink-stained wretches to uphold orthodoxy and ridicule offending ideas. That may be true for a few publications (though I have no certain, first hand knowledge that it is). But for the vast, vast majority of popular publications the pressure comes from the other direction. The same is true for most local television news, and television feature shows. As a result, a great deal of absolute crap is presented as authentic cryptoscience to the general public. It should, therefore, come as no surprise that many orthodox scientists whose primary interests lie elsewhere, don't take the time to tell the difference. Those who claim to investigate cryptoscience forfeit this excuse. I submit that cryptoscience has suffered more harm at the hands of friendly (though self-serving) publicists than at the hands of the "secular arm" (a strange image. Can an arm have more than one hand?) Anyway, Charles Berlitz is a much greater threat to competent cryptoscientific investigation than Philip Klass.

It also seems to me that Dr. Westrum shows insufficient understanding of and/or sympathy for the plight of the orthodox scientist or thinker who is faced with an anomalous claim that he does not have the time or interest to look into. The natural, inevitable and correct first response is FRAUD. Yes, correct, because there has been an enormous amount of fraud in cryptoscience. There has been plenty of fraud in orthodox science as well, but by any measure cryptoscience has compiled an even less enviable record, So the problem is not entirely, or even primarily, orthodox blindness to anomalous events.

It is not the main business of those involved in cryptoscience to spend their limited time and resources trying to curry favor with the orthodox, or explosing and expelling the fakers and true incompetents from their own ranks. Their primary task is the study of anomalous events. But I do wish that cryptoscientists would show a greater recognition of the nonsense in their field, and try to be a little less defensive, even in the face of hostility.

Damnit,it does sound funny for a grown man to spend his spare time looking for UFOs or Bigfoot. It isn't wrong, just funny, and crypto-scientists must deal more effectively with that perception than they have in the past. The must not answer criticism with a psychoanalysis of their critics as Velekovsky, Eisenbud and others have done. And they should, from this day forward,drop all references to meteors. That is an example that I have been hearing forever, and it does not justify everything. Yet, as I said at the start, I do happen to agree with a great deal of what Westrum has to say. Anomalous events should be treated more seriously, and the more seriously they are treated the better the investigations will be.

How is this hoped for end to be accomplished?

"That," as they used to say in the days of my youth, "is the $64 question."

COMMENTS BY SONJA GROVER:

I agree with Ron Westrum's assertion that: "Anything that poses the danger of erasing the boundary between standard, formally recognized 'reality' and other possibilities is violently resisted..." (initially at least). I also concur that definitions of science as the study of "the real" are inadequate; for such a view tends to block modification of various knowledge claims and data interpretations. Westrum further suggests that "much of what is now called pseudo-science" might better be viewed as "amateur science" were the scientific community to be more "open," "tolerant" and "democratic."

I do accept that sometimes - perhaps more frequently than most would care to admit - anomalous events are <u>at first</u> discounted as pseudoscientic data (e.g. Lord Kelvin's rejection of the x-ray). The magnificent aspect of science is, however, that such data is eventually incorporated <u>if it can make a theoretical contribution</u> (e.g. neutrino events). We must be wary, however, of classifying nonscience as protoscience and it is with the latter possibility that Westrum does not adequately come to terms in my opinion. Contrary to Westrum, I do <u>not</u> think that generally most pseudosciences are so classified because they involve anomalous data; though I agree this may happen on occasion (nonscience is a term I prefer as I explain elsewhere in a paper "The Masquerade of Nonscience as Protoscience,"but I will use the term "pseudoscience" for the present purposes). Most pseudosciences are so termed I believe because, for example, their theoretical assumptions are not explicit enough to be open to revision, no theoretical implications of the alleged data are clear or thought to be fruitful, the reliability of the finding(s) is questioned given the inadequacies in methodologies used,etc.

Pseudosciences, I suggest, do not generate anomalous data within their own conceptual context. Thus theoretical assumptions underlying the field tend to be static and vague, for there is no data base with which to refine or modify views within the field. Pseudosciences are thus in significant ways unable to generate data which conflict to any degree with the basic assumptions underlying their self-defined areas. Consequently, astrological theory, for instance, remains much the same today as it was hundreds of years previous.

I do not agree with Westrum that the anomalous, if noticed, is generally kept secret: not by scientists at least. Deviant reality, ESP, for example, "does not get to confront standard reality" usually not because it is suppressed but because its useful research implications are not evident given the definition of the phenomenon in question. When a concept has clear theoretical and research value, then the undetermined "reality" status of the event to which the concept refers is generally no great barrier to the notion's acceptance. The history of science, I think, bears this out.

It is not the study of the anomalous or inquiry into events of so far undetermined reality which is to be labelled pseudoscientific. Rather, it is the study of theoretically bankrupt notions defined so as to be resistant to any possible falsification or modification which are most frequently at risk to be labelled pseudoscientific, and justifiably so. Whether the study of UFO's rightly falls into this category, I cannot judge having not studied the area in depth.

COMMENTS BY WILLIAM R. CORLISS:

Ron Westrum has nicely mapped the roadblocks that an established social system erects to preserve itself. In Westrum's discussion, science is the social system. Like any other social system, science is created and maintained by people, and people generally don't like boat-rocking. The difference, of course, is that science maintains vigorously that it is always open to new thoughts and has in fact an established mechanism for incorporating change. Westrum's paper demonstrates that this mechanism functions very poorly in some cases -- the more revolutionary the subject, the crankier the mechanism becomes.

One is tempted to advise frustrated individuals with bold new theories and/or data not to worry; that their ideas and information will be recognized eventually if they have merit. After all, meteorites were finally admitted to be real; and continental drift was ultimately admitted to the fold. If we wait long enough surely UFOs, ESP, scientific creationism, Velikovsky's theories, von Dainiken's ideas, evidence for a young earth, and data supporting planetary influences on terrestrial life will all be taken seriously!! Don't count on it. The scientific establishment not only condemns these data and theories, but the proponents of each set of theories or anomalies are usually extremely intolerant of other sets of theories and anomalies. My point is that it is not really the scientific community that is at fault, it is man's nature. Let's face it, scientists are intolerant of facts and theories that challenge their worldview, and so are anomalists. Not too many UFO researchers would objectively review evidence for a 6,000-year-old-earth -- the very thought is ridiculous! Facts or no facts; a young earth is impossible. But, then, so are UFOs.

One solution to this dilemma is the formation of a real two-party, adversary system in science. The "loyal" opposition would continually apply pressure on establishment science (the "party" in power) to examine ideas and data. The newly formed Society for Scientific Exploration and, of course, the Center for Scientific Anomalies Research, are steps in this direction. Can such societies have an important impact? Maybe, but there are two facts weighing against such a development: (1) Such groups are made up of people who inevitably have "residual" intolerance toward some types of anomalies; and (2) The real cosmos may be incomprehensible to man (or woman) as he is now constituted. The anomalies "out there" are so profuse and so great as to be beyond our present ken. Either our brains are not wired right, or our social milieu prevents us from thinking right. The thought that man cannot comprehend the real cosmos, much less manipulate it, is a great heresy. Undoubtedly I will now be excommunicated by all anomalists!

Since humans are imperfect (Where have I heard that before?), mayhap the computer will rescue science. Silicon chips are value-free; and we can just pump computer memories full of data (no theories) and tell the computer to predict any future situation for us based upon past experience. No theory need be espoused. Experience would be the sole guide. Want to know tomorrow's weather or an electron's trajectory? Give the computer the initial conditions and it will employ statistics or a Monte Carlo approach to sketch out the future. I call this "computerized scientific nihilism," better known as CSN. Unfortunately, value-free though silicon chips may be, the computer's programs are based upon human logic, human expectations, and human intolerance for the "wild point." There are no non-Aristotelian computers around; there are none that can cope with a Martian's logic; and if, in some remote corner of the universe, 2 and 2 do not make 4, the computer will blow a fuse.

Is there no hope then for crypto-science, or even regular science? Of course there is. I am just as intolerant and prejudiced as anyone else.

COMMENTS BY NORMAN DIXON:

The background to Westrum's discussion of the nature of research in what have been called the "crypto-sciences" may be summarised as follows:

1. There are a number of anomalous phenomena* such as UFOs, ghosts, spontaneous human combustion, etc., which, over the ages have been reported on, written about and discussed by a sizeable minority of people. The common denominator of these phenomena is that they are inconsistent with our generally accepted model of reality.

2. Despite this, there are many people who, taking on trust reports of such phenomena, accept (i.e., believe in) the reality of the events in question.

*Reported sensory experiences.

3. There is yet another group of people, many of them scientists (in the normal sense of this term), who, whether or not they believe in the existence of these anomalous phenomena regard the reports of their existence as sufficiently important to warrant study. Members of this group, whether credulous or incredulous, are presumably sufficiently curious and open minded to try and find out, using the normal methods of science (a) whether these, or some of these, phenomena really do exist, (b) their true nature and (c) if they do not exist, their social and psychological origins.

4. Finally there are two further groups of people, the most outspoken being from the community of scientists, who, whether or not they secretly believe in the reality of anomalous phenomena, publicly espouse the view that they are not real. do not exist, and are not (therefore) proper subject matter for scientific study. It is the stated opinion of at least some of these people that those who study anomalous phenomena are "pseudo-scientists." Pseudo-science being "the study of the non-existent by the incompetent."

So much for the general context of Westrum's paper. To the present writer the most interesting feature of his analysis is not the ways of "crypto-scientists," nor the phenomena they study, but rather the light it sheds on what appears to be a strange paradox, namely that those (of the community of scientists) who pride themselves on being the most rational and hard headed of mortals are, it seems, behaving with rather less reason and a great deal more emotion than the pseudo-scientists they castigate. This conclusion is based on four considerations.

1. It is surely the case that, in the history of science, today's anomalies may well turn out to be tomorrows facts. Therefore, to ignore the anomalous, seriously reduces our chances of discovering something new.

2. It is part of the business of science to investigate, understand and explain natural events. But the report (i.e., human testimony) of an apparently, at the time, inexplicable phenomenon is itself a natural event and therefore worthy of study. Indeed whether this event is located in external reality or only in the mind of the percipient is itself a question worthy of scientific inquiry.

3. The proposition that "Crypto-scientists are not only deluded but also incompetent" besides being abusive is either tautologous or probably untrue: i.e., if we define cryptoscientists as mad, then obviously they may well be deluded because this is frequently a feature of some sorts of madness. Similarly, if we know for certain that the object of study is non-existent, then obviously and by definition a belief in the existence of the non-existent is delusional. However, if that is all such propositions are saying then they are so trivial and meaningless as to cast doubts on the sanity of those who put them forward! We must assume therefore that they are based on something rather more positive and profound, i.e., that anomalous phenomena are indeed, and have been proven, non-existent. But this is the very question which the good, as opposed to deranged, Crypto-Scientist is trying to answer. To castigate him for trying to answer a question to which he does not yet know the answer, would be as ridiculous as praising him for wasting his time on a question to which he already <u>does</u> know the answer!

Similar arguments can be advanced in connection with the second half of the proposition which attempts to link non-existence with incompetence. Whether or not the phenomena being studied exist in reality or only in the minds of those who report them (and neither eventuality, as we recall, make them unworthy of study) is surely immaterial to whether or not the person who studies them is or is not incompetent. To aver that a pseudo-scientist is necessarily incompetent is as absurd as suggesting that a "real" scientist is necessarily competent. In fact the relationship,if any,between what a person studies and how he studies it may well be the very opposite of what these propositions imply. Because of the wall of scepticism, prejudice, and downright hostility with which he is confronted, the crypto-scientist has, if anything, to be more rather than less competent than his more respectable counterparts in the scientific community. In this connection, however, Westrum does touch upon a very real problem for the "pseudo-scientist," namely that owing to the prejudice of the scientific community, he is afforded less money, less time and fewer facilities than the "real" scientist. To this extent he may certainly e rendered less competent than he would wish and less competent than those who do not suffer these deprivations.

4. From the examples which Westrum gives of the hostile responses to crypto-scientists, two conclusions may be drawn. First, to the enemies of crypto-science the means of science seem to appear far more important than the ends. Secondly, to many of the same people, the real purpose of science is evidently not to discover more about nature, but, as Westrum points out, to confirm a cherished if inaccurate "reality." One cannot help feeling it is better to preserve an unreal, incomplete, and dated "reality" than run the risk of having to modify it because of some hitherto undiscovered, unthought of, and certainly unwished for truth! That these attitudes are rooted in emotion rather than reason is suggested by the fact that closed minded bigotry is not reserved solely for crypto-scientists engaged in crypto science but may erupt when a real scientist discovers something so new and unexpected that it conflicts with previously held beliefs about the nature of reality. Thus we have the extraordinary behavior meted out to J.L.B. Smith when he discovered the Coelacanth. As Westrum points out, even though he had irrefutable evidence, in the shape of a fish with four legs, "long-time friends avoided him on the street and several people questioned his sanity."

So, what conclusions might be drawn from all this? First, it seems pretty clear that the vociferous minority in the community of scientists reveal by their hostility, intolerance, and general irrationality that they are driven to such undignified extremes by something quite other than a pure search for truth. From studies of such things as conformity behaviour, the resolution of cognitive dissonance, and the psychopathology of conservatism and authoritarianism, it might be surmised that the "something quite other" might include a neurotic fear of failure, a relatively weak grip on their sense of reality, a compulsive desire to order and simplify their world view and, in one case at least, a quite virulent jealousy towards those who, unbridled by feelings of inadequacy and fear of loss of social disapproval can successfully give vent to rational curiosity about anomalous events. (There is of course a much briefer, psychoanalytic, "explanation" of their aberrant behaviour, namely that it is a reaction against voyeurism!).

Secondly, the picture which Westrum paints does not at first sight bode well for progress in science. However, judging from the many eminent scientists who spoke at the recent IBM sponsored London Synposium "Science and the Unexpected," we may well be right in supposing that it tends to be only the more third-rate, unproductive, and uncreative members of the scientific community who are concerned to denigrate the so called crypto-scientists.

COMMENTS BY PIET HEIN HOEBENS:

In his thoughtful and interesting paper, Dr. westrum regales us with what amounts to a sophisticated version of the stock-in-trade argument of the occultist: there is more in Heaven & Earth etc., but pig-headed Official Science refuses to even look.

I do not deny that there is some truth in this complaint. I too have occasionally noticed signs of irrationality if not "pathology" in the orthodox response to deviant claims. Like Dr. Westrum, I am dissatisfied with much that passes for scientific skepticism.

I feel, however, that Dr. Westrum is largely mistaken in his analysis of the problem. In these comments, I will restrict myself to a few points where my disagreements with him seem rather fundamental.

The central theme of "Crypto-science and Social Intelligence about Anomalies" is that the scientific establishment declares "unreal" and covers-up observations that contradict orthodox assumptions. Dr. Westrum presents some anecdotical evidence to support this allegation. For all I know, such things may occasionally happen. However, it is a gross overstatement to claim that it is the rule.

I only need to remind Dr. Westrum of Mr. Corliss' encyclopaedic collection of anomalous reports, culled from Nature, Science and other impeccably orthodox sources. The establishment literature swarms with anomalies. Why then does Dr. Westrum insist that "the anomalous, if noticed, is kept secret"?

Take the example of the Coelacanth. Sensibly, Smith initially questioned the testimony of his own eyes. ("Are there halluncinations in the shape of Coelacanths?" "Am I the victim of a Piltdown-like hoax?") After repeatedly having checked his suspicions, he was left with the conclusion that the specimen was, indeed, a recently living Coelacanth. Perhaps some "long-time friends" have shunned Smith as a result. To me, it seems more relevant that the offensive creature is now completely accepted by the establishment. It was anomalous; it was noticed; it was not kept secret.

Obviously, Dr. Westrum's line of reasoning must have led him astray at some point. I suspect that this happened right in the paragraph where the author presents the idee recue that "science" and even "reality" are "something agreed upon by a given group" and so "may be different for different groups." I do not say that this view is wrong. I only say that it is too limited.

It disregards the qualitative differences between competing models of science and reality. It also fails, adequately to account for those aspects of reality that cut right across paradigmatic boundaries. The law of gravitation may be the outcome of a social negotiation process, but if you jump from the top of the Empire State Building you will drop dead regardless of your metaphysical predilections. No Skeptical Inquirer is needed to protect that part of reality from cryptoscientific subversion. Of course, this is a crude example, but it is relevant to the issues Dr. Westrum discusses.

In his paper, he makes no clear distinction between the factual status of anomalies and the evidential value of anomalous reports. That useful fish, the Coelacanth, may again serve as an illustration of what I mean. The specimen was recognized as a Coelacanth and interpreted as proving the survival of a species previously thought extinct because Smith evaluated his observations in the light of current paleozoological theory. Alternative explanations, however, are easy to think of. Perhaps a Deity with a peculiar sense of humour specially re-created the Coelacanth to tease the scientists. Perhaps the creature had arrived in a Time Machine. Perhaps the Coelacanth was a haddock suffering from a unique disease that makes haddocks look just like Coelacanths. Perhaps the carcass was a paranormal apport.

These interpretations are all consistent with the data. Smith settled for the surviving species hypothesis because that fitted best into the "research programme" or "paradigm" he was working with.

Now it is important to note that the "paradigm" would really have preferred the fish to be non-existent. This, however, was an option reality simply did not allow. The "something agreed upon by a given group" was the evidential value of the discovery, not its genuineness.

Here we may note a crucial difference between two types of anomalies: the Coelacanth type and the "paranormal" type. With type I an anomalous corpus delicti (whether in the form of a carcass or in the form of a repeatable experiment) is available for critical examination. With type II, all we have are reports of "impossible" occurrences. Such reports may contain flaws, perhaps very subtle and unusual flaws. We have no means of checking directly. The true explanation may be that the alleged phenomenon does not exist.

I think that, as a rule, the scientific establishment will not "reject" anomalies of the first type. (That such wonders may be virtually ignored because nobody has the remotest idea what to do with them is an entirely different matter.) What about anomalies of the second type? What about flying saucers, psychic phenomena, ghosts and Abominable snowmen? Here, Dr. Westrum's scenario applies - to a certain extent.

The establishment does not actually reject such reports (in the sense of stating apodictically that they are untrue). What is rejected is the claim that such reports constitute acceptable scientific evidence and are sufficiently compelling to warrant science's undivided attention. Most scientists, I suspect, will be reluctant to become actively involved and invest much time and energy in the investigation of anecdotal accounts of elusive miracles. Such accounts will be tacitly assumed to be irrelevant to science until they are at least backed up by anomalous evidence of the first type. The history of the cryptosciences (e.g. parapsychology and ufology) hardly justifies any optimism about such a breakthrough being imminent. The odds are that the scientists would be wasting their time studying psychics and questioning flying saucer witnesses. It is hardly surprising that they will not want to encourage each other to join in the hunt for what may very well be chimaeras.

