

Zetetic scholar

JOURNAL OF THE CENTER FOR SCIENTIFIC ANOMALIES RESEARCH

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- **CONFESSIONS OF A FORTEAN**
- **FIRE-WALKING**
- **UNCANNY PROPHECIES IN NEW ZEALAND?**
- **PARAPSYCHOLOGY**
- **THE MARS EFFECT**
- **CRYPTO-SCIENCE**



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ANOMALIES AND THE PARANORMAL



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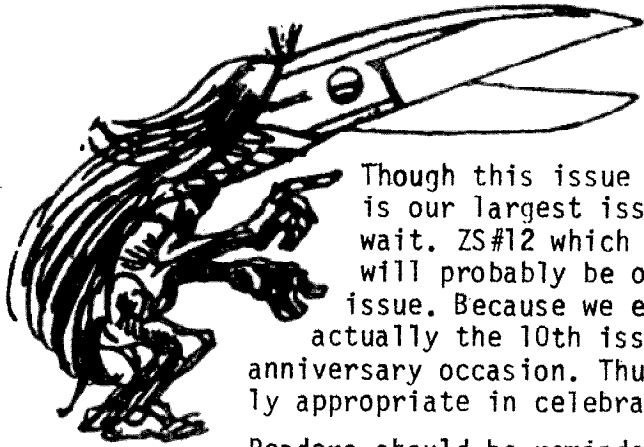
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LETTERS

Despite his answer to me (ZS 10:154), I think J. Richard Greenwell has not looked closely enough at his implication that Carl Sagan is inconsistent and that the inconsistency requires us "to assume that the environmental conditions prevailing on other planets were much more favorable for the evolution of intelligent species than they were on Earth" (ZS 8:52). According to Greenwell, Sagan said (A) that "the number of fortuitous accidents which had to occur at the right time for man to develop the way he has is truly astronomical," and (B) that "intelligence is an inevitable consequence of biological evolution, given enough time." Greenwell finds these two statements to be contradictory, but I find them to be quite harmonious. They seem to say that intelligence may develop on a million planets but human beings will develop on only one. Sagan may be wildly optimistic, but he is not inconsistent.

Greenwell (ZS 10) cites Sandon to the effect that the odds against the evolution of modern man are 2^{100} to 1. Having with my own eyes actually seen modern man and even modern woman on various occasions right here in Santa Barbara, I put those odds at zero. What the odds are greatly against--as Greenwell would agree--is duplicating the human evolutionary chain elsewhere. Nevertheless, on a million of the best planets there may be a million extraterrestrial ways to evolve an intelligent being. Since we have only one case in our sample and can't explain even that case, we are hardly in a position to judge the viability of extraterrestrial evolutions.

-- Richard de Mille
Santa Barbara, CA



EDITORIAL

Though this issue is decidedly a couple of months late, it is our largest issue to date and, I hope, should be worth the wait. ZS#12 which is scheduled to come out in late December will probably be on time, and will probably be a normal sized issue. Because we earlier had a double issue (#3/4), this is actually the 10th issue of ZS published and constitutes a sort of anniversary occasion. Thus, this very large #11 issue seems particularly appropriate in celebration.

Readers should be reminded that ZS shoots for two issues per year but is actually scheduled irregularly with subscriptions being for two issues rather than the issues of a single year. The Dialogue character of ZS makes it particularly hard to follow a rigid schedule since I am often waiting for X to send in a promised reply to Y. To facilitate matters in the future, I have decided to generally try to follow the format of having a stimulus paper and its commentaries in one issue and the reply by the author of the stimulus paper in the next issue. This should help me get issues out on time.

The Center for Scientific Anomalies Research is now pretty well organized in terms of its basic structure, and plans are being laid for a number of special events and activities for CSAR. I call your attention to the full announcement of CSAR at the end of this issue, including information about membership categories that are available.

The general policy at ZS is to publish only original papers and not to reprint articles. Jerome Clark's "Confessions of a Fortean," I have been told, was recently published in England in a UFO publication that failed to get the author's permission. This was unfortunate and does not mean a change in ZS policy.