Unlike Dr. Westrum I do not believe that this has much to do with an urge to protect society against subversive realities. A UFO crashing on the White House lawn would be far more subversive than a strange nocturnal light claimed to have been observed over Montana. Yet the crashed saucer would be accepted as an anomalous corpus delicti, whereas the nocturnal light would not. Just like the carcass of the Coelacanth was accepted, whereas a mere reported sighting of the fish would have gone the way of the poltergeist and the little green men.

Against Dr. Westrum, I contend that the "facts" that are "rejected" by the scientific establishment are characterised not by their incompatibility with orthodox prejudices but by their ambiguity and elusiveness.

Now Dr. Westrum may agree that anomalous anecdotes are not enough for overthrowing paradigms. If I understand him correctly, his principal demand is that the cryptosciences be given a fair chance to explore the possibilities of alternative research programmes without fear of ridicule or ostracism. More specifically, he demands that the cryptosciences be adequately staffed and funded.

On this point, I sympathize with Dr. Westrum. However, I find it surprisingly difficult to justify my sympathy except by referring to my liberal prejudices. The dilemma I am faced with is that, while I realize that anomalous anecdotes studies by the cryptosciences may contain the key to scientific revolutions, I also realize that only a tiny fraction of the myriad "claims of the paranormal" can be reasonably expected to be important in this respect. To put it plainly and bluntly: for every Coelacanth there are a million red herrings. Funding cryptosciences may well be the least economical way to promote the growth of knowledge.

COMMENTS BY C.L. HARDIN:

"Tales from the Crypto"

ELLIOT: We have to help him (the Extra-Terrestrial) get home.
FRIEND: Why don't they just beam him up?
ELLIOT: (Disgustedly): This is reality!

Dialogue between two young boys from the film,"E.T."

"And only one for birthday presents, you know. There's glory for you!"
"I don't know what you mean by 'glory,' Alice said.
Humpty Dumpty smiled contemptuously. "Of course you don't--till I tell you. I meant 'there's a nice knockdown argument for you!'"
"But 'glory' doesn't mean 'a nice knockdown argument,'" Alice objected.
"Why I use a word," Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean--neither more nor less."
"The question is," said Alice, "whether you can make words mean so many different things."
"The question is," said Humpty Dumpty, "which is to be master--that's all."

Dialogue between philosopher and sociologist from the book, "Through the Looking-Glass."

No matter how explicitly you announce it, "reality" doesn't mean "that state of affairs which a given social group agrees is the case."

It _does_ mean, simply, that which is the case. Is this piece of linguistic deviance willful, or is it the result of innocence about the appropriate use of the mother tongue? With some social scientists I would be uncertain, but Dr. Westrum obviously writes intelligently and felicitously. I conclude that this use of language is deliberate. What's behind it? And what's the harm in it?

The harm in it is that it blurs the important distinction between fact and belief, in much the same way as "true for" does. "It's true for Smith that God exists, but not true for Jones." "It was true for the Babylonians that the earth is flat, but it's not true for us." Did the earth change its shape sometime during a two thousand year period? Does God both exist and not exist? Clarity demands that we forego peculiar locutions that generate gratuitous questions. The existence of God (given a clear job-description for the word "God") does not depend upon the intensity of Jones' or Smith's belief nor upon the evidence or arguments that either may bring to the question. The fact of God's existence (or non-existence) is, rather, that which makes Smith's belief true (or false).

What's often behind the "true for" locution and the deviant use of "reality" is either (1) the tacit epistemological claim that we are incapable of knowing the truth about certain things, or (2) the tacit metaphysical claim that in some cases there is no fact of the matter, or (3) the tacit metaphysical claim that what is the case is constituted by what some person or group asserts to be the case. Partly because they are commonly tacit, these claims are less frequently argued for than their radical character would seem to require. If we restrict ourselves to domains in which science can claim competence--excluding, say, ethics and theology, but including, say, astronomy, UFOlogy, microbiology and crypto zoology--there are not, I would assert, any persuasive arguments for either of the metaphysical claims. I am prepared to defend this view in detail, but will forego doing it here.

What about the first thesis, that in the questions at issue we are incapable of knowing the truth? Although some practicioners of the so-called "strong program" of the sociology of science seem to hold this view, I do not think that Dr. Westrum wishes to endorse it wholeheartedly. Rather than find fault with the epistemological efficacy of the methods and precepts of the natural sciences, he complains about the social attitudes which sometimes pervert their application. His quarrel, then, seems to be with a certain _representing of reality_ that is held by the orthodox practioners of natural science rather than with the assertion that we have objective criteria for assessing such representations for accuracy and adequacy. If this is a fair statement of his position, I call upon him to abjure and detest his misleading semantical practice and replace his deviant use of "reality" with "representation of reality" throughout. On the other hand, we ought not to allow Westrum's opponents to characterize science as "the competent study of what is real," if this is taken to mean that competent scientists, practicing science in a respectable manner never advocate the existence of objects, properties or processes which are not included in the furniture of the world. Many proponents of caloric, a mechanical ether and tachyons cannot be responsibly judged to have thereby engaged in pseudoscience. If there is to be any useful employment of the term "pseudoscience" at all, it must turn on such factors as the manner of investigation and the quality of available evidence rather than merely upon whether the inquiry resulted in true assertions or a correct ontology. For present purposes, and for the sorts of cases that Dr. Westrum is talking about, we might do better to employ a somewhat less judgemental term, "deviant science."

I think that anyone who is inclined to read The Zetetic Scholar at all will deplore savage zeal and ignorant passion in the supposed service of scientific orthodoxy. But an attitude which dismisses Uri Geller, Erich von Däniken, and orthodox astrology can't be all bad. Let's see what can be said in favor of the resistance to deviant science.

First, it represents an efficient use of resources. The garbage density in much stock academic research may often be high, but in deviant science it is typically much higher. If the payoff in, say, psychical research is potentially large, the probability of its paying off is quite small, especially given the standard of research which has characterized most of its history, so the expectation value is correspondingly low.

Second, the history of marvelous claims makes one understandably wary of another bunch that looks for all the world like what has gone before. We feel retrospectively embarassed by the eighteenth century rejection of meteorites, but how similar this was to other claims of wonderous occurrences in the skies! Think of all those swords and severed heads to be seen in comets or, in our own century, the acrobatics of the sun over Lourdes, attested to by thousands of people. Such public testimony, whether tricked up in legal dress or not, is far more likely to yield fruit for psychology or sociology than for astronomy or meterology.

Third, a very great deal of what goes into the accepted scientific picture of the world has been carefully cross-checked by relatively independent procedures. If an anomaly is to challenge such a fixture successfully it must either have such an evidentiary weight as to overbalance these contrary observations, or it must be accompanied by a powerful and persuasive theoretical account which will show that these other observations are only apparently contrary to it. Either of these conditions is typically very difficult to realize in practice, and if they are not realized the rational response to an anomaly is either to explain it away or to put it on a back shelf until the conditions can be met.

For all of that, the beauty of scientific institutions as they have been up until now is that they are resistant rather than impervious to change. Their overall rationality has a corporate character and depends as vitally upon having a minority of dissidents as it does on having a majority of standard practicioners. The minority must be tolerated, and must have access to forums where their views can be heard with some semblance of objectivity. But it does not follow that dissident views require equal time, attention, or financing from the public purse. As a good sociologist like Dr. Westrum well knows, institutions most effectively stifle dissidence by coopting it. Would official dollars for UFO research have any other outcome than a Son of Condon Report?

COMMENTS BY STANLEY KRIPPNER:

"Science as a Beauty Contest: Some Remarks on the 'Cryptosciences'"

Ron Westrum's article contains so many insights and provocative ideas that it is difficult to limit one's discussion. For example, he makes the observation that the "cryptosciences" are not adequately staffed or funded. True, the "cryptosciences" are usually poverty sciences. I have seen parapsychological experimentation carried out with second-hand equipment by investigators who do psi research in their spare time. This situation can not be a defense of shoddy research. However, an awareness of it can be useful in determining what improvements need to be made in future replications and extensions of the work. Long-range planning in psi research, for example, is hampered by the simple fact that none of the major American parapsychological laboratories are assured of funding past the next few years. In addition, high-level conceptualization by researchers is difficult when one has other professional duties which take priority over the time which can be devoted to the study of psi phenomena.

Westrum makes an excellent point when he calls for the use of the term "amateur science." This sobriquet would fit many efforts in the field of psi research by novices as well as those by some outstanding professionals in different vocations who simply enjoy "studying psi" on evenings, weekends, or vacations but without the background needed to do exemplary research. Further, an "amateur scientist" can always, given time, become competent. A "pseudoscientist," however, typically reaches a cul-de-sac from which there is no exit.

Westrum operates from a sociological perspective, one which is badly needed in parapsychology. Thus, he is able to make cogent comments on the way in which the "reality" that a science investigates is typically created by the scientific community. This "reality" resembles the winner of a beauty contest; just as fads and fashions in beauty change over the years and vary in different locations, so does the "reality" which can be legitimately investigated by science vary. Losers of the beauty contest, or of "reality" popularity polls, are considered second-place at best, or unfit for serious consideration at worst. In the 1700s people who saw meteorites would often remain silent because the discussion of falling objects was ridiculed and derided by establishment science.

Psychology, as a science, is considered second-rate by some representatives of the "hard sciences." And in psychology itself, some members of the Psychonomic Society regard the rest of psychology in a manner comparable to the way that most astronomers treat astrology.

If the "cryptosciences" study "things that might be there," we are left with the conclusion that meteorites and coelacanths were studied by "cryptoscientists" until their reality was convincingly demonstrated. Thus, the term "cryptosciences" needs to be examined before it is generally adopted to determine whether it may be prejudicial. Indeed, the discipline that studies anti-matter, black holes, and quarks is not considered a "cryptoscience" even though the reality of these items has not been demonstrated. It is apparent to me that the distinction between "science" and "cryptoscience" is not one of research methodology or competence of the workers in the field but one based on what a society's scientific establishment considers to be proper topics of study. Therefore, the real difference between the "cryptosciences" and the approved "sciences" is sociological in nature; the former investigates the existence of phenomena against which there is a bias among significant numbers of powerful scientists in the society.

COMMENTS BY TREVOR PINCH:

The view that the acceptance and rejection of knowledge claims (including those labelled deviant) in science can be understood with little or no reference to the natural world is by now well elaborated within the sociology of scientific knowledge. From within that tradition the most exciting aspect of Westrum's paper is to be found in his unravelling of some of the mechanisms whereby different "world views" or "realities" can be maintained such that "The deviant experience is thus forced into a separate world, where for the most part it can neither change or be changed by standard reality." It seems that even the most carefully researched reports of the crypto-scientists will not sway the opinions of their more conventional colleagues.

Westrum is particularly illuminating on the subject of experimental competence. He shows that a scientist, who is otherwise highly competent, quickly becomes defined as incompetent when he/she enters the world of crypto-science. Hence it would make little difference if the best talent and the most resources were devoted to crypto-sciences. The failed excursions of respectable institutions and scientists of orthodox pedigree into ESP research (SRI and Hal Puthoff perhaps being the latest) testifies further to this point. What this indicates is that prior belief or dominant conceptions of scientific reality tend to shape attitudes towards the phenomena claimed by crypto-science (or for that matter para-science) - scientific evidence, no matter how good, will not change matters substantially. As Westrum puts it: "Society seems to be able to find out only about what it is willing to accept."

Although I have no quarrel with Westrum concerning the thrust of his sociological analysis I am a little puzzled as to the basis of his speculation at the end of his paper that the open-mindedness of the scientific community might be enhanced by more public participation in crypto-scientific activity. This might be true if crypto-scientific activity were to have any impact on mainstream science. However, if Westrum's sociological analysis is correct it seems that the future of crypto-sciences cannot but be bleak. The fate of institutional rejection, with little or no prospects of convincing orthodoxy, does not seem to me to be a happy recipe for public involvement. Indeed such public involvement might take away the little legitimacy crypto-science has. After all, popularising is widely detested and is a cause of suspicion within mainstream science. Public participation without any scientific successes seems to be the fate of parapsychology today. Perhaps parapsychology should properly be called an "amateur science" rather than a "pseudo science," but it is still a rejected science.

COMMENTS BY GERD H. HÖVELMANN:

"Reality, Relevance, and Responsibility"

Most of Dr. Westrum's arguments are quite suitable to adequately describe the various problems confronting the "cryptosciences," and I find myself agreeing with many of them. Nevertheless, I will comment on some particular points he makes as well as on one further point he fails to make:

(1) Dr. Westrum's insight that "reality" means "that state of affairs which a given social group agrees is the case" (italics added) is of considerable importance. I would even move a step further and hold that this is the only way to make sense of that term. It can easily be shown that the conception of "reality" as it is held by Logical Empiricism of the Vienna Circle[1] and by Critical Rationalism of the Popperian school of philosophy of science,[2] viz. the claim (or more accurate: the assurance) that there exists such a thing as an "objective reality" idependent of any human effort to discern it, must lead to logical inconsistencies. Strictly speaking, it is not even possible to avoid circular argumentation when trying to substantiate that claim. In short, Dr. Westrum's understanding of "reality" (although later in his paper he justly criticizes this understanding for being "responsible for many of the intolerant and often questionable attacks on 'pseudoscience'") is the only reasonable one in a rational discourse. This does imply, moreover, that what is "real" for a given social group is not a mysterious property of the "outer" world but always a lingually constructed system of sentences held to be valid by that very social group for the time being. As Dr. Westrum correctly states, it is a common experience that these different social groups always tend to maintain their respective conception of "reality"[3] since it "plays a certain role in the individual's sense of security."[4]

(2) Dr. Westrum writes:

> "My opinions about the competence and incompetence of my fellow investigators [in UFOlogy; GHH] are gained in ways not dissimilar from the ways in which ordinary physical and biological scientists form assessments of each others' comptence."

Here, I believe, Dr. Westrum is guilty of over-simplification. I doubt that there is a WESTRUM I forming assessments of his fellow sociologists' competence as an academic sociologist at Eastern Michigan University (i.e. as a "normal scientist") and a WESTRUM II forming assessments of his fellow UFOlogists' competence as a UFOlogists (i.e. as a "cryptoscientist"). I am rather convinced that, in the latter case, Dr. Westrum judges the competence of his fellow UFOlogists as a "normal scientist" doing "cryptoscientific" research. It is impossible, I think, to leave one's scientific attitude at the gate-house when doing "cryptoscientific" research and to pick it up again when turning back to "normal scientific" research at the Department of Sociology. And Dr. Westrum himself seems to share my opinion when he says that

> "The logic of what I am doing is (I would argue) within the basic logic of science."

(3) Dr. Westrum lays special stress on the suppressive role social intelligence processes are frequently playing in view of anomalous observations (and I fully agree with everything he says in this respect) as well as on the fact that those who make such anomalous observations are often left with great psychological problems. Many instructive examples illustrating the latter argument are to be found especially in the parapsychological literature (Richet[5]; Beloff[6]; Rogo[7]).

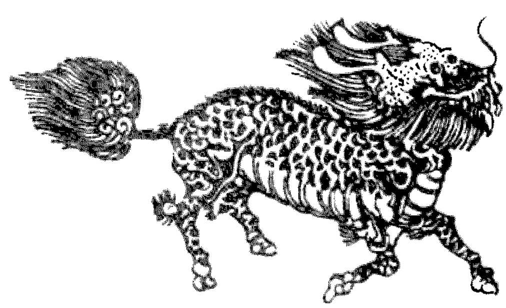
(4) I doubt that Dr. Westrum's concluding speculation that encouraging the "ordinary citizen ... to understand and to participate in the process of science" might help solving the science/pseudo-science demarcation problem is sound. It is true, of course that UFOlogy cannot do without these citizens' observations. But there are some other fields among the "fringe" sciences (such as parapsychology) where obtrusive lay "researchers" who frequently hold untestable metaphysical and supernatural ideas are already over-represented.[8]

(5) Finally, let me turn to one aspect which remains completely disregarded in Dr. Westrum's paper: Dr. Westrum largely complains about the common experience that "present knowledge, particularly knowledge sanctified by constant usage -- and therefore familiarity -- can thus be a major barrier to acquisition of new knowledge." This description is quite correct, of course, but I cannot see why this fact should necessarily be branded. In my view, it should rather be appreciated that new knowledge is only considered as important if it can be shown to be <u>relevant for the actions of human beings</u>. My opinion is that the <u>relevance of "cryptoscientific" research (just as that of "normal scientific" research)</u> has always to be justified <u>with regard to the vital interests of human</u> beings. So, if society (and science) believe that time spent on UFOs is time wasted, the UFOlogist is obliged to demonstrate why it is not (this aspect has been overlooked even by Fleck and Kuhn, and neglected by Feyerabend). Here is the point, then, where the unpleasant suppressive power of social intelligence processes sets in again.

If some "cryptoscientific" investigations cannot be shown to be relevant in one way or the other to the solution of human problems, why, then, should we continue to conduct such investigations? Remember that we are responsible for what we are doing as scientists! The purpose of science is not to find out what kind of world we are living in, or what the destination of man or of the universe is, but rather to help us to meet our vital interests and the requirements of our everyday lives (that is why I have called parapsychologists' participation in survival research into question[9]). In this respect consider Charles S. Peirce's pragmatic maxim[10] which is not at all antiquated. Among other things, Peirce demands that one should always "begin by asking what is the immediate use of thinking about"[11] a thing or a fact. So, I cannot agree with Dr. Westrum's opinion that "the value of such ['cryptoscientific'; GHH] activities seems obvious." I rather believe that this value has to be demonstrated.

Notes:

1. Cf. for instance the journal edited by leading representatives of the Vienna Circle: Erkenntnis/Journal of Unified Science, Vols. 1-8 (1930-1939/40).
2. Cf. Karl R. Popper, The Logic of Scientific Discovery. London: Hutchinson, 1959; Karl R. Popper, Conjectures and Refutations. London: Routledge & Kegan Paul, 1963 (esp. chapter 10); Karl R. Popper, Objective Knowledge. Oxford: Clarendon Press, 1972.
3. Cf. Ludwik Fleck, Genesis and Development of a Scientific Fact. Ed. by T.J. Trenn & R.K. Merton. Chicago: University of Chicago Press, 1979 (esp. chapters 4.3-4.4); also cf. the various papers both in Helga Nowotny & Hilary Rose (eds.), Counter-Movements in the Sciences. (Sociology of the Sciences, Vol. III). Dordrecht: Reidel, 1979, and in Roy Wallis (ed.), On the Margins of Science. The Social Construction of Rejected Knowledge. (Sociological Review Monograph 27). Keele, Staffordshire: University of Keele, 1979.
4. A most interesting psycho-dynamic approach to the frequent "reaction of incredulity" has been submitted by the Italian psychoanalyst Emilio Servadio, "Parapsychologie und 'Unglaubigkeitsreaktion'," in: Zeitschrift fur Parapsychologie und Grenzgebiete der Psychologie 2 (1958): 1-9. Also cf. the results of research in "social perception."
5. Charles Richet, "On the conditions of certainty," in: Proceedings of the Society for Psychical Research 14 (1899): esp p. 159.
6. John Beloff, "Belief and doubt," in: W.G. Roll, R.L. Morris & J.D. Morris (eds.), Research in Parapsychology 1972. Metuchen, N.J.: Scarecrow Press, 1973, 189-200.
7. D. Scott Rogo, "Parapsychology and the genesis of doubt," in: Parapsychology Review 8 (1977): 6, 20-22.
8. Cf. Gerd H. Hövelmann, "Some recommendations for the future practice of parapsychology." Paper presented at the 25th Annual Parapsychological Association Convention, Cambridge, August 1982.
9. Gerd H. Hövelmann, op. cit.
10. In: Charles S. Peirce, "How to make our ideas clear," in: Collected Papers of Charles Sanders Peirce. Vol. V. (Ed. by C. Howthorne & P. Weiss). Cambridge, Mass.: Belknap Press of Harvard University Press, 1960, 248-271 (esp. p. 257).
11. Charles S. Peirce, op. cit., 262.