When a ZS stimulus paper is sent out for commentaries, about twice as many experts are invited as actually decide to participate. A number of readers have asked why X or Y was not invited to comment; usually they were but declined to participate. I have been tempted to publish the names of all those invited, but declining to participate, I feel, should include total non-participation, and readers might misread the reasons for such refusals.

Once the author of a stimulus paper has responded to his/her commentators, the field is open for all ZS readers to join in the continuing dialogue. However, I will not accept comments from the readers (without special cause) for ZS publication until after the original author has a chance to reply. The obvious exception is when a reader has special information of relevance unlikely to be known to the author of the paper. Otherwise, we will observe the courtesy of letting the paper's author first make his/her points about the analyses of the commentators.

The last several issues of ZS have included a number of critical papers

dealing with Michel Gauquelin's "Mars Effect." This is continued in this issue. I am pleased to call readers' attention to "The Abell-Kurtz-Zelen 'Mars Effect' Experiment: A Reappraisal" which appears in CSICOP's journal THE SKEPTICAL INQUIRER of Spring 1983 (pp.77-82). Though that article does not mention any of the critical papers that have appeared in ZS, it is clearly --at least in part-- a reponse to the papers by Patrick Curry and, especially, Richard Kammann which appeared in ZS#9 and #10.

In this issue of ZS, Patrick Curry and Piet Hein Hoebens register their views on this "reappraisal," and I think it important that I note that the pieces by Professors Flew and Eysenck in this issue were received before the "reappraisal" was published. Michel Gauquelin has indicated that, aside from whatever reservations he may have about the adequacy of this reappraisal, the article is "courageous." I would like to publicly agree and commend George Abell in particular for his efforts in getting this "reappraisal" published. I would also like to go on record as indicating that I find myself in full agreement with the remarks of Piet Hein Hoebens in this issue. All scientists are human and all of us make mistakes, but the important thing is that we try to respond to our critics and keep science a self-correcting system by acknowledging errors. That has now been done.

It is an unfortunate fact that many persons associated with CSICOP have imagined that I was interested in discrediting that committee. My goal has never been that. My goal has been either to force reforms within CSICOP that would make it live up to its own stated goals or (more likely) to allow the general scientific community to see that CSICOP was an advocate body (which is not in itself discreditable at all) just as are the various anomaly organizations CSICOP attacks advocate bodies. I think these goals have now both been met. I think CSICOP is unlikely to make the same mistake again (especially now that they published a statement that they would no longer conduct research --in THE SKEPTICAL INQUIRER of Spring 1982, p.9), and I think the scientific community now clearly has the evidence before it that shows CSICOP is not the purely objective and neutral and non-pre-judgemental body that some have pretended.

The important thing now, as Hoebens has pointed out, is that new independent studies be conducted to test the validity of Gauquelins' findings, which include numerous extraordinary correlations other than just the "Mars Effect." I hope that CSICOP and other anomaly-interested groups will now encourage such needed research. CSAR and I will be very happy to act towards that end. Again, I congratulate CSICOP on publishing the "reappraisal," and I hope this clears the air for fuller cooperation between CSAR and CSICOP.

The full matter of James Randi's "Operation Alpha," which involved duping several parapsychologists into "accepting" Randi's accomplice magicians who were planted in a psi research laboratory, will be discussed in a coming issue (probably #12) of ZS. But I am pleased to let ZS readers know that the Parapsychological Association has announced that it is contacting the major magicians' societies for lists of appropriate members who might usefully consult with psi researchers in the future. I would like to congratulate the Parapsychological Association's executive council for this important move. Though Randi's actions may have been something of a catalyst towards this action, the move had been proposed to the PA council by me two years ago and is also related to a special roundtable panel of conjurers which was convened at the Parapsychological Association's meeting this summer. That panel included eight magicians.

CONFESSIONS OF A FORTEAN SKEPTIC

JEROME CLARK

The nadir of my career as a Fortean was reached in 1973 when I was researching and writing an article which subsequently appeared in Fate. The article was later incorporated into the text of The Unidentified, a book coauthored by Loren Coleman, who is otherwise blameless in the horror story to follow.