COMMENTS BY BRIAN INGLIS:

"Cryptoscience"

Why not "parascience?"

I would suggest that a distinction needs to be made between three types of phenomena with which parascience is currently identified.

First -- to take Ron Westrum's examples -- the Lock Ness Monster, bigfeet, and such. They fall into the same category as the coelacanth, in that although they may now be classed as occult, were they to be found they could be fitted into the orthodox scheme of things with little difficulty (and, incidentally, no apology for the derision with which reports about them have been greeted so far).

Second: UFOs. Like meteorites (I am grateful for his quotes from two centuries ago), they may yet be brought into an orthodox sciences framework (again, with no apology). So far, however, it is not possible to say with certainty whether they are "real," in that sense; or -- say --- materialisations; or a form of hallucination.

Third: all forms of psi phenomena. For some reason Westrum leaves these out of account, except for ghosts (which -- or some of which -- may fall into the second class): ESP, psychokinesis, and the ways in which they are exploited, such as divination by table-turning, automatic writing, crystal-gazing, and water-divining (water-witching). All these have a psi component; and it is this which is presenting scientists with a threat. Physics may now be almost ready to accomadate psi; psychology is not. Psychology would, in fact, take a fearful knock if it had to accept extra-sensory communication. Its "controlled" experiments would have to be consigned to history's dustbin.

The question which now faces parapsychologists is whether they should abandon the attempt to convert scientists, and concentrate their attention on the public. The nearest the public has ever come to overruling the scientists was in the table-turning era of the 1850s, when hundreds and thousands of people saw the "laws" of nature being broken in their own homes. Uri Geller started something of the same kind, ten years ago. Public opinion polls now show a massive majority -- around eighty per cent -- of all classes ready to accept ESP.

And then? The father of scientific scepticism, Eusebe Salverte, conceded in <u>The Occult Sciences</u> a century and a half ago that "when the improbability of a fact is the chief objection to the acceptance of its reality, the evidence which attests it regains all its value if the improbability is proved to be only apparent." Doubtless he had meteorites in mind: they had just been accepted. It looks as if ESP and PK are very close to acceptance, whether scientists like it or not.

COMMENTS BY ROY WALLIS:

It may well be that I have not entirely caught Ron Westrum's drift in this piece. As far as I understand his concern, it would seem to be that:

(1) Society doesn't want to hear about things which might be contrary to prevailing conceptions of reality.

(2) It therefore stigmatises those who pursue such knowledge as "pseudo-scientists" and the like.

(3) It refuses to fund such activities.

(4) For reasons (1)-(3) the best scientists do not involve themselves openly in such activities.

(5) This closes off to society potentially valuable knowledge (and somehow subverts democracy).

I wonder what to make of this. Anyone who has hung around the occult milieu as much as Ron Westrum and I, and probably most readers of this journal, will know that ideas are more plentiful than money. Hence there has to be a mechanism for the allocation of scarce resources and this will inevitably - and I see no inherent menace in this - depend upon the expert opinion of existing established scholarly opinion. So some potentially interesting activities are going to rank low on the priority scale in any social order I can conceive.

Scientists are only human after all and have the same problem of making a living, a reputation, etc as the rest of us. They will, therefore, seek to pursue their careers in fields which promise returns in these desired values. For all that, science - despite what the recent enthusiasm for a highly consensualist view of the institution has led some to believe - is a remarkably liberal world. It permits a substantial amount of "moonlighting" from allocated research funds, and any scholar or scientist with enterprise will early become accomplished at "creative accounting". It possesses mechanisms for sanctioning deviants undoubtedly, but they are less rigorous and punitive than in almost any institution outside the occult milieu. And the fact is that we do now accept that meteorites and coelacanths exist. Maybe when Westrum and his colleagues come up - through "moonlighting", working weekends, etc - with as good evidence for UFOs and the like, we'll accept those too.

My biggest problem with Westrum's article is that he seems to perpetuate conceptually the attitudes and actions he deplores by his distinction between "science" and "crypto-science". Those attitudes and actions are predicated upon the assumption that science studies what is real, what is actually there, while other activities, variously labelled "pathological science", "pseudo-science" or "crypto-science" (depending on how hostile the labeller is to the activity) only study things which aren't there, or only might be.

Science is an enterprise identified by pursuit of the truth through logical and empirical critical appraisal, not in terms of any characteristics of its subject matter - even whether that subject matter exists or not. The individuals involved were doing science even though phlogiston and n-rays (probably) do not exist. Some science is well endowed and respectable, other science is poor and marginal. Stratification in the pursuit of knowledge is as likely an inescapable fact of life as stratification in the rest of society.

Now, of course, some subject matter is more resistant to logical and empirical critical appraisal than other subject matter. Who would know that better than sociologists such as Westrum and me! And in the absence of other popular appeal, or redeeming features - such as possible relevance to the training of people who are useful rather than merely decorative such as social workers, survey researchers, civil servants and the like(!) - those activities will doubtless be less well-funded than nuclear physics. So what? But then, perhaps I have not understood Ron Westrum's central thrust.

COMMENTS BY WILLIS W. HARMAN:

It seems to me that such terms as "pseudoscience," and perhaps even "cryptoscience," are less than helpful. If we approach our scientific explorations with appropriate humility in the first place, such appellations are unnecessary because all exploration, however well-meaning and competently done, risks turning out to be "pseudoscience," and study of any unexplained phenomena could be temporarily "cryptoscience." The Neo-Darwinist "phenomenon" or origin of species through random mutation and natural selection is perhaps one of the better examples of a presumed phenomenon, "cryptoscientifically" studied and accepted by generations of biologists, which in the end turns out to be very dubious indeed. On the other hand, it appears that although they are typically reluctant to discuss the matter with a skeptical inquirer, a very significant fraction of educated American adults have had some form of "out-of-body" experience in which they were apparently able to "see" things clairvoyantly which were not possibly visible to their physical eyes. Yet study of this phenomenon is all too easily stigmatized as "pseudoscience" because it appears to violate the contemporary concept of what is physically possible.

Scientists no less than other mortals are vulnerable to the sort of self-deception occasioned by the psychodynamic defense mechanisms known as "resistance" and "denial." It is not shameful to share these failings with our fellow-humans. When we are appropriately humble about our truth-seeking activities, we will find less occasion for namecalling of the "pseudoscience" variety. As Kierkegaard pointed out long ago, there are two ways to be fooled -- to believe something that isn't so, and to refuse to believe something that is so. All of us are vulnerable to both, and all can help in the correction.

EDITOR'S NOTE:

ZS readers may find the following paper of relevant interest: Willis W. Harman, "Human Consciousness Research: Problems and Promises of an Emerging Science," paper presented at the annual meetings of the American Psychological Association, 1981. The paper is available through the Institute of Noetic Sciences, 2820 Union St., San Francisco, CA 94123.

COMMENTS BY J. RICHARD GREENWELL:

I found Dr. Westrum's paper to be well thought out and insightful. I have no specific criticisms, but I would like to relate his observations to a very interesting psychological phenomenon known as conformity.

It is conformity which keeps society's trains running, so to speak, and, of course, it is critical for the continued existence of civilization. At a certain level, however, it may begin to suffer from diminishing returns. One way to look at science is to perceive it as a multi-varied activity conducted by persons more-or-less in the same peer group, and it is in peer groups that conformity becomes more pronounced. There are so many sub-disciplines in science, however, that the conformity effect can be diluted. Although an invertebrate physiologist, for example, would certainly expect a geomorphologist to behave "like a scientist" -- whatever the invertebrate physiologist's perception of "science" is -- he or she might not be altogether understanding of correct theory or methodology in geomorphology, or even what geomorphology is!

Consequently, the invertebrate physiologist and the geomorphologist share only a common understanding of what science is -- or should be -- beyond that they may sound completely illiterate to each other. The invertebrate physiologist would certainly be reluctant to challenge the geomorphologist on whether some aspect of the latter's work is "legitimate" science.

This interaction could be referred to as an "interdisciplinary" relationship. The situation changes as one moves down the scale, disciplinary speaking. One could collect a group of persons within the field of earth sciences, such as a geomorphologist, a hydrologist, a seismologist, a petrologist, and a paleontologist. They certainly would have a much better understanding of each other's fields than the invertebrate physiologist would have of any one of them. They would be more apt to critique procedures used by persons in these allied fields. Such a relationship is still generally called interdisciplinary, which is sometimes questionable (I am reminded of an "interdisciplinary" academic workshop once held at the University of Arizona; upon closer scrutiny, I found that the participants were civil engineers, chemical engineers, electrical engineers, mechanical engineers, nuclear engineers, and systems engineers; as far as I was concerned, they were just a bunch of engineers sitting around a table!)

At the next level, the situation changes dramatically. What before was an "interdisciplinary" relationship is now an "intradisciplinary" relationship. All individuals are in the same subdiscipline, they will be highly competitive, they will be watching the every move of others within the sub-discipline, and they will be very wary and sensitive of criticisms made by others on their work. They will tend to conform fairly rigidly to the norms expected of them within the sub-discipline.

It is never written, and rarely stated, but individuals producing anomalous data, or proposing the study of anomalous events, will, in time, be gradually "punished" by other sub-discipline members for their non-conforming behavior. (Dr. Westrum has pointed out one way of determining the criteria for deciding what is anomalous, based on Fleck.)

Curiously, there is usually a "grace period" during which it is hoped that the nonconforming individual will reform and come to his/her senses on his/her own accord. If that does not eventually happen, however, the nonconforming behavior of the renegade is often all that is judged, the information generated, whether correct or not, being dismissed without adequate analysis and evaluation.

This enforced conformity is not just a theoretical model devised by bored psychologists. It is also very real, as a number of scientists who have suffered professionally over their cryptoscience involvement can attest. I personally know one scientist who was verbally informed that a full professorship will never be forthcoming (at least at that institution) due to his cryptoscientific work -- dispite the fact that his "legitimate" work is considered superior. Another individual once told me (in confidence) that she would like to pursue research in a cryptoscientific area within her sub-discipline, but that she "cannot afford to do so."

It is also important to understand that the conformity phenomenon operates at greater strength the higher the number of individuals involved. This functional relationship has been demonstrated in a large number of experiments. Individuals will thus be much more apt to "close ranks" and condemn a cryptoscience at, say, a professional meeting or conference than in a more private discussion with just a few individuals.

Thus, the cryptosciences (those that have generated any data at all) have to validate themselves with little or no help from their "parent" disciplines. While this validation is sometimes accomplished after decades of battle and debate, imagine the difficulty confronting those cryptosciences which have no "parent" discipline!

We should also remember that areas of inquiry considered legitimate or important are often so recognized only because of historical circumstance. The principal reason why psychiatry is more esteemed than clinical psychology, for example, is because it "arrived" first, and has been supported all along by a powerful medical establishment. Some cryptosciences, one could speculate, are perhaps not investigated by professionals today simply because of now long-forgotten circumstances or events which occured a century or more ago.

It is thus probable that the conformity phenomenon is causing a postponment in the acquisition of new knowledge in certain areas of scientific inquiry. This has certainly happened in the past, and there is little evidence to support the notion that this aspect of human behavior has suddenly changed in the second half of the twentieth century.

I think we can safely assume that "disciplinary conformity" is going to be around for a long time to come. Perhaps, however, this will not be such a bad thing. We have to keep the trains of science running, even if some are late on occasion.

COMMENTS BY MORRIS GORAN:

At the very start, spontaneous human combustion, ghosts, Loch Ness monsters and Bigfoot are identified as the subject matter of "cryptosciences." I looked forward to a discussion of these intriguing topics but found nothing. Nonetheless I did appreciate Westrum's candor in confessing to be a ufologist.

I am not all sympathetic to his invading the territory of rock hounds, variable star observers and other amateur scientists. This last phrase has always been used for unpaid naturalists dealing with traditional science material and never for those dealing with anomalies.

Changing the title from "cryptoscientist" to amateur scientist does not change the facts. Pursuers of ghosts and the like do so at their own volition and risk. No one propels them into the area. When engrossed they encounter the same problems of anyone living extensively with their subject. Other obligations and responsibilities suffer, recognition is not always there, and funding is never enough. Artists, philosophers, hobbyists and those who take up the cudgel for unpopular causes know this as well as the "cryptoscientists."

The latter erroneously compare themselves to Galileo and Semmelweis, with the unstated assumption that in due time truth will out and "cryptoscientists" will be honored and accepted. The similarity in the case of Westrum and some others is more to Isaac Newton, Alfred Russel Wallace and Sir William Crookes. These were accomplishers in their own field who were enmeshed in pseudoscience. Richard S. Westfall has indicated that the attraction idea in the law of universal gravitation may have arison from Newton's immersion in alchemy.[1] Westrum's professional work appears to be related to his UFO interest.

If Westrum is unhappy about UFO study being called a pseudoscience, he can take refuge in the contention that the label pseudoscience "has played an ideologically conservative and morally prescriptive social role in the interests of that order."[2] Or he and others can diligently knock on the door of the American Association for the Advancement of Science. Perhaps some Margaret Mead will come along and push them into the society; of course, ten years after their supposed "approval," a John Wheeler will come along and ask for their dismissal. Or why not simply call the subject crypto matter and avoid the claim to be science? It may be pioneering to form a new unit away from the discipline being accused of contributing to environmental pollution, war horrors and inhumanity.

NOTES

1. Richard S. Westfall, "The Influence of Alchemy on Newton" in M.P. Hanen, Margaret J. Osler and Robert G. Weyant, Eds., Science, Pseudo-Science and Society, Waterloo, Ontario, Canada: Wilfrid Laurier University Press, 1980, p. 145ff.
2. Roger Cooter, "Deploying Pseudoscience Then and Now" in Hanen et al., p. 237.

COMMENTS BY GERALD L. EBERLEIN:

According to Westrum's definition of pseudo-science, his definition of "crypto-science" could be: "Crypto-science is a study of what is possibly real by the competent and/or the amateur scientist." This demarcation between science and pseudo-science seems to run into at least three problems.

First problem: Understanding crypto-science literally as research in (as yet) hidden phenomena or anomalies, renders the problem of demarcation between crypto-science and science more difficult. Doesn't science seek (as yet) hidden facts, anomalies, explanations? Following its traditional meaning, the investigation of hypothetic anomalies is as much the task of normal science as are the testing and explanation of factual anomalies. The demarcation between science and pseudo-science begins to be unclear just at this point. The discovery of the Coelacanthus or the scientific explanation of meteorites are not instances of crypto-science, but of successful zoological and astronomical research. Wouldn't it be more reasonable to speak of "parascience" instead of "crypto-science"? Demarcation between accepted sciences and "parasciences" as "side" sciences (literal meaning of para-science) would become clearer and less pejorative than the term "pseudo-science."

Second problem: Certainly consensus is a necessary criterion for the definition of a scientific situation, but not a sufficient criterion for sound research ("normal science"). Criteria of intersubjectivity of the "scientific method" have to be added. Only the cognitive interaction between Player 1 (scientist) and Player 2 (nature, society, culture), is apt to generate objective information as a result.

Westrum seems not to consider the difference between normal and revolutionary science. The latter gives a key position to hypothetical or factual anomalies not corresponding to the accepted paradigm ("disciplinary matrix"). The pursuit of revolutionary science implies that the traditional consensus about "scientific reality" is questioned or even shattered, while the new "reality" has not yet been accepted. Given this situation, actual, not to mention hypothetical anomalies of a changing science,are not yet recognized; and just this seems to be the problematic situation of the crypto-sciences and crypto-scientists.

Third problem: Westrum explicitly admits "amateur science" and "non-scientist observers." Thus he casts doubt on the originally required condition of competence, and comes dangerously close to his definition, "pseudo-science is a study...by the incompetent." A differentiation between "scientist" and "non-scientist observer" is inconceivable, either from a practical or a philosophical point of view. Aside from the fact that theoretical elements are always implicit in observations, trained scientific imagination is just as essential as a trained theoretical background for scientific observation. Only these two conditions protect the observer from subjective fantasies, i.e. actually becoming a pseudo-scientist himself.

COMMENTS BY ROGER W. WESCOTT:

"Caution, Courage, and Temerity in the World of Science"

Westrum does well, I think, to emphasize the fact that science as we know it is not an autonomous product of abstract intellect but the fluctuating output of a complex social system. Like all such systems, science--as represented, in this country, by the membership of the American Association for the Advancement of Science and its constituent professional groups--has its leaders, its followers, and its outcasts. To be accepted as a scientist by most other scientists, one must accept the current intellectual consensus in one's specialty, as manifest in its regnant paradigm or paradigms.

Psychologically, the need for such acceptance seems to be just as strong in the scientific community as in any other social group. And acceptance is usually sought with avidity, even when it may contradict the glittering generalities of the profession, such as personal open-mindedness, political disinterestedness, and intellectual curiosity. If such contradiction occurs, as it commonly does when new theories are advanced or new areas of investigation are proposed, the typical response is the most familiar one in anxiety-provoking situations: that of denial.

Denial itself can take various forms in the world of science. One is refusal to accept new data as valid. Another is refusal to accept inferences based on those data as reasonable. And a third is refusal to regard the proponent of the data or inferences as reliable. When these refusals are combined, as not infrequently happens, they sometimes take on the self-contradictory quality of the defense of a man accused of stealing a pocket-watch, who reportedly responded, "In the first place, nothing was taken. In the second place, the thief wasn't me. And, in the third place, I intended to return it anyhow."

Sometimes the denial is subtler but more general, as in the cases of the two well-known scholars cited in Westrum's third footnote. Although one showed more willingness than did the other to explore anomalous data, both agreed that fear of ostracism plays no part whatever in a scholar's choice of subject-matter. In such cases, it is hard to avoid the conclusion that denial of fear is itself motivated by fear, even if the nature of that fear remains uncertain. When conscious, it may be the fear that laymen will discover how ignobly motivated scientists are and refuse to continue granting them special status and perquisites. When unconscious, it may be the fear that, if the pettiness of the scientific community is squarely faced, the scientist who faces it will lose interest in continuing his own scientific work.

As regards the psychology and sociology of science, there are apparently no major issues on which I differ from Westrum. There are, however, two concepts to which he refers as if they were self-defining, although both seem to me to be problematic. One of these is "existence," and the other is "impossibility." To the extent that existence is a characteristic of objects rather than of events, I think that it may be legitimate to deny existence to some quite familiar phenomena, such as lightning, as well as to some disputed ones, such as spontaneous human combustion. In neither case, of course, does this procedure constitute a dismissal of the phenomenon in question. What it does constitute is a reclassification of both phenomena from the category of entities to that of occurrences.

Impossibility is a concept about which I am even more dubious than I think Westrum is. For I know of no means by which it can be demonstrated. Possibility, to be sure, is a different matter. To prove that something is possible, one need only demonstrate that it has occurred or existed at least once. But, to prove it impossible, it is never enough to demonstrate that no reputable reporter has observed it, since the next observation may always yield a contrary result. (This principle is one to which I refer as "the improbability of impossibility."[1])

I concur with the admiration that Westrum obviously feels for those scientists who have the social and political courage to study anomalies in a scholarly community that shuns them. But, in addition to this kind of courage, I would also commend purely intellectual courage. While the distinction between these kinds of courage is doubtless gradient rather than discrete in nature, I think that it is worth making. Social and political courage is public courage, requiring the emotional strength needed to stand-up against external dissuasive pressure. Intellectual courage, on the other hand, is private courage, requiring the emotional strength needed to stand up against inner doubts and misgivings. It typically involves the resolution needed to pursue a new idea to its logical conclusion, even when that conclusion clearly contravenes prior beliefs. Of the two types of courage, the latter probably plays a more crucial part in increasing knowledge and the former in disseminating it.

Yet even the nature of courage is not self-evident. There is no clear line that separates courage from prudence on the one hand or rashness on the other. My own feeling is that the courage best suited to scientific discovery is the kind which verges on temerity but that the courage best suited to scientific verification is the kind which verges on cautiousness. Nonetheless, variable as courage may be, depending on circumstances, I concur with what I perceive to be the implicit value-premise of Westrum's article: that, in addition to such well recognized scientific virtues as objectivity and respect for evidence, we should acknowledge the equal importance of scholarly courage and zest for cognitive adventure.

REFERENCE:

Roger W. Wescott, "Paranthropology" (esp. pp. 342-343), in Extrasensory Ecology: Parapsychology and Anthropology, Joseph K. Long, ed., Scarecrow Press, Metuchen, N.J., 1977.

DR. WESTRUM WILL RESPOND TO HIS COMMENTATORS IN A FUTURE ISSUE (ZS#11).

CHINESE PARAPSYCHOLOGY

A BIBLIOGRAPHY OF ENGLISH LANGUAGE ITEMS

COMPILED BY MARCELLO TRUZZI

BACKGROUND MATERIALS

Brown, L.B., Psychology in Contemporary China. Oxford: Pergamon Press,1981.

Chin, Robert, and Ai-Li S. Chin, Psychological Research in Communist China, 1949-1966. Cambridge, Mass.: M.I.T. Press, 1969.

Ching, C.C., Psychology in the People's Republic of China," American Psychologist, 35 (1980), 1081-1084.

Culliton, Barbara J., "Science in China," Science, 206 (26 Oct. 1979), 426-428 & 430.

David, E.E.. Jr., "China: Objectives, Contradictions, and Social Currents," Science, 203 (9 Feb. 1979), 512-515.

Gardner, Howard, "China's Born-Again Psychology," Psychology Today, August 1980, pp. 45-50.

Kessen, William (editor), Childhood in China. New Haven, Conn.: Yale University Press, 1975.

Munro, Donald J., The Concept of Man in Contemporary China. Ann Arbor: University of Michigan Press, 1977.

Orleans, Leo A.,"Science, Elitism, and Economic Readjustment in China," Science, 215 (29 Jan. 1982), 472-476.

----- (editor), Science in Contemporary China. Stanford, Cal.: Stanford University Press, 1980.

Restivo, Sal P., "Joseph Needham and the Comparative Sociology of Chinese and Modern Science," Research in Sociology of Knowledge, Science and Art - Vol. II. (JAI Press), 1979, pp. 25-51.

Richter, Maurice N., Jr., "Chinese Science in the Cultural Revolution: A Reinterpretation," paper presented at the annual meeting of the Society for the Social Studies of Science, Atlanta, November 1981.

Stevenson, Harold W., "The Saga of Chinese Psychology," Contemporary Psychology, 27 (Oct. 1982), 773-775

GENERAL ARTICLES

Anonymous, "Visiting Speakers at the FRNM: Zheng Reviews Psi Research in China," The FRNM Bulletin, No. 28, July 1982, pp. 5-6.

-----, "Peking Can't See ESP," San Francisco Examiner, Feb. 25,1982, p. A16.

-----, "The Parapsychology Controversy," China Reconstructs, June 1982, p. 51.

Chen, Xin, and Mei Lei, "Study of the Extraordinary Function of the Human Body in China," paper presented at the Annual Meeting of the Parapsychological Association held jointly with the Society for Psychical Research, Trinity College, University of Cambridge, August 20, 1982. (Xerox.)

Ebon, Martin, "Parapsychology in Contemporary China," Parapsychology Review, 12, 5 (1981), 1-5.

-----, "China Opens the Door to the Paranormal," Fate, February 1982, pp. 69-76.

He, Chengying, "Overview of Psi Studies in China During the Past Three Years," Shanghai, May 10, 1982. (Xerox, disributed at the 1982 meetings of the Parapsychological Association.)

Kiang, T., "Sighted Hands: A Report on Experiments with 4 Chinese Children to Test Their Ability to See Colour Pictures and Symbols with Their Hands," Journal of the Society for Psychical Research, 51 (1982), 304-308.

Krippner, Stanley, et al., "Summary Statement of the Committee for the Study of Exceptional Human Functions," October 30, 1981. (Statement for the press by the parapsychology study group visiting China upon their return to the U.S.)

Puthoff, Harold E., "Report on Investigation into 'Exceptional Human Body Function' in the People's Republic of China," paper presented at the Annual Meeting of the Parapsychological Association held jointly with the Society for Psychical Research, Trinity College, University of Cambridge, August 20, 1982 (xerox).

Shum, J.C., "Reading Without the Eyes -- Report from China," Bulletin of the British Psychological Society, 34 (1981), 125-126.

Teng, Lee C., "Letter" (Report of witnessing of eyeless vision by Chinese boy), Society for Psychical Research, 51 (Oct. 1981), 181-183.

Wen, Chun, "China's 'Super Children' Claim Amazing Powers," manuscript for China Features, P.O. Box 522, Beijing, 1981. (Xerox.)

Zheng, Rongliang, "Parapsychology in China," paper presented at the Annual Meeting of the Parapsychological Association, Syracuse University, August 21, 1981. (Xerox.)

Zheng, She, "Parapsychology: Is It Real?" China Reconstructs, January 1981, pp. 50-51.

TRANSLATED RESEARCH

A. From Nature Journal (Ziran Zazhi)

Nature Journal, 3, 4 (April 1980). Translation Division, Foreign Technology Division, WPAFT, Ohio 45433. (FTD-ID (RE) T-1766-80.) 9 articles. Also available from The Mobius Group.

He, Chongyan, "Investigations of the Profound Mysteries of Biotics -- Opening Speech at the Human Body Special Functions Scientific Discussion Meeting"

Chen, Shouliang, and He Muyan, "Investigative Report on Special Sensing Mechanisms in the Human Body (1)-- The Question of the Authenticity of Special Sensing Mechanisms"

Xu, Xinfang, et al. (written by Zhang, Lihong), "Investigative Report on the Mechanism of the Recognition of Characters and the Discrimination of Colors with the Ear"

Xie, Yuyu, et al., "Brief Summary of Observations on Xie Chaohui's Use of the Ear to Recognize Characters and Discriminate Pictures and Colors"

He, Dahua, et al., "Investigative Report on Tang Yu's Discrimination of Colors and Recognition of Characters with the Ear"

Chen, Souliang, et al., "Decline and Recovery of Jiang Yan's Special Sensory Mechanism"

Reporter for Nature Journal, "Observation Report on the Non-Visual Recognition of Images"

Luo, Dongsu, "Discussion of Non-Visual Recognition of Images and the Electromagnetic Sensor Mechanism in the Human Body"

Zheng, Feng, "New Advances Made in Study of Exceptional Human Functions," Nature Journal, #8 (August 1980), p. 606. Translation available from The Mobius Group (2525 Hyperion Suite 8, Los Angeles, CA 90027).

Zhao, Yongjie, et al., "Biodetector Experiments on Human Body Radiation Physics," (from Nature Journal, 4, 8 (August 1981)), Psi Research, 1, 1 (1982), 77-84.

Zhu, Nian Lin, et al., "The Primary Measurement of Mechanical Effects of Paranormal Function of the Human Body" (Abstract only), Nature Journal, 4, 5 (1981), 348. (Available from H.E. Puthoff at Stanford Research Institute International.)

B. From the Human Body Exceptional Function Newsletter

Qian, Xue Sen, "Some Theoretical Ideas on the Development of Basis Research in Human Body Science" (June 30, 1981 issue), Psi Research, 1, 2 (1982), 4-15.

C. Other Articles

Special Physics Research Team, Institute of High Energy Physics, Beijing, "Exceptional Human Body Radiation," Psi Research, 1, 2 (1982), 16-23,

Hsu, Hung-Chang, and Zhao Yun-Je, "An Approach to Psi Radiation Signals," Institute of High Energy Physics, Beijing University, 1982. (Xerox, distributed at the Annual Meeting of the Parapsychological Association, 1982.)

Zhao, Uyn-Je, and Hsu Hung-Chang, "EHBF Radiation: Special Features of the Time Response," Institute of High Energy Physics, Beijing University, 1982. (Xerox, distributed at the Annual Meeting of the Parapsychological Association, 1982.)

RELEVANT OTHER SOURCES

Acta Psychologica Sinica (official journal of the Chinese Institute of Psychology of the Academia Sinica, reestablished in 1977).

Xinli Kexue Tongxun Xuebao (Psychological Science Newsletter; also published by the Chinese Institute of Psychology)

[Both of these can be ordered through the Joint Publishing Co.; (Hong Kong Branch), 9 Queen Victoria Street, Hong Kong.]

DEBUNKING BIORHYTHMS: A Supplement *

IVAN W. KELLY

Bradshaw, C.W. Validity of biorhythms for predicting death. The Journal of Psychology, 1982, 110, 39-41.

Demuth, P. Wobbly biorhythms. Human Behavior, April 1979, 53-55.

Dezelsky, T.L., & Toohey, J.V. Biorhythms and the prediction of suicide behavior. Journal of School Health, September 1978, 399-403.

Haywood, K.M. Skill performance on biorhythm theory's physically critical day. Perceptual and Motor Skills, 1979, 48, 373-374.

Hines, T.M. Biorhythm: A critical review. The Skeptical Inquirer, 1979, 3, 4, 26-36.

Holmes, D.S., et al. Biorhythms: Their utility for predicting post-operative recuperative time, death, and athletic performance. Journal of Applied Psychology, 1980, 65, 2, 233-236.

Hunter, K., & Shane, R. Time of death and biorhythmic cycles. Perceptual and Motor Skills, 1979, 48, 220.

Khalil, T., & Durucz, C. Biorhythms. In G.O. Abell & B. Singer (Eds.) Science and the Paranormal. New York: Charles Scribner's Sons, 1981, 105-118.

Latman, N.S., & Garriott, J.C. An analysis of biorhythms and their influence on motor vehicle fatalities. Accident Analysis and Prevention, 1980, 12, 283-286.

Lyon, W.S., et al. Biorhythm: Imitation of science. Chemistry, 1978, 51, 3, 5-7.

Owens, A.J. On the use of fourier transforms to explore biological rhythms. Byte, April 1981, 314-326.

Prytula, R.E., et al. Studies on the perceived predictive accuracy of biorhythms. Journal of Applied Psychology, 1980, 65, 6, 723-727.

Schneider, W. von. Statistiche uberprufung biorhythmischer daten aus unfalluntersuchgen. Arbeit and Leistung, 1967, 7, 125-127.

Trinkaus, J.W., & Booke, A.L. Biorhythms: Another look. Psychological Reports, 1982, 50, 396-398.

Wolcott, J.H., et al. Correlation of general aviation accidents with the biorhythm theory. Human Factors, 1977, 19, 283-293.

Wolcott, J.H., et al. Correlation of choice reaction time performance with biorhythmic criticality and cycle phase. Aviation, Space, and Environmental Medicine. 1979, 50, 1, 34-39.

Wood, L.A., Drider, D.W., & Fezer, K.D. Emergency room data on 700 accidents do not support biorhythm theory. Journal of Safety Research, 1979, 11, 4, 172-175.

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* To ZS#1, p. 47.

ON "PATTERNS OF BELIEF IN RELIGIOUS, PSYCHIC AND OTHER PARANORMAL PHENOMENA"

CHARLES SULLIVAN

Sobal & Emmons (1982) present much useful data concerning paranormal beliefs in an excellent large sample. However, their analysis of the correlations between beliefs and also of the underlying dimensions of paranormal belief seems invalid because the correlations between beliefs are dependent on the overall frequency of each belief in their sample.

The overall percentage of believers for different phenomena varied widely from 10.4% (witches) to 65.1% (life after death), respondents having simply indicated belief or non-belief. It seems that Sobal & Emmons calculated Pearson product-moment correlation coefficients on such dichotomous data (these coefficients are probably better known as phi coefficients) without realizing that they are severely affected by the wide variation in the overall frequency of each belief. For example, given that 10.4% of the sample believed in witches and 65.1% believed in life after death, the maximum possible correlation coefficient between these two beliefs is not the usual 1.0, but merely 0.25. Table 1 shows the maximum possible correlations between every pair of beliefs given their overall frequencies (calculations following Guilford, 1965, p. 336). Essentially, two beliefs can only correlate highly when their overall frequencies are similar. As Table 1 shows, fully one third (23 out of 66) of the coefficients are restricted to values of 0.50 or less.

Thus the correlation coefficients given by Sobal & Emmons (in their Table 2, p. 15) are not really directly comparable. For instance, it seems unwise to simply emphasize the high correlations for belief in angels of $r = 0.70$ and 0.43[1] with belief in devils and life after death respectively (p. 9), when the maximum possible correlation with belief in angels for 4 of the 9 remaining variables is less than 0.40.

Obviously the subsequent factor analysis based on such correlation coefficients is highly questionable. More specifically the overall frequency of each belief can be seen as follows to make an artefactual contribution to the three very plausible factors extracted. Sobal & Emmons (p.8) demarcate three levels of overall frequency of belief: "In general, there was a high level of belief in the religious phenomena of life after death (63%), angels (54%), and devils (39%), along with ESP (50%). A lower level of belief existed for precognition (37%), astrology (29%), deja vu (29%) and clairvoyance (24%), and there was a very small amount of belief in the Loch Ness Monster (14%), Sasquatch (13%), ghosts (12%) and witches (10%)." These three groups found by considering the overall frequency of each belief correspond almost exactly to the grouping of beliefs shown by the three 'factors' extracted (religious, psychic, other beings). This leads one to realize that even if there was really only one general factor underlying all 12 beliefs, a factor analysis of the phi coefficients could well yield three 'factors' similar to those found by Sobal & Emmons simply because there were three groups of beliefs differing in overall frequency i.e. because correlations <u>within</u> each of the three groups of beliefs (thus between beliefs of similar overall frequency) would be relatively unrestricted while correlations <u>between</u> groups would be more restricted.

Finally, there is a little other evidence that religious beliefs tend to be independent of other paranormal beliefs. In a small unpublished study by the author (conducted and anlysed while unaware of the work by Sobal & Emmons), 124 New Zealand students rated their confidence in 13 beliefs on a 9-point scale. The confounding effects of dichotomous variables for subsequent analysis were thus clearly avoided.[2] A Principal Components Analysis suggested that underlying the beliefs were a general superstitious belief factor[3] and an orthogonal religious factor.[4]

Table 1: Maximum possible phi coefficients between beliefs

	Angels	Devils	Life After Death	Loch Ness Monster	Sasquatch	Witches	Ghosts	Astrology	ESP	Precognition	Deja Vu	Clairvoyance
Clairvoyance	.52	.70	.43	.72	.70	.58	.65	.87	.55	.72	.86	
Deja Vu	.60	.81	.49	.62	.60	.50	.56	.99	.64	.84		
Precognition	.72	.96	.59	.52	.50	.42	.47	.83	.76			
ESP	.94	.79	.77	.40	.38	.32	.36	.63				
Astrology	.60	.80	.49	.63	.61	.51	.56					
Ghosts	.34	.45	.28	.90	.93	.90						
Witches	.30	.41	.25	.81	.84							
Sasquatch	.36	.49	.30	.96								
Loch Ness Monster	.37	.50	.31									
Life After Death	.82	.61										
Devils	.75											

NOTES:

*The author would like to thank Richard Kammann and Jack Clarkson for their excellent advice.

[1] Using the figures in the correlation matrix on p. 15 of Sobal & Emmons.

[2] Admittedly several of the variables were skewed, a problem somewhat analogous to that of differing overall frequencies of belief with dichotomous data.

[3]High loadings for 10 items, concerning ESP, spirits in haunted houses, astrology, black cats etc., UFO's, personal psychic powers, auras, Tarot cards, biorhythms, chariots of the Gods (von Daniken's theory).

[4]High loadings for the three remaining items, concerning God, the evolution of man, possession by spirits or demons.

REFERENCES

Guilford, J.P. Fundamental Statistics in Psychology and Education. New York: McGraw-Hill, 1965. (4th edition)

Sobal, J. and Emmons, C.F. Patterns of Belief in Religious, Psychic, and other Paranormal Phenomena. Zetetic Scholar, No. 9, 1982, 7-17.

J. SOBAL & C.F. EMMONS REPLY:

We would like to make several points about Charles Sullivan's (1983) comments about our paper on patterns of paranormal belief (Sobal and Emmons, 1982). He questions the portion of the paper examining the relationships between paranormal beliefs and dimensions underlying the beliefs. The disaggregation of the beliefs by demographic attributes of the survey respondents stand independently of his comments.

We were highly aware of the problems involved in using dichotomous variables in correlational analysis. However, there are two methodological "camps" with respect to this practice, and it appears that he falls into one and we are practitioners of the other. Psychologists generally use experimentally based data and have a great deal of control over dat-collection, permitting them to be very strict in their application of statistical techniques and the violation of the assumptions in individual statistics. Sociologists, in contrast, work in the field more than the laboratory and are very limited in the types and quality of data they collect. Survey based research, especially secondary data analysis such as this, often involves the utilization of statistics for a lower level of measurement than specified in their assumptions. The use of dichotomous "dummy" variables has been common in surveys for decades (see Suits, (1957) for an early presentation), and is frequently discussed even in elementary presentations of survey analysis (Blalock, 1979; Nie, et al, 1975). This utilizes the robustness of regression based statistics to generate the maximum ability to make theoretical inferences from data at a low level of measurement. There have also been examples of using factor analysis with dichotomous variables (Christoffersson, 1975; Muthen, 1978). Thus we accept the potential for our analysis to be statistically imprecise at the cost of moving toward contributions to theory about paranormal beliefs which can be further examined in subsequent investigations.