Years before then, back when I was 11 or 12 years old, I was rummaging through the library of the small Minnesota town where I grew up. I came upon a book entitled The Coming of the Fairies by Sir Arthur Conan Doyle. It dealt with a series of photographs taken by two young English girls who claimed that they regularly encountered fairies in a wooded area near their Cottingley, Yorkshire, home. In due course they produced pictures of these beings. The pictures, which appear in Doyle's book, struck me as hilariously unconvincing. The "fairies" resembled nothing so much as cardboard cutouts.

Many years later I read Jacques Vallee's Passport to Magonia and was taken with his attempt to link traditional fairylore to modern fly-ing saucer lore. I began reading in the considerable scholarly literature on fairy beliefs. In one of these books, Katherine Briggs' The Fairies in Tradition and Literature, I came upon a brief account of the Cottingley episode, about which Dr. Briggs, one of Britain's leading folklorists, wrote, "As one looks at these photographs, every feeling revolts against believing them to be genuine." Yet, noting some of the unexplained aspects of the affair, she went on guardedly to suggest that the pictures might be psychic photographs.

She was troubled by a few odd items of evidence, such as the testimony of three photographic experts who said they didn't know how the pictures could have been faked.

Intrigued, I reread Doyle's book and two others on the subject. I was impressed not so much by the testimony of the photographic experts as by the demonstrated inability of would-be debunkers to come up with plausible, nonextraordinary explanations. Typical of the blunders was Houdini's bold assertion that the models for the fairy figures came from a certain advertising poster. This allegation was widely published and uncritically accepted. But eventually, when investigators located copies of the poster in question, they found that the "fairies" depicted on it looked not at all like those in the Cottingley pictures.

I was also interested to read that as late as the early 1970s, over 50 years after the events in question, the two photographers, both now elderly women, seemed to stand by their earlier testimony.

So, following Briggs' lead, I cast all caution to the wind. I was

*A revised version of a paper delivered at the annual meetings of the International Fortean Organization at the University of Maryland, October 1981.

at least wise enough to concede that the Cottingley fairies didn't "look" real but dismissed that as a subjective consideration. To me the absence of convincing negative evidence, coupled with the presence of positive evidence (however thin), added up to the conclusion that these might be authentic "thoughtographs" much like those Ted Serios is said to produce.

To this day I can't believe how stupid and how credulous I was.

As we know now beyond any reasonable doubt, the Cottingley pictures are clumsy and absurd fakes. In his 1978 book Ghosts in Photographs Fred Gettings reveals that the models for the figures came from a popular children's book of the period. Photoanalysis by William Spaulding's Ground Saucer Watch has shown that yes indeed, the figures are of cardboard, just as my 11-year-old eye had told me many years ago.

Robert Sheaffer, in his effort to debunk the story, contributed to the grand tradition of misleading nonsense by claiming, on the basis of the thinnest possible circumstantial evidence, that Theosophical writer Edward Gardner was the mastermind behind the hoax -- an assertion that quickly fell victim to Occam's Razor, but not before proving once again that the Cottingley affair could as easily make fools of disbelievers as of believers.

In their recent books nonadmirers of mine like Sheaffer and Martin Gardner have resurrected my foolish remarks on these nonfairy-nonthoughtograph pictures in an effort to discredit me. Sheaffer even claims that he, as the man who commissioned Spaulding to analyze the pictures in 1977, "forced" me to relinquish my support. He doesn't mention that to the contrary, I accepted this first truly solid negative evidence with almost unseemly haste, in part because I like to think I am intellectually honest and in part because on some level -- specifically the level of my psyche at which the embers of common sense still glowed, however faintly -- I had long suspected that in taking the pictures seriously I was making a very, very dumb mistake.

Another mistake was in assuming the existence of "thoughtographs," the evidence for which is shaky at best. In other words, I had attempted to explain a dubious claim with another dubious claim. Realizing belatedly that I was lost deep in a jungle of Fortean unreality, I decided that it was high time to cut and slash my way through the undergrowth and return to safety, sanity and skepticism. At the end of my harrowing adventure, my hair was whiter but my head was clearer.

The moral of the story is this:

- (1) There is something to be said for common sense.
- (2) Just because the debunkers are wrong, it doesn't necessarily follow that therefore the proponents are right.
- (3) The time had come for this proponent to do some serious rethinking of his position.

* * *

There is a wonderful piece of verse by Spiritualist poet Ella Wheeler Wilcox. Its title is "Credulity" and it goes:

If fallacies come knocking at my door
I'd rather feed and shelter full a score
Than hide behind the black portcullis Doubt
And run the risk of barring one Truth out.

And if pretention for a time deceive
And prove me one too ready to believe
Far less my shame, than if by stubborn act
I brand as lie, some great colossal Fact.

That sounds to me like a prescription for the kind of "open-mindedness" that permits the brains to fall out of one's head. But it is an apt description of a mentality we encounter all too frequently on this side of the paranormal controversy. It's the Will to Believe coupled with the Refusal to Disbelieve. It is the mindset that is skeptical only of claims of fraud or error.

To achieve it, one starts with the love of mystery. There's nothing wrong with that in and of itself. The problem is that some of us, even after all this time, even after we have no excuse for not knowing better, seem more interested in pursuing mysteries than in securing answers. To some, mystification is the beginning and end of paranormal inquiry. Mysteries are to be preserved and defended at all costs. And that may be why, after all this time, all we have to show for our efforts are a seemingly unending number of unanswered questions and a certain grotesque satisfaction in declaring, as one of the literature's enduring cliches goes, that such-and-such a mystery remains unsolved -- proclaimed, incidentally, as an expression of triumph, not as an admission of defeat.

I suggest we take a fundamentally different view. If we are to make any progress in our inquiry, we would be better off celebrating the solutions of mysteries rather than the perpetuation of mysteries.

Charles Fort himself was less a lover of mysteries than an eccentric with a perverse taste for the kind of pompous humbug associated with authority figures who feel they must account for unaccountable phenomena about which they not only know little but apparently prefer to know little. The resulting "explanations" are predictably preposterous and it is not hard to conclude that the explainers suffer from a case of anomaly-phobia sufficiently advanced to severely impair their reasoning faculties.

Anomaly-phobia, of course, continues to claim its victims. We all remember how the Air Force dealt with UFOs -- identifying them, for example, as astronomical bodies not even visible at the time of the reported sighting. We have all seen the inept criticisms of psi, lake-monster reports and other anomalous claims. We have listened incredulously to self-appointed protectors of the public welfare who assert, apparently with straight faces, that acceptance of unexplained phenomena is not only wrong but dangerous, perhaps even conducive to the collapse of civilization. Some of us have exposed the errors and baseless claims of the debunkers, and recently we have seen scandalous revelations about the way these would-be defenders of science and reason deal with evidence that runs contrary to their beliefs.

Reading Fort and tracing all that has happened since his time, a number of paranormal proponents seem to have concluded that because some mundane explanations are bogus, most or all are bogus. In ufology, for instance, the standard line has it that 90 to 95 percent of raw reports are potentially explainable; still, to some in the field, just about any specific raw report of an object in the sky is of a UFO. Some enthusiasts still believe that Jimmy Carter saw a UFO, not the planet Venus, and that many of our astronauts encountered UFOs in space.

More Fortean than we might care to admit still consider the Bermuda Triangle a genuine mystery, despite Larry Kusche's masterful expose in The Bermuda Triangle Mystery Solved. In fact, the Triangle, along with its similarly fictitious counterparts, the "vile vortices" of the world, still occupies a prominent place in the fertile imaginations of a few theorists. The alleged powers of Uri Geller and other metal-bending wonder-workers are blithely assumed to be real and incorporated into extraordinary explanation-schemes, even though the only thing about metal-bending that has ever been established with undeniable certainty is that fraud figures largely in the phenomenon. And our ranks are infested with guileless souls who still look to the novels of Carlos Castaneda as support for their metaphysical views. All things are possible in a separate reality, we are told, but we are not warned that all things are possible as well in Cloud Cuckooland.