His point about the maximum possible correlations illustrated in table 1 (from Sullivan, 1983) is well taken, and involves theoretical as well as statistical interpretation. The difference between the amount of belief in paranormal phenomena and the dimensions of belief underlying these phenomena can be made conceptually, but in practice the two concepts are related. We stress the interpretation in our paper that what is present is a triad of dimensions, while he interprets it as three levels of belief. The data as they are available cannot be used to sort out this conceptual distinction, and we wait for other investigations in belief about paranormal phenomena to solve this dilemma.

In the last paragraph of Sullivan (1983) he discusses unpublished data (which we look forward to seeing soon in Zetetic Scholar) giving support for two factors rather than our three. This gives substantive support to our analysis distinguishing "religious" and "psychic" dimensions, and does not adequately rule out a third dimension involving belief in "other beings." The analysis by Sullivan (1983) only considers one "other being" belief (spirits in haunted houses), precluding its existence as a viable factor in his analysis while still separating religious and psychic factors. In addition, the two factor analyses are not directly comparable because: 1) entering different variables into factor analyses leads to the emergence of unique factors, and 2) Sullivan chose orthogonal rather than our oblique factor analysis.

In summary, we appreciate the concern of sullivan over the methodology we employed; his points are well taken, and he raises some important issues. However, we were operating from a different methodological tradition in our data analysis, aware of the problems involved, and still support the existence of three factors involved in paranormal beliefs. We hope that he and others will pursue further analysis in this topic, and feel that the cumulative process of multiple inquiries and replications will lead to a more complete understanding of paranormal beliefs in the United States and other societies.

References

Blalock, H.L., Social Statistics. New York: McGraw-Hill, 1979.

Christoffersson, A.,"Factor Analysis of Dichotomized Variables," Psychometrica 40:5-32, 1975.

Muthen, B.,"Contributions to Factor Analysis of Dichotomized Variables " Psychometrica, 43:551-60, 1978.

Nie, Norman, et. al., Statistical Package for the Social Sciences. New York: McGraw-Hill, 1975.

Sobal, Jeff and Charles Emmons," Patterns of Belief in Religious, Psychic and Other Paranormal Phenomena," Zetetic Scholar, #9:7-17, 1982.

Suits, D., "The Use of Dummy Variables in Regression Equations," Journal of the American Statistical Association, 52:548-51.

Sullivan, Charles, "Patterns of Belief in Paranormal Phenomena? A Critical Note on Sobal and Emmons," Zetetic Scholar, #10:(in press), 1983.

LETTERS

ON OCCULTISM & SECULARIZATION

In the last issue of ZS (1982:#9;3) Dr. Truzzi remarked that CSICOP's position that "'irrationality' and 'pseudoscience'...could subvert the progress of civilization" is "naive." Although I like to agree with his position, I have found that many scholars think otherwise and that history tends to support their position. Am aware that Truzzi has in the past contended that the flirtations with witchcraft and Satanism were a progressive aspect of secularization ('of the demonic') and hence not a threat to civilization, but I wonder if this is more true of the long run than the short.

Many scholars take the view that the popular interest in occultism and cults is merely a reflection of anomie, but some of the CSICOP members and others (Kurtz:1977, Lustig:1976) have gone on to suggest that such irrationality has a highly virulent radical potential. An often cited example is the influence of popular occult beliefs in the rise of nazism - particularly the "Fuhrer prinzip"; but this facet of the growth of nazism has scarcely been addressed by responsible scholars. Further support for the CSICOP position can be derived from Robert Darnton's Mesmerism and the End of the Enlightenment in France, but the exact role that mesmerism played in bringing about the French Revolution and especially the Terror remains obscure. Then of course there were the Renaissance occultists and millenarians and reactionary witch hunts, which is best attested to by the quite controversial later works of Yates (1972,1979) and more recently Jobe (1981). Other scholars, most notably Billington (1980), Tiryakian (1970,1974) and Webb (1974,1976) have examined the influence of occult beliefs on the growth of radical social beliefs and movements. Altogether, it appears that CSICOP's position regarding the socially negative potential of the popular belief in various pseudosciences and related forms of irrationality is not entirely without foundation. Indeed, this idea has been formalized by Cooter (1980), who has contended that pseudoscience is a "label" that has been applied to areas deemed threatening to society. Among the few scholars who have contended otherwise is Moore (1977), who uses the example of spiritualism; but he could be criticized for having a marxist ('religion is an opiate') bias.

In sum, I am curious as to how Dr. Truzzi would go about defending his position and hope he will elaborate on this problem in some future issue.

REFERENCES

Billington, James H.: Fire in the Minds of Men. New York: Basic Books, 1980.

Cooter, Roger: "Deploying 'Pseudoscience': Then and Now," in M. P. Hanen, et al (eds): Science, Pseudo-Science and Society. Canada: Wilfrid Laurier Univ. Press, 1980.

Darnton, Robert: Mesmerism and the End of the Enlightenment in France. New York: Schoken Books, 1968.

Jobe, Thomas H.: "The Devil in Restoration Science: The Glanvill-Webster Witchcraft Debate," in Isis, 72, #263, (1981), 343-356.
Kurtz, Paul: "The Psychology of Belief," in The Humanist, May/June, 1977, 42-3.
Lustig, Lawrence K.: "Science and Superstition: An Age of Unreason," 1976 Encyclopedia Britannica Book of the Year. Chicago: Univ. of Chicago, 1976, 270-3.
Moore, R. Laurence: In Search of White Crows. New York: Oxford Univ. Press, 1977.
Tiryakian, Edward: "Structural Sociology," in J.C. McKinney and E. Tiryakian (eds.), Theoretical Sociology, New York: Appleton-Century Crofts, 1970.
-----(ed.): On the Margins of the Visible. New York: J.C. Wiley and Sons, 1974.
Webb, James: The Occult Underground. Illinois: Open Court Publishing Company, 1974.
-----: The Occult Establishment. Illinois: Open Court Publishing Company, 1974.
Yates, Frances A.: The Rosicrucian Enlightenment. Great Britain: Granada Publishing Ltd., 1979.
-----: The Occult Philosophy in the Elizabethan Age. London: Routledge and Kegan Paul Ltd., 1979.

-- CHRISTOPHER C. SCOTT
McLean Virginia

M. TRUZZI REPLIES:

I have never argued that involvement with demonisms was "progressive" during the classical heretical period of history. My argument has pertained to the contemporary flirtation, much of which is non-serious, that reflects a breaking away from the traditional supernaturalism associated with the orthodox religions. Thus, popular occultism is largely a half-way-house from orthodox supernaturalism to eventual full naturalism.

I object to Mr. Scott's apparent equation of occultisms with "irrationality" and "pseudoscience." Though antisciences and pseudosciences certainly do exist, they should not be confused with protoscientific research programs that have grown out of them, nor should those orientations necessarily be labelled "irrational" (even if they erroneous). Superficial correlations such as that of nazism with some German occult views should not be translated as causation; and this view overlooks occultisms elsewhere such as the occult boom in England during the same period. Moore (who I do not interpret as a marxist) has correctly argued that spiritualism was a secularization of orthodox religion (which criticized it for not being spiritual), and parapsychology is a further secularization of spiritualism (see John J. Cerullo's The Secularization of the Soul for similar argument). Scott also overlooks the empirical surveys of those like Robert Wuthnow and W.S. Bainbridge and R. Stark which indicate that cult members are those who have left orthodox religions rather than those who have left science. Those who worry about entry into a "bizarre new Age of Superstition" seem to forget that we are still leaving an older Age of Superstition which they define as approved religion rather than superstition. I think it was Amrbose Bierce who defined superstition as "the other fellow's religion." The dominant criticism of occultism still comes from orthodox religionists (e.g., Billy Graham) compared to which criticism by Humanists and non-religionists is small potatoes. This demonstrates that the main threat of occultism is to religious orthodoxy.

ON THE PARA COMMITTEE

Thank you so much for sending me a copy of the "Zetetic Scholar" Nr. 9/1982, which I read and studied with great interest. Excuse me, that I have nevertheless been shocked by some items.

In his comments on Gauquelin's answer to the recent statement of the Belgian Para Committee, Mr. Dommanget criticizes the expression "strongly involved," which both Gauquelin and I spontaneously used in our texts, but it is just the plain truth!

If, during more than seven years, I attended all the meetings of the committee, all dedicated to the same problem (c.q. the Mars effect), and all taking place in another city (I live about 35 miles from Brussels!), I believe the expression "strongly involved" is quite normal.

On the other hand, the people of the committee now are in flagrant contradiction with themselves. If indeed I had no knowledge, neither in statistics nor in astronomy, why did the staff of the committee invite me to attend its meetings, during (bis) at least seven years?

Moreover, I have written intermediary reports while we were proceeding with the work.

Mr. Dommanget complains of "personal attacks" and "untrue statements" that should have been made by Mr. Gauquelin. But, in this case, he should be the first example of a contrary attitude!

Regarding my own involvement in the research, Dommanget now writes: "What kind of a professor" (sic). I can tell you that I have <u>three</u> different university degrees (copies of which I can send to you, <u>if</u> need be) and that I am <u>still</u> teaching for earning my living. Moreover, <u>before</u> entering <u>the</u> committee, I had been busy for more than <u>twenty years</u> with the problems of planetary movements and especially <u>with</u> the claims of astrology!

I hope that this new statement of mine will help to point out what has been really the case in (and with!) the Belgian committee.

-- LUC M.J.I. DE MARRÉ
Antwerp, Belgium

ON THE BRUGMANS EXPERIMENT

I have to apologize for an error in my "Reply to Beloff and Schouten" concerning the Brugmans experiment (<u>Zetetic Scholar</u>, No. 7). The percentages I have as 36% and 8% in the bottom paragraph on page 142 should have been 27% and 7%. The conclusions are not affected.

More significantly, I now believe that the two sentences following the one containing the above figures are too favourable to Brugmans' defenders. The argument concerns the high proportion of responses explicitly recorded as ambiguous, and the fact that this proportion drops spectacularly among the hits, suspiciously suggesting a reluctance on the part of the recorder to admit ambiguity when a hit was in prospect. I said that Schouten and Kelly, while not concealing this finding, had shown that it could not account for more than one third of the hits. In reality, they showed this only on the assumption that all ambiguous responses were recorded as ambiguous, when occurring among the clear misses. This assumption is far from obvious. It is quite possible that the degree of ambiguity large enough to permit the recorder to express his bias when a hit was in prospect, was less than the degree necessary to trigger his explicitly recording a response as ambiguous. If this were so, there would be no way of estimating a limit to the bias we are considering, and Schouten and Kelly's defense (on this point) thus collapses.

This case provides a good example of how easily one can overlook a subtle source of bias. Schouten and Kelly are not the first parapsychologists to fail to take account of ambiguity in the identification of ambiguity.

-- CHRISTOPHER SCOTT
London, England

ON UFO THEORIES

Concerning Richard de Mille's comments (ZS #9) on my response to Abell (ZS #8), Dr. de Mille states that "it is not human beings he [Sagan] imagines on distant planets, simply intelligent beings." I see nothing in my response to Dr. Abell that contradicts this statement. I criticized Dr. Sagan for his belief in the galactic proliferation of intelligent extraterrestrial beings.

Dr. de Mille then goes on to criticize evolutionary theory, a debate I do not wish to enter at this time, partly because it would deviate too far from our topic of discussion, and partly because there already exists a vast amount of literature on the subject. I am sure that most ZS readers have access to this literature.

Since writing my original response to Dr. Abell, I have come across an interesting little article by H. Sandon (1966), which reinforces even further the improbability of a multitude of galactic supercivilizations. Sandon puts the odds against the evolution of organic soup to modern man, involving 100 critical evolutionary steps over 4×10^9 years, at 2^{100} to 1 (!). However, as each one of those steps would be confronted by many alternatives, not just two, "the real odds against a repetition of the chain of events, even on a world identical to ours, are incalculably great."

In response to Dr. Abell's response, it is gratifying to learn that he does not, in fact, accept the existence of Sagan's one million supercivilizations in the galaxy, and that he was only using that figure for illustrative purposes. However, I would still take exception to his new remark concerning whether or not (or how often) we could have been visited by such extraterrestrial intelligences. I can only repeat the comment in my response to Mr. Farish (ZS#8), top of page 59): "The fact is that scientists have not the foggiest idea of the level of technology possible extraterrestrial civilizations may have attained, nor 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."

I thank Dr. Abell for his apology, and I also apologize if I misinterpreted or misrepresented some of his own points.

-- J. RICHARD GREENWELL

ON DEFINING UFOS

Much as I respect Michael Martin for his attempt to produce a scientific definition of the term "UFO" (ZS #9), I think there are a number of factors that he should be aware of.

Firstly, whilst he comments that little effort has been made on this point from within the subject, I must stress that this is not strictly true. At the 1979 International UFO Congress, organized in London by BUFORA, the then newly formed PICUR (Provisional International Committee for UFO Research) spent most of its time at backroom meetings endeavouring to thrash out a mutually agreeed upon definition. This committee includes leading representatives from most active ufological nations, many of whom are scientists, and that particular session was attended by Dr. J. Allen Hynek. This was no doubt one reason why the final agreed upon version differed little from that adopted by Dr. Hynek personally, to which Mr. Martin makes reference.

I am not actually attempting to defend either ufology or PICUR here, because in many respects the outcome of these lengthy discussions was a failure. It was severely criticised from within the subject by many researchers after it was published. Some of these criticisms match those made by Michael Martin. There were additional problems too. The point I am trying to make is that this shows rather well that defining the term "UFO" is a fundamental difficulty which I doubt can be overcome. There are many reasons for this. For example, the subject of "ufology" covers such a broad range of sciences (physics, psychology, sociology and meteorology, to name just four) that any definition is going to have to be an open-ended one in order to take account of this. And since science abhors open-ended definitions, the goal of a scientific but realistic definition of any UFO terminology seems to me to be almost unreachable.

My second point is...does it matter? I am at a loss to understand, after all of Mr. Martin's semantics, just how much further it would get us to have achieved a workable definition. Would it bring us any closer to knowing what a "UFO" was? What worries me is that if it could be argued that it did, then we would have set out in advance the restrictions we are placing onto our solution, merely by the act of defining it. I fear that in my view it is grossly unwise to define something before you have much idea about what aspects of science it represents. Hence, I am extremely suspicious of these current attempts to define terminologies strictly. If the price of making "ufology" scientific is prejudging our results, then I for one would rather remain unscientific.

Mr. Martin might be interested in a definition I adopted, for my own use, in my 1981 book UFO Study (London: Robert Hale). Let me stress that this was never intended as any more than a guide for me and is certainly not meant to be a rigid proposal......"UFO: A stimulus, visual or otherwise, that provides the percipient with information about an unidentified phenomenon which appears to him to be in, or originate from, the atmosphere or beyond" (P. 12). I am sure there are problems with this definition, as with any other. Indeed I can already see some looming myself. I could explain why I think certain aspects of it are necessary, but I trust the previous explanations will suffice at this stage. I will add more, if necessary, after Michael Martin has made comment.

However, there are other points that I feel need to be said about the value of definitions, which it is best to illustrate by way of examples.

In March 1978, a most interesting case occurred at Risley in Cheshire. (It is summarised in my book UFO Study). Ken Edwards, a service engineer, was returning from a union meeting when a white luminous mass (which he interpreted as figure-like) came down a steep embankment and crossed the road in front of his van. The "figure" stopped in the centre, in front of Ken's now parked vehicle. A beam of light was emitted from it and appeared to strike him. The "figure" then proceeded on its way, passed through a ten-foot high security fence, and disappeared. Subsequent effects on Ken were burns on his fingers, severe damage to the van's radio transceiver (a power surge explosion through the diode circuits in fact), his watch stopping at the time of occurrence, loss of consciousness and what is best described as a "mass of thoughts suddenly swimming through his head." What is more, subsequent to the period of investigation (but within one year) Ken developed cancer so severely that despite intensive treatment he died in March 1982. He never connected his illness with his experience, although he insisted about its reality status right up to the end. There may be no connection, but there are suggestions that there could be.

The clear point at issue is this...on any rigid definition of the term "UFO", as proposed by Hynek or Martin for example, this case would not be a UFO case. What would one define it as?...A ghost?...A sasquatch? It is clearly neither. It shares so many parameters with other UFO reports of the same category (where the luminous mass is witness interpreted as a "spaceship") that I have no doubts whatsoever as to how we ought to define it, if we hope to understand the problems it represents. So, I am less than happy about any definition that serves to eliminate cases of this description. For it is not unique. It does not matter, incidentally, what Ken encountered. There are indications that it might have been some form of radiating energy phenomenon (he was immediately adjacent to a major atomic energy research reactor!). But it may very well be that other reports of the same thing are found within the UFO records (where the interpretation of the phenomenon is more UFO orientated). The crucial analytical point, surely, is the comparision of internal characteristics.

In closing, I want to make another remark about Dr. Martin's article. I got the distinct impression that he was trying to formulate a definition on the presumption that only reports which remain unidentified (by whatever definition of this one chooses) are of "scientific interest" (he uses those exact words). I would contend that this is a seriously misrepresentative viewpoint. If he cares to study Allan Hendry's book (The UFO Handbook), and my previously refered to UFO Study, he should soon appreciate why. As Hendry shows, there is little difference between IFO and UFO data. And as I endeavour to show, different witnesses can, and do, interpret the same stimulus in terms of both categories. In one particular instance it was possible to prove that a phenomenon reported faithfully by one witness as a UFO was in fact a helicopter (proved because I saw it!). Yet the story of the other witness remained unwavering, and it was a typical close encounter UFO experience. This was the first hint that I ever had that the distinction between UFO and IFO is not as clear cut as we might like to think. Since that time I have looked for, and become disturbed by, the remarkable number of seemingly strong close encounters (including at least two cases involving animate entities!) which have been generated by the stimulus of the moon. One would imagine that the moon, being such a familar object, could not possibly be so significant, and generate such extreme cases (quite often multiple witness events I would stress). This seems to imply (and Hendry would, I think, agree with me) that the IFO data has an interesting problem within it too. Almost certainly it is one which falls into the category "psychological" (although I can think of some pretty good arguments as to why it need not necessarily be so). Even so, the problem is evident. A definition which excludes from scientific interest any case which is identified may be treading on very thin ice.

In the light of my remarks I would be most interested to know if it is still felt a) desirable; and b) feasible, to create an effective scientific definition of the term "UFO".

-- JENNY RANDLES
Cheshire, England

I am not at all sure that the hunt for the elusive UFO-definition is a worthwhile occupation, but the temptation to comment on Michael Martin's efforts is irresistible. He deftly analyses the problems, but the definition he ultimately offers, while it may satisfy a philosopher or a metaphysician, is not one that a working ufologist will feel comfortable using. It seems to me that, using his reasoning, we could arrive at a more practicable formulation.

He rightly affirms, if not in so many words then surely in his procedure, that any UFO-definition must be both relative and provisional: relative, because unidentified things are by definition contingent on things that have been identified, and provisional, because anything in a state of being unidentified may at any moment cease to be so.