Those who wish to return to earth might consider some ways of getting back. Here are a few:

(1) Don't assume that the experts are always fools.

Scientists and other scholars are not infallible, it need hardly be said. They are human beings and they have human failings, prejudices and blindnesses. But at the same time we must always remember that as specialists who have devoted their professional careers to their special areas of interest they are likely to know far more than you do about these subjects. If you take issue with them, chances are they are right and you are wrong. It is even possible that you are a crank.

On the other hand, if a scientist pronounces on something outside his area of expertise, then he is an amateur and he has no greater claim on the truth than any other untrained commentator. When an eminent astronomer presumes to tell us what to think about UFOs, it is often immediately apparent to anyone who knows the literature that the man is talking through his hat. When, however, that same astronomer talks astronomy, better listen. And if you don't agree with him, proceed very cautiously.

(2) Don't believe every story you hear.

Some months ago my wife was babysitting for a married couple of our acquaintance. The man was an officer in the Army reserve, holding a high security clearance which rendered him privy to various military and intelligence secrets. He worked as a research scientist at a major university.

He regularly confided some of these secrets to his wife, who then confided them to my wife, who then told them to me. Beyond recalling that all these presumed secrets were sensational in nature, I have

forgotten most of them. Of those I remember, one -- related in the midst of the Iranian hostage crisis -- was that our government knew the Iranian militants had executed several of their American captives. My informant also said that on a particular date the United States would invade Iran. You get the idea.

I never believed any of this, needless to say, but I couldn't resist the temptation to ask him -- tongue firmly embedded in cheek -- if, as a man well-versed in hidden truths, he knew if there were any substance to those stories about crashed saucers and pickled aliens purported to be in the Pentagon's possession. He immediately assumed a stern, official-looking expression and declared that was something he couldn't talk about. Not long afterwards, however, he added that the truth, if he were to confide it, would shock me. On two or three subsequent occasions he brought up the subject and let it be known that if I pressed him at all, he would tell me the whole story. For obvious reasons I never bothered.

I mention this as a cautionary tale. Remember, the man has impeccable credentials. He is a military officer; he does have a high security clearance; and he is a research scientist at a major university. And he is also, it is clear, a spinner of yarns. Next time you read a story about a crashed saucer told by a man with similarly impressive credentials, remember him.

In fact, there is a whole branch of modern folklore waiting to be seized upon and catalogued by scholars of popular culture. These are what I call "Soldier's Tales; or, the Horrendous Secrets I Learned in the Service." We ufologists hear them all the time. A few even purport to be first-hand accounts describing involvement in retrievals of crashed spaceships, the taking of spectacular UFO films, the witnessing of a fatal encounter between an airplane and a UFO, and so on. Such stories -- or at least those with enough specific detail to permit follow-up investigation -- seldom check out.

I can only speculate on the motives of the yarn-spinners, but it's not unreasonable to theorize that for many people the most important period in their lives was the time they spent in the military, when in fact some may well have been privy to secret information. All human institutions, including intelligence agencies, have rumor mills through which stories may circulate. The environment in which such fantasies are related may give them a false authority. Those individuals who pass into civilian life may repeat the rumors in good faith. Other persons, not acting in good faith, may simply place themselves inside the rumors to impress girl friends, wives and acquaintances.

(3) Don't get emotionally involved.

I have always been amazed at the tenacity with which some people hold to favorite beliefs and the rationalizations to which they will resort when these beliefs are threatened.

I remember reading an exchange in a Fortean journal between a critic of the Bermuda Triangle and a prominent promoter of same. The critic outlined some quite specific reasons for disbelieving anything particularly mysterious is going on in the fabled region. The proponent

responded by remarking that the critic didn't know what he was talking about because once, when the two were on a television show together, he had asked the proponent if the New Yorker were a newspaper!

Apparently this argument made sense to the proponent, but I can't imagine its making sense to anybody else. It is an extreme example of how emotional commitment to a position or to a specific claim can close us to rational argument and open us to irrational defensiveness. It can lead us -- and this, by the way, is as true of debunkers as of believers -- to feel that the truth is greater than the sum of its facts.

It is easy to say that facts are all that matter. It is not always easy, however, to act on that knowledge. This is especially true at a time when paranormal and other anomalous claims are under attack by professional debunkers who gleefully jump on any mistake proponents make (while of course refusing to acknowledge any of their own) and do their best to paint these proponents as fools who can't tell the difference between valid and invalid data. The effect is to force a proponent, if he isn't sensitive enough to know better, to assume a burden of infallibility.

Not long ago an ongoing controversy was settled when a certain item of information came to light. This new information proved that the claim in question was fallacious because it had been based on erroneous assumptions.

The controversy had gone on for several years, with debunkers on one side of the issue and a prominent proponent on the other. The proponent -- let's call him X -- and his allies skillfully refuted the debunkers' arguments, most of which were demonstrably false or irrelevant. But finally an independent researcher, Y, who had no particular stake in the controversy, discovered disconfirming data which showed that, while the debunkers' arguments were mistaken, their conclusion -- that the claim was unfounded -- was correct. The critics, predictably passing over their own errors, equally predictably chortled about their "victory" and had fun at X's expense.

X's response was to cast aspersions on Y's motives and to mount an emotional defense of the claim using post-hoc rationalizations and shaky arguments. When I talked with him about the controversy, X talked less about facts than about face -- his own in particular and all anomalists' in general -- and about the use to which the debunkers were going to put Y's information. He made it appear that the fate of all anomaly investigation rested on the preservation of the claim. To him it seemed the finding of facts had become distinctly secondary to the scoring of points, just as it always had to those debunking opponents whom he so long had criticized so eloquently.

Let's not be afraid to admit it when we're wrong. And let's not make the mistake of getting emotionally involved with -- or staking our professional reputations on -- a particular idea or a particular case. That doesn't mean that we aren't entitled to our opinions about the merits of various claims or that we should refrain from expressing these opinions and citing our reasons for holding them. It just means that we ought to understand clearly that what we believe and what is

need not necessarily bear a blood relationship.

(4) Don't hesitate to criticize.

Throughout this article I have referred to our critics the debunkers. They call themselves "skeptics," which they aren't, and I think we ought to stop calling them that, too. Marcello Truzzi defines the difference between the skeptic and the debunker as the difference between one who doubts and one who denies. In the paranormal field there is, Fort knows, plenty of room to doubt.

Unfortunately we hear too much from the deniers and too little from the doubters. We are not likely to get rational arguments from those who choose to define the controversy in apocalyptic terms. Anyone who believes, as some debunkers say they do, that civilization will collapse if too many people believe that Bigfoot exists is not likely to concern himself with such small matters as reasonable arguments. That is too bad for the rest of us because it means we have to look elsewhere for the kind of good critical review that anomaly studies urgently require.*

The true skeptics, at least those willing to put in the time to familiarize themselves with the literature, the issues and the personalities, are all too few in number. Most can be found in the pages of Truzzi's Zetetic Scholar, which I recommend highly to all serious anomalists.

But it appears that the major part of the policing of the field will have to be done by us. To our credit we have produced a surprising body of critical studies of various claims. But much, much more is needed.

The more we learn, the more we see the necessity for great care in assessing the data. Some stories hold up under the most searching scrutiny. Others, including some we hadn't expected (such as the 1897 UFO "calfnapping" and the Barbados "restless coffins"), collapse and blow away. We can be certain that more of the old favorites will meet a like fate.

I urge each of you to pick a particular case -- one that everybody "knows" to be true but that has not been documented in our time -- and follow it as far as it goes. If you are able to substantiate it, great; then we have a solid piece of evidence. If you disp it, that's great, too. Who needs a bogus mystery when we already have far more real ones than we can possibly deal with?