These aspects must be built into our definition, so how about:

By UFO is understood a phenomenon which causes a percipient to report what seems to be a physical object, flying or capable of flight, but which neither he nor anyone else has yet been able to satisfactorily identify, as regards either its nature, origin or purpose, with any known object.

-- HILARY EVANS
London, England

MORE ON UFO THEORIES

I hope Mr. Greenwell will forgive my not knowing his language well enough, but I believe our disagreement may be due to having ot transform into Engligh a thought that was conceived in another language.

In fact there are two terms in Italian, rather different in meaning, which are translated in English by one word only: "knowledge." It is therefore necessary to differentiate between "branche del sapere" (branches of knowledge) and "branche del noto" (branches of what is well known). The former refers to various cognitive aspects which can be grouped under a single general denominator (i.e., Physics, Mathematics, Sociology, Religion, and so on), while the latter refers to what has already been ascertained and interpreted in the various fields (i.e., black body radiation theory, etc.). The result is that the field of the "known" is like an oil spot that spreads into the field of the "unknown," subdivided into its various sectors (the branches of knowledge) in which attempts are made to act according to the proper cognitive method. Whatever is on the border of the unknown is usually the subject of attack on the part of the researcher who tries to bring it back into the realm of the well known, the known and interpreted within one or more branches of knowledge. I therefore rather wonder at Mr. Greenwell's surprise.

No one is surprised when black holes, tachyons or gravitons are considered as belonging to the branches of knowledge, even though we are unable to say whether or not they "exist." But I think the problem is something else: up to now few scholars exist in the world who can claim they are doing research on UFO phenomena. In fact, in order to research anything, the following requisites are essential: at least a university background, the possibility of doing it on a full time basis, the necessary instruments, a scientific mentality, an open mind and feet firmly anchored to the ground.

The result is that up to date very few researchers (they could be counted on the fingers of one hand) have been able to confront the problem "per se inserting it (where possible) in the framework of the acquired knowledge.

The UFO problem is very complex and research must be done by those who are able to do it.

In other fields amateurs have succeeded in becoming even more than professional. This is not the case with ufology, where the individuals involved are rarely up to the task. But up to now amateurs have dominated the field in ufology (and unfortunately not always of the best kind). The result is that they have been able to do as they liked, to let their imaginations run wild, producing a proliferation of basically ridiculous theories.

It is high time that those who have the intellectual capacities, the knowledge, the instruments and full time at their disposal became conscious of the fact that they are needed. The era of "Silly Theories" must come to an end.

-- ROBERTO FARABONE
Bologna, Italy

BOOK REVIEWS

Science et Antiscience. By the Secretariat international des Questions Scientifiques (no editor listed). Paris: Editions du Centurion, 1981. 197 pp. 39 francs (approximately $6.30 U.S.).

Reviewed by Gregory R. McGuire

Although rather uneven in scope, this colloquium, presented by the International Movement of Catholic Intellectuals, presents several papers of interest to scholars of the "antiscientific" enterprise. Before looking at these papers, it is perhaps important to mention that his book is a clear example of the problems encountered in the labeling of non-accepted scientific endeavours as antiscientific. As Pinch & Collins have elsewhere pointed out, the term antiscience refers more clearly to an attitude than to scientific enterprise, and the confusion between the two meanings of the term is quite apparent in this book wherein political ecology movements are discussed side by side with activities which have been better termed protoscientific, pseudoscientific, or deviant. Still, it must be pointed out that European scientific activity is politicized to a degree that would probably surprise most North American researchers, with Europeans generally much more sensitive to both the uses and abuses to which their research is employed. Of particular interest in this vein is the opening discourse of J. Ladrière (Courants d'antiscience; causes et signfications) in which are discussed both the motivations and typical characteristics of antiscientific movements, with an emphasis on the common ground of all groups fighting against an installed scientific orthodoxy. Similary, S.L. Jaki, in discussing the differences between political movements and unorthodox science, provides an interesting exploration of the historical and philosophical roots of the debate between marginal and orthodox science. But it is the presentation of J. Courcier (Considérations à partir de l'épistemologie contemporaine) which has the greatest potential importance to readers of Zetetic Scholar, with an explanation of the epistemological base of both language and logic in the demarcation of antiscientific research. Of particular note is the exploration of the work of the French historian Foucault concerning the development of the relationship between objects and the corresponding language tools. Although Foucault is mainly known to North American audiences for his work in the history of mental asylums, his theories on the development of language (notably Les mots et les choses) presents one of the clearest explanations I've yet run across of the manner in which the very act of classifying phenomena necessarily results in the often arbitrary exclusion of some concepts. Although the explanations of political orientations may hold only marginal interest for the study of anomalous research, this colloquium could possibly be useful for both the manner in which these orientations are necessarily entwined with unorthodox science in Europe, as well as for the indications given as to the European conception of antiscientific/psuedoscientific enterprise and the ways in which this conception differs from North American views on unorthodox science.

Loch Ness Monster. By Tim Dinsdale. 4th edition, Routledge and Kegan Paul, London, Boston, and Henley, 1982. 218 pp., 20 plates. $9.50 (original paper).

Reviewed by Henry H. Bauer

All zetetic scholars should read this book. Dinsdale has been the leading field investigator at Loch Ness for more than two decades, and his writings are invaluable on at least two levels: for those interested in the Loch Ness phenomenon, of course; but also for those who are concerned with the investigation of any anomalous phenomenon. And this fourth edition contains a great deal of new material (50 pages and 7 plates) that is significant on both levels.

Of the most general interest are the descriptions of how Dinsdale's interest was first aroused, how he prepared himself to investigate the subject, and the continuing struggle to have the scientific establishment take note of empirical data whose soundness is little, if at all, short of indisputable.

Dinsdale had extraordinary luck: on the last day of his first expedition to Loch Ness in 1960, he obtained 16mm film of a large, rapidly moving hump in the water -- 20 years later, still the most incontrovertible evidence that large, unidentified animals inhabit Loch Ness. But the value of that evidence stems not from Dinsdale's luck, but from the careful prior thought he had given to the problem, and his understanding of what is needed to establish data as sound: he filmed a control sequence featuring a motor boat moving over the same course as the hump; he had independent people certify to the sealing of his camera; he had Kodak unseal the camera and develop the film; he gave all details of camera, lens, filming point, etc.; he made the film available to technical experts. Investigators of other anomalies have much to learn from the meticulousness of Dinsdale's approach in this, and in his later expeditions (described more fully in his other books: notably Project Water Horse, Routledge and Kegan Paul, 1975). Instructive and exemplary also has been Dinsdale's eschewing of public controversy and polemic. He is quite clear that only the amassing of overwhelmingly sound and unambiguous evidence will carry the day, and he concentrates on that task: and that is a refreshing (and productive) difference from those many proponents of the reality of various anomalies who complain endlessly of the conservatism of science and find themselves tempted to describe evidence as more compelling than it actually is. Dinsdale was sufficiently convinced that the quest is worthwhile that he relinquished a career in aeronautics to make possible more frequent expeditions to Loch Ness; in his late thirties, with a family of four, he found ways to make a living that also gave him time to hunt down the truth at Loch Ness. When a would-be investigator of anomalies tells me that he is inhibited by lack of time or lack of money, I think of Dinsdale, and wonder whether the would-be investigators are not actually inhibited by lack of conviction than by those other lacks.

Perhaps of particular interest on the general level is Appendix D of the book; here, Dinsdale gives a reappraisal of some of the classic evidence. Clearly his conviction is now so secure that he is able to examine individual pieces of data quite critically: the possible discrediting of this photo or that is not, for him, a threat to the case as a whole. Again, this is a fine example for other investigators, who so often cling to every possible though farfetched support for their views that they actually argue counterproductively.

Insofar as the Loch Ness phenomenon is concerned, this book is indispensable. Appendix A gives highlights of field work up to early 1981. Appendix B gives an important summary of sonar results. And Appendix C has the only full, published details of the Smith movie of 1977 and the Shiels stills of the same year. Those latter are reproduced in plates 19 and 20, and one of them also is shown in color on the cover of the book. Other plates new to this edition show the remarkably high resolution achieved by sonar, in the image of a sunken Wellington bomber; and the intriguing multiple-echo sonar-trace that seems to often characterize the Nessies -- perhaps because of a particular orientation in the sonar beam of an animal with distinctly different regions of the body and appendages.

Of course, no single book can now cover all the significant material dealing with Loch Ness (see Z.S. no. 7, December 1980, pp. 30-42), but this book is classic and indispensable.

Ball and Bead Lightning. By James Dale Barry. London and New York: Plenum Press, 1980. 298 pp.

Reviewed by W.N. Charman

Ball and bead lightning are atmospheric phenomena which have excited curiosity and speculation over a span of at least 300 years and whose nature still remains enigmatic today. Indeed, the very existence of the phenomena is still occasionally challenged. Ball lightning is the name given to the mobile, luminous spheres, having lifetimes of many seconds or even minutes, which are usually although not exclusively, observed in the vicinity of thunderstorms. Bead lightning is the long-lived residue of a normal cloud-to-ground or cloud-to-cloud lightning stroke; it appears as a series of descrete luminous regions, separated by dark spaces, along the path taken by the stroke. Barry's book reviews the properties ascribed to the two types of "lightning" and examines critically the relevant evidence for their existence.

The author starts by outlining the characteristics of "normal" lightning flashes. These have been extensively studied in recent years, aided by the fact that tall structures may be repeatedly struck so that systematic investigation of such features as the charge transfer, temperature and time course of the flash can be made. Perhaps the most important property in relation to the long lives claimed for ball and bead lightning is that the total duration of a "normal" flash rarely exceeds 0.2 sec.

After this introduction Barry moves to a review of the history of studies of ball and bead lightning and then to a detailed consideration of the evidence for bead lightning. Rather surprisingly, although bead lightning normally raises less controversy than ball lightning, it appears that (p.26) "most of the photographs reported to be of bead lightning are at least questionable, and should probably be dismissed." Pinch effects or oscillations in the lightning channel have been postulated as origins for the beads, but Barry concludes that an adequate explanation has not yet been found. The various properties of ball lightning as deduced from eyewitness reports are next summarized, and estimates of the energy density are made. It is concluded that, contrary to many people's belief, the balls do not necessarily have a very high energy content and that a typical energy density may only be ~ 1 Jcm^{-3}; there is no evidence from the records that several distinct classes of object are involved.

An exhaustive discussion of the photographic evidence for ball lightning then follows. This includes almost all of the records published in the literature and is therefore of great value although the clarity of some reproductions is rather poor. Barry rightly stresses the problems in interpreting time-exposure photographs which purports to show the track of moving lightning balls and points out that many of these "tracks" were probably produced by a combination of camera movement and a stationary light source during the exposure. The photographs of natural events are compared with photographs of luminous structures produced in the laboratory during attempts to simulate ball lightning.

Rightly, Barry devotes his next chapter to a discussion of the reality of ball lightning as a phenomenon. Are eye-witness reports attributable to, for example, the persistence of after-image following observation of a normal flash? Are estimates of ball dimensions and lifetime likely to be accurate? Could the photographic records be explained in terms of other, well-accepted phenomena? Barry concludes that, while much of the evidence for ball lightning may be questionable, a hard core of reliable material remains.

The rest of the text is devoted to a description of efforts to simulate ball lightning in the laboratory using a variety of techniques. It is tantalising that while many luminosities with at least some of the characteristics of ball lightning have been produced, few have had lifetimes exceeding 1 second and these studies at best serve merely to clarify the parameters involved in an effective ball lightning model. The book closes with what is undoubtedly the best available bibliography on the subject, with over 1600 references to both observations and experimental studies.

In all, this is a useful and interesting introduction to what, according to one's viewpoint, may either be an area of atmospheric physics or of social psychology. Systematic scientific study of naturally-occuring ball lightning is unlikely to make rapid progress since no sites are known where there is a high probability of observing the phenomenon within a reasonable span of time. The quality of eye witness reports can, however, be enhanced by minimising the delay between the event and its recording and by ensuring the all relevant information is secured, while much remains to be done in the laboratory in investigating the properties of related discharge and other phenomena. This book will undoubtedly help to stimulate interest in this fascinating field.

Science and Unreason. By Daisie Radner and Michael Radner. Belmont, CA: Wadsworth, 1982. x + 110 pp. $6.95

Reviewe by Gordon Hammerle

The title of this book conveys the bifurcation the authors see between science and unreason (i.e., "pseudoscience"). Included in the latter category are the beliefs espoused by The International Flat Earth Research Society, Erich von Däniken's books, biorhythm theory, creationism, Worlds in Collision, and parapsychology. Later they deal with what they call "borderline cases"; continental drift and sociobiology are the examples they use. Yet, they take pains to point out that these cases do not undermine their distinction between "science" and "pseudoscience." Rather, they argue that these are just cases where it is hard to decide which category applies.

The authors have attempted to describe the classic philosophy of science and to show how "pseudoscience" differs from accepted science. Anomalous viewpoints are not treated individually except as examples of how they do not conform to orthodox scientific reasoning.

The heart of the book lies in the "marks of pseudoscience" that the authors present. They argue that the presence of any such characteristic (e.g., the use of irrefutable hypotheses) is sufficient to identify "pseudoscience." This is undoubtedly overstated, in that orthodox scientists sometimes lapse into some of the ways of "pseudoscience" the Radners list (one of which is refusal to revise in light of criticism). A better approach, I believe, was outlined by Fred Gruenberger in his article, "A Measure for Crackpots" (Science, 25 September 1964, pp. 1413-1415). He identified some of the main attributes of the scientist, assigned point values to each, and then suggested judging each fringe area of science on the extent to which these characteristics are present in their proponents. Unlike the Radners, Gruenberger argues that no single test can discriminate the crackpot from the legitimate scientist. Indeed, this approach suggests a continuum (though scientist and crackpot may not be the best choice of words for the endpoints). Gruenberger's work is not cited here; indeed, the book has relatively few references.

One strength of the book is the wide variety of examples used. Geology, astronomy, medicine, biology and physics are drawn upon as well as the better-known anomalies. Some prior familianty with von Däniken, Velikovsky, and content areas such as biorhythms is assumed or is necessary to fully appreciate their discussion. Though the authors refer to the "gray area where scientific respectability is questionable," it is clear that they find the fringe area to be populated with "cranks, quacks, and crooks" (the title of their fourth chapter). Indeed, in the preface they indicate that "instead of treating the fringe areas one by one, we lump them all together," and thus include some Skeptical Inquirer tidbits such as "Christ Was a NASA Pilot" and "Dog Proclaims Innocence by Telepathy."

The writing is on the dry side and occasionally gets bogged down in explaining terms from the writers' own field, philosophy. There is evidence of a strong effort to avoid sexist language. While this is commendable, they use constructions such as "the crank goes looking for enigmas and she rejoices when she finds them." Such sentences only serve to distract the reader. With judicious editing, the book could have been reduced to the length of a journal article.

The book succeeds in differentiating how the reasoning and philosophical assumptions of (some) members of the scientific fringe differ from that of traditional science. The book makes explicit some of the reasons why we tend to reject some approaches to nature. Almost any book that deals with the philosophy of science is bound to oversimplify and distort how science really works. Yet, for man, that philosophy represents an ideal, and the Radners find that the scientific fringe does not conform to it. They conclude that this makes most would-be revolutionaries merely cranks.

The Andreasson Affair. By Ray Fowler. Prentice-Hall, Englewood Cliffs, New Jersey, 1979. 239 pp. $8.95.
The Tujunga Canyon Contacts. By Ann Druffel and D. Scott Rogo. Prentice-Hall, Englewood Cliffs, New Jersey, 1980. 264 pp. $9.95.
Missing Time: A Documented Study of UFO Abductions. By Budd Hopkins. Richard Marek, New York, 1981. 258 pp. $12.95.

Reviewed by Ron Westrum

If psychologists spent less time measuring the cognitive consistency of undergraduate students and more time on pressing human problems, then we might have a better understanding of the abduction experience, one of the phenomena of psychology whose importance is evident when its full features are realized. The usual scenario reported is this: a person driving home from out of town has a UFO experience on the way, not a particularly impressive sighting, but intriguing. He/she also notices, upon arriving home, that it took longer than it should have, perhaps by an hour or two. In the weeks or years that follow, the person is bothered by dreams or by certain stimuli, perhaps an extremely strong negative reaction to certain places or situations. For one reason or another, the person seeks hypnosis to remember what happened during the amnesic period. Under hypnosis, the person "remembers" being taken aboard a UFO by aliens, and may undergo a quasi-medical examination in the process.

This sounds bizarre, and it certainly is. But reports of abduction cases (known cases, that is), now number in the dozens, although relatively few have been written up. All the information which I have been able to gather from persons who have done research into such cases suggests that the amnesic period is associated with a real traumatic experience, and the postabduction syndrome (if one can call it that) certainly is consistent with a repressed stressful experience. If the experience reported were a rape or a car accident, the clinical psychologist would be on familiar ground. This is, however, not the case. The repressed experience is described as a UFO event, or involves aspects that would be extremely familiar to UFO investigators. But even as a UFO investigator I have trouble accepting the literal truth of the witnesses' recollections. And the recollections are vivid. Under hypnosis, the subject can hyperventilate, grimace, cry, and show other signs of violent emotion as he/she "remembers" the events that took place.

Psychologists who confront such experiences have two matters to explain. The first is the content of the abduction experience. While Alvin Lawson and others have suggested that UFO abductions represent archetypal experiences, such as birth trauma or near-death experiences, the "technical" details of the experiences are surprisingly uniform; and even in the case of strongly religious witnesses, as described in the book by Fowler, there is relatively little religious material, compared with what one might expect. But the content of the experience is only half the problem. Why was the witness, or witnesses, amnesic for the period in question? Sometimes these events involve two or more witnesses. And why, if there is a simple reason for the amnesia (temporary carbon dioxide poisoning for instance, causing unconsciousness), does the witness so manifestly seem to be exhibiting signs of repressed trauma? Is the abduction experience a cover for some other kind of traumatic experience? If so, why are such experiences never remembered?

The book by Fowler discusses the apparent abduction experience of Betty Andreasson, a New England housewife. Fowler's careful investigation of the case, including lie detector tests, is in many respects a model of how such research should be carried out, although there are certain questions that remain. The Druffel/Rogo volume, more free-wheeling and more complicated, deals with the abduction experiences of several women who knew each other in California. The Hopkins volume is a record of his gradually increasing involvement with research on abductions, written after the fact to be sure, but very interesting in terms of the development of his thought and the implications of such experiences for the lives of his ostensible "abductees" and ultimately for the rest of us as well.

Each of these three books relate the authors' search to unravel this mystery, and it should be no surprise that all three assume that a UFO abduction experience actually took place in the cases examined. Fowler discusses one case in detail, Druffel/Rogo and Hopkins each tackle several. All three provide interesting insights and details as to the nature of the quasi-scientific investigations they carried out in order to get the information. Al three provide important human chronicles of the abductees' and investigators' attempts to come to grips with the experiences involved.

Hopkins' book is the most comprehensive and provides a very useful over-view of the abduction experience, and also makes some disturbing suggestions. Do the aliens abduct people twice, once as youngsters and then as adults? Are certain wounds one has carried since childhood (but whose cause one cannot remember) the traces of alien medical procedures? Just how many abductions are there, anyway? Hopkins's book suggests that they must number in at least the hundreds of thousands. The Druffel/Rogo book suggests that abductions may occur within the same social network: is the experience contagious? What is going on here?

It is to be hoped that the neglect of these interesting phenomena by the psychological community will not long continue. Perhaps they have a simple explanation. If so, what is it?

<u>The Prophecies and Enigmas of Nostradamus.</u> Edited by Liberte E. LeVert. Firebell Books, Glen Rock, N.J., 1979. x + 257 pp. $20.

Reviewed by James Randi

At last there is available a proper, careful translation of the major Nostradamus verses, prepared by a scholar whose knowledge of the archaic French of that period, plus a dedication for presenting a true interpretation, allow us to examine the claimed "Prophecies" with greater acuity. The original edition of 1555 was used to prepare this present translation, though later editions are used to cover quatrains not included in the earlier one.

The job, admits the author, is never easy, due to the strange manner in which Michel de Nostredame prepared his work, the obvious printers errors, and the purposeful ambiguity of what were called, the Centuries.

Familiar with the construction of the "vers commun" form used by Nostradamus, LeVert (real name, Everett Bleiler - the pen-name is an anagram, in honor of the subject) points out several places where the printer - or editor - could very well have altered the text and added to the confusion. Nostradamus, in his other writings (mostly on culinary arts!) shows that he was quite capable of handling the language well. Thus, breaks in style probably indicate errors.

Then, too, there have been deliberate changes made after-the-fact in order to make the quatrains agree with the events. Personally, I have always been struck by the fact that different writers have managed to obtain very much different solutions to the same stanzas. For example:

Century I - verse 60:

An emperor shall be born near Italy
Who shall be sold to the Empire at a high price;
They shall say, from the people he associates with,
That he is less a prince than a butcher.

This can apply equally well to Napoleon, Hitler or Ferdinand II, and has been so used by "interpreters" of the French seer, but Bleiler wryly comments that it fits any number of the Roman Emperors much better. The expression "near Italy" is typical of Nostradamus, who thus covers France, Yugoslavia, Austria, Switzerland, Greece and Albania in one phrase.

It becomes apparent, upon reading this book, that much of the mystery of the writings of Nostradamus lies in the poor and biased translations students have had to work with. With much of this problem done away with, we are surprised to see that he was, in some cases relating events of his own day, rather than making predictions. One favored quatrain, which is said to foretell the Great Fire of London in 1666, almost certainly refers to the year 1555, instead:

Century II - verse 51:

The blood of the just shall be wanting at London.
Burned by thunders, at twenty three the sixes--
The foolish woman shall fall from high place.
Of the same sect several/a greater number shall be killed.

Zealots assume "three times twenty" and add two sixes, thus they have "666" and they throw on a thousand for good measure. It happens, according to my history book, that Bloody Mary, in 1555, sent a number of Protestants to the stake with bags of gunpowder between their legs, thus providing the "thunders" -- and they went in groups of six. The original "antique" is properly translated as, "foolish" or "senile," by Bleiler, and obviously refers to the crazed queen of England who enjoyed herself with the spectacles. In any case, this is a much more probable source of the verse, and it can hardly be used as a prediction of the London fire.

Reproduced from the author's (one can hardly accept "editors") typed manuscript, the book is accompanied by a correction sheet. It is hardbound and privately printed.

BOOKS BRIEFLY NOTED

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

Alexander, Marc, HAUNTED HOUSES YOU MAY VISIT. London: Sphere Books, 1982. 184pp. 1.50 pounds, paperback. A mainly nonscholarly book for tourists to the United Kingdom.

Baker, R. Robin, HUMAN NAVIGATION AND THE SIXTH SENSE. N.Y.: Simon and Schuster, 1982. 138+xx pp. $14.50. Highly controversial experiments alleging magnetic ability in human beings. Failures to replicate raise serious questions, but this work should not be dismissed too readily, Fascinating stuff.

Baran, Michael, INSIGHTS INTO PREHISTORY. Smithtown, NY: Exposition Press, 1982. 114pp. $7.00. A sequel to the author's ATLANTIS RECONSIDERED, this essay continues to argue for the existence of Atlantis and Lemuria and their use of earth core gravitational energies which ultimately brought them cataclysms. Works in the writings of Mdme. Blavatsky, Edgar Cayce, Ruth Montgomery, and widespread folk legends from everywhere. An extraordinary linking together of a host of dubious scenarios into a grand synthesis which, alas, is no more convincing than its parts. An excellent example of this genre,

Barber, Chris, MYSTERIOUS WALES. North Pomfret, VT: David & Charles, 1982. 243 pp. $22.50. An excellent volume in the series of books on similar regional lore by this publisher. Very good tourist fare.

Barnouw, Erik, THE MAGICIAN AND THE CINEMA. N.Y.: Oxford University Press, 1981. 128pp. $12.95. A fascinating study of the role of conjurors in movies and moviemaking. The chapter on "Fantasms" should be of special interest to ZS readers for its accounts of early projection illusions including those adopted by some spiritualists. Particularly valuable, too, for its discussions of magicians' cloaking their presentations with the mantle of the occult and the fuzzy line between conjuring as science and conjuring as magic. Beautifully illustrated with rare reproductions. Recommended.

Blackmore, Susan J., BEYOND THE BODY: AN INVESTIGATION OF OUT-OF-THE BODY EXPERIENCES. London: Heinemann, 1982. 271+xv pp. 8.50 pounds. An exceptional and highly critical work in constructive skepticism. An outstanding survey and analysis of the OOBE experience. Highly recommended.

Bord. Janet and Colin, EARTH RITES. London: Granada, 1982. 273+xiv pp. 8.95 pounds. A well illustrated survey of fertility rites and customs. Much excellent folklore and historical material, but uncritical discussions of contemporary psychokinetic and related phenomena such as the claims of Uri Geller and dowsing. Despite such problems, the survey is encyclopedic and constitutes an excellent review of fascinating traditional lore, and the heavy annotation should lead the reader into further and critical literature.

Broad, William, & Nicholas Wade, BETRAYERS OF THE TRUTH: FRAUD AND DECEIT IN THE HALLS OF SCIENCE. N.Y.: Simon and Schuster, 1982. 256pp. $14.95. A remarkable survey on scientific fraud which concludes that fraud is endemic in science today and a function of institutional factors within science -- that it is not a case of bad apples but that the problem is with the barrel. Highly recommended.

Brody, Howard, PLACEBOS AND THE PHILOSOPHY OF MEDICINE: CLINICAL, CONCEPTUAL AND ETHICAL ISSUES. Chicago: University of Chicago Press, 1980. 164+vii pp. $13.50. An excellent survey of the research and the deeper philosophical issues, this study is an excellent complement to Michael Jospe's THE PLACEBO EFFECT IN HEALING which covers much of the same territory but is strangely uncited by Brody.

Capra, Fritjof, THE TURNING POINT: SCIENCE, SOCIETY AND THE RISING CULTURE. N.Y.: Simon and Schuster, 1982. 464pp. $17.50. Capra applies the new physics and holistic, systemic approaches to the crises of our times including everything from economics to health. A cry for cultural transformation.

Cerullo, John J., THE SECULARIZATION OF THE SOUL: PSYCHICAL RESEARCH IN MODERN BRITAIN. Philadelphia: Institute for the Study of Human Issues (ISHI), 1982. 194+xvi pp. $18.50. An excellent study in cultural history tracing the development and social meaning of the growth of parapsychology. All critics of parapsychology, especially, should read this book. Highly recommended.

Christian, William A., Jr., APPARITIONS IN LATE MEDIEVAL AND RENAISSANCE SPAIN. Princeton, NJ: Princeton University Press, 1982. 349+vii pp. $25.00. A find scholarly study of the accounts of the appearances of Mary and other saints in rural Spain from 1399 to 1523 with special attention to the social significance and the reactions of acceptance and rejection. Recommended for specialists.

Christie-Murray, David, REINCARNATION: ANCIENT BELIEFS AND MODERN EVIDENCE. North Pomfret, VT: David & Charles, 1982. 287 pp. $26.50. A very good survey of the evidence and views on reincarnation which especially concentrates on theological as well as scientific considerations. Recommended.

Collins, H.M., and T.J. Pinch, FRAMES OF MEANING: THE SOCIAL CONSTRUCTION OF EXTRAORDINARY SCIENCE. Boston: Routledge & Kegan Paul, 1982. 210+vii pp. $27.50. A quite remarkable work of particular importance for those interested in the history of the metal-bending research. Written from the relativistic orientation, it reveals the complexity of the issues and details usually glossed over. A very significant sociological contribution. Highly recommended.

Corliss, William, compiler, LIGHTNING, AURORAS, NOCTURNAL LIGHTS, AND RELATED LUMINOUS PHENOMENA: A CATALOG OF GEOPHYSICAL ANOMALIES. Glen Arm, MD: Sourcebook Project (P.O. Box 107, Glen Arm, MD 21057), 1982. 242+v pp. $11.95. This remarkable new volume in a projected series of 25 volumes catalogs and evaluates the anomalies uncritically presented in the sourcebook series. These volumes are indispensible for any serious anomalist and this new series is amazing in its scope. This first volume is a must for any serious ufologist especially. Highly recommended.

Crail, Ted, APETALK & WHALESPEAK: THE QUEST FOR INTERSPECIES COMMUNICATION. Los Angeles: J.P. Tarcher, 1981. 298+xvi pp. $14.95. Though biased in favor of the animals-have-language camp, this overview of the controversies is a good introduction to the issues and personalities (people and animals) involved. Not a scholarly work but a good journalistic account.

Davis, Philip J., and Reuben Hersh, THE MATHEMATICAL EXPERIENCE. Boston: Houghton Mifflin, 1981. 440+xii pp. $9.95 paperback. A marvelously lucid presentation of the history, aesthetics, pedagogy and personalities of mathematics written for the layman. Should be of special interest and value for many ZS readers. Recommended.

Demos, John Putnam, ENTERTAINING SATAN: WITCHCRAFT AND THE CULTURE OF EARLY NEW ENGLAND. N.Y.: Oxford University Press, 1982. 543+xiv pp. $25.00. A magnificent psycho-socio-historical study of great interest and depth. Highly recommended.

Di Orio, Ralph A., CALLED TO HEAL: RELEASING THE TRANSFORMING POWER OF GOD. Garden City, NY: Doubleday, 1982. 260pp. $14.95. Roman catholic priest DiOrio discusses the meaning and character of his well publicized healing abilities in an ecumenical spirit. Basically a book of his reflections on what he and others see as his gift of healing. Mainly a philosophical-theological essay.

Dömötor, Tekla, HUNGARIAN FOLK-BELIEFS. Bloomington: Indiana University Press, 1982. 324 pp. $17.50. A fascinating and authoritative over-voew of the folklore of Hungary, especially that of the 19th and 20th centuries. Much on the paranormal from fairies to werewolves. Surprises include the rarity of vampire lore in Hungary. Excellent bibliographical section and most interesting photographs. A descriptive rather than analytic survey by subjects, many related to supernatural and magic beliefs.

Dossey, Larry, SPACE, TIME & MEDICINE. Boulder, CO: Shambala, 1982. 248+xv pp. $8.95 paperback. A physician's argument for a systemic approach to medicine incorporating notions of nonlinear time and quantum physics concepts. A search for new, nonmechanistic models for health and illness.

Emmons, Charles F., CHINESE GHOSTS AND ESP: A STUDY OF PARANORMAL BELIEFS AND EXPERIENCES. Metuchen, NJ: Scarecrow Press, 1982. 307pp. $17.50. An extraordinary study using survey research methods on over 3600 subjects in Hong Kong. Striking similarities are revealed between this population and Western studies. Recommended for specialists.

Eysenck, H.J., and D.K.B Nias, ASTROLOGY: SCIENCE OR SUPERSTITION? London: Maurice Temple Smith, 1982. 244+xi pp. 7.95 pounds. A fine survey of the scientific case for astrology generally concluding in the negative but contending that cosmobiological work (especially that of M. Gauquelin) shows the possible presence of planetary effects. Highly recommended.

Eysenck, Hans J., and Carl Sargent, EXPLAINING THE UNEXPLAINED: MYSTERIES OF THE PARANORMAL. London: Weidenfeld and Nicolson, 1982. 192pp. 9.95 pounds. A highly illustrated and provocative survey by two proponents within psychology. Will not convince critics of psi but certainly should be read by them. Too frequently, results are spoken of as "found" rather than "reportedly found" when these studies are disputed by critics. The biases of the book, however, are transparent, and it covers a great deal of information in relatively short length in lucid fashion. Recommended.

Feyerabend, Paul K., PHILOSOPHICAL PAPERS, VOL. I: REALISM, RATIONALISM AND SCIENTIFIC METHOD and PHILOSOPHICAL PAPERS, VOL. II: PROBLEMS OF EMPIRICISM. N.Y.: Cambridge University Press, 1981. 353+xiv pp. and 255+xii pp. $44.50 and $34.50. Remarkable and important essays with new introductions for overview. Feyerabend's views favoring anarchism within science make him especially hospitable to maverick thinking and ideas connected with anomalies and paranormal claims. Though many of the essays in these volumes are for the specialist (including a surprising essay on Wittgenstein), much of what Feyerabend has to say is relevant for ZS readers, especially his essays dealing with the mind-body problem, the ideas of David Bohm, Karl Popper, and Thomas Kuhn. Highly recommended.

Fideler, David R., JESUS CHRIST IS THE SUN OF GOD: AN INTRODUCTION TO THE GNOSTIC ORIGINS OF THE CHRISTIAN MYTHOS. Grand Rapids, MI: Philosophical Book Service (P.O. Box 1181; 49501), 1982. 57+viii pp. $6.00 paperback. An ecclectic and creative little metaphysical-occult "text" in which Christ is seen as the "solar logos." Ascientific mumbo-jumbo unless you are into neo-Pythagorean mysticism.

Fuller, Robert C., MESMERISM AND THE AMERICAN CURE OF SOULS. Philadelphia: University of Pennsylvania Press, 1982. 227_xvi pp. $20.00. An important contribution to intellectual history showing the relations between mesmerism and the Mind-Cure and New Thought movements. Recommended.

Gallup, George, Jr., with William Proctor, ADVENTURES IN IMMORTALITY. N.Y.: McGraw Hill,1982.226pp.$12.95. A look at the several national polls taken by the Gallup organization on beliefs and attitudes toward the afterlife and related ideas. Many surprises,e.g., 23% of the adults polled believe in reincarnation and a larger percentage of the public than scientists believe in extraterrestrial life. A must.

Graves, Tom, DOWSING: TECHNIQUES AND APPLICATIONS. London: Mayflower, Granada, 1980. 190pp. 1.25 paperback. A general survey and how-to book on dowsing making no attempt to convince the skeptical. Based on a simple "try it and see for yourself" basis. Perhaps the best book for those less interested in validating dowsing than trying it (with no fears for the pitfalls of subjective validation).

Graves, Tom, editor, DOWSING AND ARCHAEOLOGY. Wellingborough, Northamptonshire: Turnstone Books, 1980. 122 pp. 2.75 pounds, paperback. A collection of articles from the Journal of the British Society of Dowsers. Fascinating sourcebook of field reports but disappointing if you seek scientific evidence or experimental arguments.

Graves, Tom, and Janet Hoult, editors, THE ESSENTIAL T.C. LETHBRIDGE. London: Granada, 1982. 267pp. 1.96 pounds, paperback. A good brief introduction to the writings of the eccentric archaeologist and with an informative introduction to his life and work by Colin Wilson. Lethbridge's wideranging speculations on ghosts, poltergeists, terrestrial magnetism, healing and even interplanetary warfare are represented here along with his better known work on dowsing and pendulum "experiments."

Gregory, Richard L., MIND IN SCIENCE, N.Y.: Cambridge University Press, 1981. 641+xi pp. $29.95. An impressive history from myth to modernity, ranging from Aristotle to Popper. Remarkably readable and learned. A fascinating survey by a neurophysiologist. Recommended.

Grim, Patrick, editor, PHILOSOPHY OF SCIENCE AND THE OCCULT. Albany: State University of New York Press, 1982. 336pp. $30.50 clothbound; $9.95 paperback. An excellent introductory text to the complicated problems surveyed, consisting of both reprinted and original papers (the latter being of special interest to ZS readers likely to be familiar with the former). The "occult" of the title is somewhat misleading since that term is not analytically defined except by opposition to "science." The book is really about the demarcation problem between science and pseudoscience using the examples of astrology, parapsychology, ufology, and the ancient astronaut hypothesis. Recommended.

Hahnemann, Samuel, ORGANON OF MEDICINE. Los Angeles: J.P. Tarcher, 1982. 269pp. $7.95 paperback. A translation of the 1810 classic work on homeopathy into contemporary language.

Hibbard, Whitney S., and Raymond W. Worring, PSYCHIC CRIMINOLOGY. Springfield, IL: Charles C. Thomas, 1982. 108+viii pp. $16.75. A very disappointing work given the large mass of literature on this subject. Credulous and uncritical of the earlier work examined and methodologically so undetailed as to be nearly worthless as a survey of the law enforcements purportedly interviewed.

Hirst, Paul, and Penny Woolley, SOCIAL RELATIONS AND HUMAN ATTRIBUTES. N.Y.: Tavistock Publications, 1982. 297+xi pp. $24.95 hardbound; $8.95 paperback. Relevant for ZS readers for its third section dealing with witchcraft and rationality.

Hitching, Francis, THE NECK OF THE GIRAFFE: WHERE DARWIN WENT WRONG. New Haven and N.Y.: Ticknor & Fields, 1982. 288pp. $13.95. An exceptionally lucid book presenting the problems with natural selection and Darwinist evolutionary theory. Not to be confused with a Creationist book (though it commends the Creationists for oten asking the right questions but providing unscientific answers), this volume is an excellent survey and introduction to the debates involving all quarters from Velikovsky to S.J. Gould. Recommended.

Hodson, Geoffrey, FAIRIES AT WORK AND AT PLAY. Wheaton, IL: Theosophical Publishing House, 1982. 126pp. $4.75 paperback. A charming little book but serious only for the believer in fairies.

Houston, Jean, THE POSSIBLE HUMAN: A COURSE IN EXTENDING YOUR PHYSICAL, MENTAL AND CREATIVE ABILITIES. Los Angeles: J.P. Tarcher, 1982. 229+xxv pp. $9.95 paperback. Lots of creative exercises which apparently are endorsed by many. I found a lot of it silly, but I don't claim to be in touch with myself. One man's meat may be another man's broccoli.

Hoyle, Fred, EVOLUTION FROM SPACE (THE OMNI LECTURE) AND OTHER PAPERS ON THE ORIGIN OF LIFE. Hillside, NJ: Enslow, 1982. Perhaps Hoyle's most controversial writings. Fascinating stuff.

Kelly, Edward F., and Ralph G. Locke, ALTERED STATES OF CONSCIOUSNESS AND PSI: AN HISTORICAL SURVEY AND RESEARCH PROSPECTUS (Parapsychology Monograph No. 18). N.Y.: Parapsychology Foundation, 1981. 94 pp. $6.00 paperback. A review and call for new research according to a model developed. Well done.

Kendon, Adam, editor, NONVERBAL COMMUNICATION, INTERACTION, AND GESTURE; SELECTIONS FROM SEMIOTICA. N.Y.: Mouton, 1981. 549+viii pp. $40.00 clothbound; $17.00 paperback. An important collection of semiotics of special interest for ZS readers re such matters as cueing behavior in cold readings, Clever Hans phenomena, etc. Fascinating readings on the subtleties and multichannel character of communication.

Kitcher, Philip, ABUSING SCIENCE: THE CASE AGAINST CREATIONISM. Camrbidge, MA: MIT Press, 1982. 213+x pp. $15.00. A "manual" for defense by evolutionists against creationists. Kitcher has taken the trouble to read the arguments of the creationists and generally does an excellent job of reply.

Klarner, David A., editor, THE MATHEMATICAL GARDNER. Belmont, CA: Wadsworth International, 1981. 382+viii pp. $24.95. Thirty original articles for Martin Gardner on his 65th birthday. Though best known to most ZS readers for his critical writings on the paranormal, he is primarily renown for his work in recreational mathematics. Delightful essays and I particularly enjoyed "Supernatural Numbers" and "In Praise of Amateurs."

Knorr-Cetina, Karin D., THE MANUFACTURE OF KNOWLEDGE: AN ESSAY ON THE CONSTRUCTIVIST AND CONTEXTUAL NATURE OF SCIENCE. Elmsford, NY: Pergamon, 1981. 189+xiv pp. $30.00. An important work in the sociology of science with special significance for the protosciences. Recommended for the specialist.

Lind, Tom, compiler, THE CATALOGUE OF UFO PERIODICALS: A SAID OF SAUCERS PUBLICATION, July 1982. (Available from Tom Lind, P.O. Box 711; Hobe Sound, FL 33455-0711.) 281+iv pp. $ paperback. A remarkable compilation on UFO publications information for about 1500 going and defunct sources. Periodical supplements will be issued for this computerized inventory. Highly recommended.

Lundahl, Craig R., editor, A COLLECTION OF NEAR-DEATH RESEARCH READINGS. Chicago: Nelson-Hall, 1982. 240+xv pp. $19.95. A nice collection of reprinted and original essays with an excellent essay by Michael Grosso reviewing the explanations that have been offered for this phenomenon.

McAdams, Elizabeth E., and Raymond Bayless, THE CASE FOR LIFE AFTER DEATH: PARAPSYCHOLOGISTS LOOK AT THE EVIDENCE. Chicago: Nelson-Hall, 1981. 157+vii pp. $15.95. Clearly written and covers a wide area but hardly a truly critical work or a serious parapsychological effort.

Moss, Peter, with Joe Keeton, ENCOUNTERS WITH THE PAST: HOW MAN CAN EXPERIENCE AND RELIVE HISTORY. Harmonsworth, Middlesex: Penguin Books, 1981. 233pp. 1.50 pounds. A popular audience book on past-life regression via hypnosis. Interesting case histories but neither scholarly nor science.

Mullin, Raymond, MIRACLES AND MAGIC: THE MIRACLES OF SPELLS OF SAINTS AND WITCHES. London: Mowbray, 1978. 300pp. No price given. A survey of miracle literature concentrating on the medieval but ranging over immense territory. An excellent scholarly but entertaining survey.

Neiser, Ulric, editor, MEMORY OBSERVED: REMEMBERING IN NATURAL CONTEXTS. San Francisco: W.H. Freeman, 1982. 433+xiii pp. $12.50 paperback. An excellent scholarly collection emphasizing the need to study memory as it works in nonlaboratory setttings. Highly relevant to anomalies research since human testimony needs to be examined light of what we know of the fallible character of memory.

Newton-Smith, W.H., THE RATIONALITY OF SCIENCE. Boston: Routlege & Kegan Paul, 1981. 294+xxii pp. $19.50. A defense of the rational character of science against the recent criticisms of science by Kuhn, Lakatos, Feyerabend and Popper. Proposal of a "temperate rationalism."

O'Keefe, Daniel Lawrence, STOLEN LIGHTNING: THE SOCIAL THEORY OF MAGIC. N.Y.: Continuum, 1982. 581pp. $24.50. A quite remarkable and encyclopedic survey of the sociology-anthropology of magic evidencing erudition and first-class scholarship. Should be read by all sociologists with a concern for belief systems and a better understanding of the role of magic in social life. Highly recommended.

Parise, Frank, editor, THE BOOK OF CALENDARS. N.Y.: Facts-on-File, 1982. 387pp. $29.95. A huge compendium of the world's many calendars and calendar systems. A basic reference work.

Pavlos, Andrew J., THE CULT EXPERIENCE. Wesport, CT: Greenwood Press, 1982. 209+xvi pp. $27.50. An excellent and balanced book on the social psychology of religious cults by a psychologist. A good introduction to a growing literature in this area but needs to be supplemented by the recent sociological studies (e.g., that of Shupe and Bromley) for a fuller picture.

Planer, F.E., SUPERSTITION. London: Cassell, 1980. 376pp. 9.95 pounds. A highly critical overview of prediction of the future, the world of spirits, and magical practices. Not without some interesting observations but the sort of criticism of the paranormal that infuriates it proponents and with much cause. Simply a superficial and highly opinionated analysis of things the author really does not know very much about.

Playfair, Guy Lyon, THIS HOUSE IS HAUNTED: THE INVESTIGATION OF THE ENFIELD POLTERGEIST. London: Sphere Books, 1981. 275+xi pp. 1.50 pounds, paperback. An interesting but unconvincing account of an alleged poltergeist.

Randi, James, FLIM FLAM! PSYCHICS, ESP, UNICORNS AND OTHER DELUSIONS. Buffalo, NY: Prometheus, 1982. 342 pp. $9.95 paperback. A new edition with minor changes from the earlier hardbook. The book's reprinting is heralded as necessitated by publishers' bias towards paranormal books, ignoring the great many anti-occult books including ones published by this book's original publisher. Unfortunately, Randi has not been adequately responsive to the critics of his earlier edition since the corrections in this edition are inadequate. Nonetheless, the virtues of the book certainly warrant its remaining in print and the new edition is therefore welcome.

Randi, James, THE TRUTH ABOUT URI GELLER. Buffalo, NY: Prometheus, 1982. 235+vii pp. $4.95 paperback. A revised and enlarged edition of the 1975 THE MAGIC OF URI GELLER. A welcome edition of Randi's best book which contains important updating materials that should be of interest to anyone re the rise and eclipse of Uri Geller. I hope a future edition might add an index. Recommended.

Rao, K. Ramakrishna, editor, J.B. RHINE: ON THE FRONTIERS OF SCIENCE. Jefferson, NC: McFarland, 1982. 263+vii pp. $19.95. A festschrift for the late Dr. Rhine incorporating both papers presented at his memorial conference and several new papers prepared especially for this volume. Includes not only appreciations for his work but some important critical statements. Recommended.

Reiser, Martin, POLICE PSYCHOLOGY: COLLECTED PAPERS. Los Angeles: LEHI Publishing Co., 1982. 355+xvii pp. $24.95. . Of special interest for ZS readers because two papers in this collection deal with the use of psychics by police agencies. Both experiments used control groups and showed negative but revealing results.

Richards, John Thomas, SORRAT: A HISTORY OF THE NEIHARDT PSYCHOKINESIS EXPERIMENTS, 1961-1981. Metuchen, NJ: Scarecrow Press, 1982. 356pp. $17.50. A description of the macro-PK claims of the Society for Research on Rapport and Telekinesis in Missouri. These claims have made little positive impression on either parapsychologists or critics. Scientifically just about worthless and I felt after getting into the book like the "spirit" who at one point answers: "Who cares? This is the sort of book that critics of parapsychology laugh aloud at while reading it.

Robinson, Diana, TO STRETCH A PLANK: A SURVEY OF PSYCHOKINESIS. Chicago: Nelson-Hall, 1981. 277pp. $17.95. A general review of the psychokinesis literature which is thoroughly uncritical but entertainingly written and good for introducing the novice to a wide literature.

Ruse, Michael, DARWINISM DEFENDED: A GUIE TO THE EVOLUTION CONTROVERSIES. Reading, MA: Addison-Wesley, 1982. 356+xvii pp. $12.50 paperback. A vigorous defense of Darwinist thought including a perhaps too strong attack on the Creationists in that he would totally exclude such ideas from the science curriculum. Surely if their ideas are as stupid as Ruse claims, teaching science students to see through them would produce more critical scientists and the ideas should have little negative impact. Creationist answers may be foolish but their questions may be useful.

Sebeok, Thomas A., THE PLAY OF MUSEMENT. Bloomington: Indiana University Press, 1981. 312+vi pp. $35.00. A marvelous collection of essays showing remarkable erudition while entertaining and teaching. Sebeok covers everything from "Star Wars" to Zoosemiotics as he speculates upon the associations within the universe of signs. From Sherlock Holmes and C.S. Peirce to the aesthetic impulse in animals to Clever Hans. Scholarship, humor and imagination; a pleasure.

Sheldrake, Rupert, A NEW SCIENCE OF LIFE: THE HYPOTHESIS OF FORMATIVE CAUSATION. Los Angeles: J.P. Tarcher, 1981. 229pp. $12.95. A controversial theory of "morphogenic fields" which shape the shapes and instincts of living organisms through "morphic resonance" --direct connections across both space and time. An unorthodox approach to evolution which is currently undergoing empirical testing and which could prove revolutionary if substantiated. A wild but sober theory.

Shepard, Leslie, editor, ENCYCLOPEDIA OF OCCULTISM & PARAPSYCHOLOGY SUPPLEMENT. Detroit: Gale Research, 1982. 231pp. $70.00. A hardbound volume integrating the supplemental information formerly available in the Occultism Update volumes issued to supplement this encyclopedia. Full indexing and cross-referencing is included. Particularly valuable for its many address to minor publications and specialized journals. Recommended.

Smullyan, Raymond, THE LADY OR THE TIGER? AND OTHER LOGIC PUZZLES INCLUDING A MATHEMATICAL NOVEL THAT FEATURES GODEL'S GREAT DISCOVERY. N.Y.: Alfred A. Knopf, 1982. 226+ix pp. $13.95. A wonderful new collection of puzzles for those interested in logic and seeming anomalies. Many of the problems involve strange creatures with unusual characteristics. Thought provoking recreations.

Solomon, Jack and Olivia, compilers, GHOSTS AND GOOSEBUMPS: GHOST STORIES, TALL TALES, AND SUPERSTITIONS FROM ALABAMA. University: University of Alabama Press, 1981. 202+xii pp. $18.95. A splendid folklore collection of regional materials. Entertaining lore.

Sontag, Frederick, and M. Darrol Bryant, editors, GOD: THE CONTEMPORARY DISCUSSION. N.Y.: Rose of Sharon Press, 1982. 419+vi pp. $12.95. The Unification Theological Seminary sponsors seminars as part of its New Ecumenical Research Association and this is a collection of the papers from its large 1981 conference in Hawaii.

Spence, Lewis, BRITISH FAIRY ORIGINS. Wellingborough, Northamptonshire: Aquarian Press, 1981. 206+ix pp. No price listed, paperback. A welcome reissue of this 1946 work by Spence, the author of many works dealing with British mysteries including other books on faeries.

Summers, Montague, editor, THE SUPERNATURAL OMNIBUS. North Pomfret, VT: Victor Gollancz, 1982. 622pp. A new edition, reprinting the classic 1931 collection by the late Montague Summers, perhaps the greatest "Catholic" scholar on "works of the devil." Thirty-six stories and an introduction by Summers. The subtitle says it all: "Being a collection of stories of apparitions, witchcraft, werewolves, diabolism, necromancy, satanism, divination, sorcery, goety, voodoo, possession, occult doom and destiny." Perhaps the best single collection of such stories.

Thompson, Clive, editor, SITE AND SURVEY DOWSING. Wellingborough, Northamptonshire: Turnstone Press, 1980. 118+x pp. 2.75 pounds, paperback. An anthology of articles from the Journal of the British Society of Dowsers. I found D.M. Lewis's "Why the Scientist Doubts the Dowser" of special interest.

Whitlock, Ralph, WATER DIVINING AND OTHER DOWSING: A PRACTICAL GUIDE. North Pomfret: David & Charles, 1982. 144pp. $14.95. A "practical" rather than scientific guide to dowsing in which quite extravagant claims are made for dowsing's use in health and disease diagnosis, archaeology, and with maps and photographs rather than actual sites. Basically a "how-to" book.

Wilber, Ken, editor, THE HOLOGRAPHIC PARADIGM AND OTHER PARADOXES: EXPLORING THE LEADING EDGE OF SCIENCE. Boulder, CO: Shambala, 1982. 300pp. $8.95 paperback. An outgrowth of the papers in ReVision Journal around the attempts to integrate religion and science around the works of Karl Pibram and David Bohm. Fascinating speculation linking East/West and mystical thought with quantum physics. The book unfortunately frequently states things like "the theory demonstrates" as though these matters have been empirically validated rather than conjectured. Nonetheless, stimulating stuff and a good introduction to the emerging dialogue around these ideas.

Wilson, Bryan, editor, THE SOCIAL IMPACT OF NEW RELIGIOUS MOVEMENTS. Barrytown, NY: Unification Theological Seminary (distributed by Rose of Sharon Press), 1981. 236+xix pp. $10.95. A very interesting collection with some excellent papers. I found "The Rise and Decline of Transcendental Meditiation" by Bainbridge and Jackson of special interest.

Wilson, Colin, FRANKENSTEIN'S CASTLE: THE RIGHT BRAIN: DOOR TO WISDOM. Sevenoaks, Kent, England: Ashgrove Press, 1980. 128pp. 2.95 pounds. A somewhat rambling discourse on harnassing the forces of the right side of the brain. Wilson seems unaware of the critical work in this area. Nonetheless, Wilson brings in all sorts of interesting literary and philosophical material as well as much personal anecdote that recommends the book despite what I thought was a lack of focus in it.

Wilson, Colin, POLTERGEIST: A STUDY IN DESTRUCTIVE HAUNTING. N.Y.: G.P. Putnam's Sons, 1982. 382pp. $14.95. Perhaps the wildest book on poltergeists in recent years since Wilson believes that the macro-PK theories are wrong and that we really are dealing with disembodied spirits in these cases. In addition to its "reactionary" viewpoint, Wilson also makes a number of factual errors. Essentially a supernaturalistic rather than parapsychological approach to the phenomena.

Wilson, Colin, and John Grant, editors, THE DIRECTORY OF POSSIBILITIES. London: Corgi Books, 1981. 303pp. 2.50 pounds, paperback. A quite useful compendium of short encyclopedic articles about all sorts of topics, personages, and sites of anomalous and improbable character, written by 13 contributors. Much fascinating stuff, generally well handled.

Wilson, Ian, ALL IN THE MIND. Garden City, NY: Doubleday, 1982. 268+xix pp. $15.95. Perhaps one of the best critical works on reincarnation ever produced. A careful look at past-life regression cases in relation to the literature on multiple personalities. Essentially a constructively skeptical approach which recognizes mysteries of the human mind while discrediting reincarnation claims based on regression materials. Recommended.

Ziman, John, PUZZLES, PROBLEMS AND ENIGMAS: OCCASIONAL PIECES ON THE HUMAN ASPECTS OF SCIENCE. N.Y.: Cambridge University Press, 1981. 373+ox pp. $14.95. A collection of essays, many of them radio talks on many aspects of the sociology of science. Many of the essays should be of great interest to ZS readers. Among these would be "Some Pathologies of the Scientific Life," "Some Manifestations of Scientism," but my own favorite is the most amusing "Whistle-blowing."

Zohar, Danah, THROUGH THE TIME BARRIER: A STUDY OF PRECOGNITION AND MODERN PHYSICS. London: William Heinemann, 1982. 178+xii pp. 8.50 pounds. I found this pro-precognition book quite impressive. The author recognizes that the evidence she marshall would not convince an independent panel of disinterested scientists for the "controlled production of precognitive data under repeatable experimental conditions" has so far not been gathered. Nonetheless, she presents us with a fine array, usually but not always critically examined, of the existing evidence and discusses this in the light of contemporary ideas in physics. Since the author is cognizant of the limitations of her evidence and argUments, and does not normally overstate her case, the book is to be respected even by those of us who might evaluate the evidence differently. Recommended.

Zusne, Leonard, and Warren H. Jones, ANOMALISTIC PSYCHOLOGY: A STUDY OF EXTRAORDINARY PHENOMENA OF BEHAVIOR AND EXPERIENCE. Hillsdale, NJ: Lawrence Erlbaum, 1982. 498+xiii pp. $29.95. A very important new textbook which generally takes a critical but fair-minded and open approach to claims of the paranormal. To be reviewed extensively in a forthcoming issue of ZS.

CENTER FOR SCIENTIFIC ANOMALIES RESEARCH

A CSAR REPORT

Announcements of the formation of CSAR appeared in ZS#8 and ZS#9. Readers interested in details about its functions should see those announcements or can write to CSAR (address below) for information.

The CSAR DIRECTORY OF CONSULTANTS is now underway. About 100 experts on various phases of anomalies have applied to CSAR and been accepted. We hope to obtain another 100 applications and encourage appropriate ZS readers to apply. Being a CSAR Consultant does not make one a member of CSAR, and listing is a free service; the purpose of the directory is to create a public network of experts re anomaly research covering <u>all</u> spectrums of opinion.

The first issue of THE CSAR BULLETIN should be out soon. It has been decided to postpone membership openings in CSAR for several months. Details will appear in ZS#11.

CSAR is currently sponsoring four projects: (1) the Psychic Sleuths Project, which examines uses of alleged psychics by law enforcement agencies (see the several reports already published in ZS); (2) the Anomaly Project,which deals with the UFO poll of industrial scientists and engineers (see the report in ZS#8); (3) the Chinese Parapsychology Monitor (for which CSAR received funding to send its Director to China; see the bibliographic report in this issue); and (4) the Soviet-U.S. Military Psi Research Monitor (a compilation of materials related to governments' parapsychology efforts).

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* To advance the interdisciplinary scientific study of alleged and verified anomalies. * To act as a clearinghouse for scientific anomaly research. * To publish a journal (ZETETIC SCHOLAR), a newsletter (THE CSAR BULLETIN), research reports and bibliographies. * To promote dissemination of information about scientific anomaly research. * To create a public network of experts on anomaly research through publication of the CSAR DIRECTORY OF CONSULTANTS. *To sponsor conferences, lectures and symposia related to anomaly research. * To promote improved communication between critics and proponents of scientific anomaly research.