Let's not be afraid to criticize friends and colleagues -- or

*This is not to say, I wish to emphasize, that the debunkers are always wrong or that they have made no contribution whatever to serious research. Some of their work does withstand critical scrutiny. So, however, does some of the work of extreme believers. My point is that debunkers' and believers' claims must be approached with caution, with judgment reserved until all sides have been heard from.

even ourselves -- when they or we stray from the paths of common sense and caution. Along the way some egos will get bruised, but if those you criticize -- tactfully, I hope -- are as concerned with fact-finding as you are, they'll get over it. We all make mistakes. The only unforgivable mistake is the knowing perpetuation of error.

(5) Don't assume that all mysteries, even the genuine ones, have solutions.

Once, reflecting on his involvement with the mystery of the Loch Ness monster, Roy Mackal remarked to me that he could never understand the resistance of so many scientists to the idea of Nessie. After all, he said, Nessie is a "rather mundane sort of idea. We already have other larger freshwater animals such as the sturgeon. . . . Sometimes I think it would almost be worth the game if the phenomenon at Loch Ness were all that earthshaking. But it's not. It violates no basic law of zoology to suggest that there are large animals in the loch."

Many of us have come to assume that we are dealing with phenomena that border on the miraculous, phenomena that if understood properly would shake the scientific establishment to its very foundations. That may be so in a limited number of cases, but in the great majority of cases I think it's wiser to conclude that the various mysteries will eventually yield to solutions that are not only un-extraordinary but also uninteresting.

The late F. W. Holiday once wrote a book in which he contended that Nessie is a strange phenomenal manifestation from another realm of being. In reality, as Mackal and other zoologically-trained investigators have shown, Nessie looks and acts precisely as any large animal would under the circumstances.

We read books that would have us believe fossilized footprints prove that Homo sapiens walked the earth millions of years ago. Yet a recent scientific investigation shows that the prints are neither of great age nor of human origin. They are almost certainly camel tracks and they may be only 8000 years old. "Skyquakes," sometimes attributed to UFOs, are now being studied by Thomas Gold and Steven Soter of Cornell University. They have learned that such phenomena have a geophysical explanation. The fabled moving rocks of Racetrack Playa, California, are caused by the interaction of wind and rain.

And so on and on. We would do well to recall that before meteorites were understood they were considered so bizarre as to be utterly unbelievable. There was a time not so long ago when meteorites were Fortean phenomena.

* * *

It is high time that we get serious. And if we are going to be serious, then we are going to have to be cautious and careful. And if we are cautious and careful, we're going to look a lot more like skeptics than believers. Which is fine, and in the true Fortean spirit. Charles Fort was skeptical of establishment humbuggery and so are those of us who follow in his footsteps. That hasn't changed and I hope it never will. But now it's time that we train a skeptical eye on our own humbuggery as well.

UNCANNY PROPHECIES IN NEW ZEALAND: AN UNEXPLAINED SCIENTIFIC ANOMALY

RICHARD KAMMANN

March 10, 1982. Dunedin, New Zealand. Radio Station 4ZB.

Host: Phil Henry. Guest: Emory Royce. Time: 10.56 a.m.

HENRY: You have shown up to have some psychic abilities yourself. Do you have any predictions that you think will come true, and that you think will be as accurate as an astrologer?

ROYCE: A very senior world leader is going to come to an end. The code number I'd like to--this is a very specific one, I know who it is-- but I'm going to use just the numerological code number because I don't want to make an announcement of the person's name, it would be too catastrophic, I think, on the radio, but let's say "ten dot ten."

HENRY: "Ten dot ten." Have you got a date for this event?

ROYCE: That one looks to me like it's coming in the latter part of this year, and I get something around the eighth month. Now I should send that in a registered letter to myself to make sure that prediction is not disputed.

June 28, 1982. Dunedin, New Zealand. Radio Station 4ZB.

Host: John Jones. Guest: Emory Royce. Time: 10.35 a.m.

JONES: What about the "ten dot ten" prediction associated with a very senior world leader coming to an end, as you put it, the eighth month? Was the time when this would happen--have you managed to get any deeper on this one? Could you be more specific? You weren't quite sure whether it was the eighth calendar month or how it worked, were you?

ROYCE: It felt to me shortly after I made that prediction that it was more likely eight for October, "octo" meaning the eighth number in the Roman number system.

JONES: Now this is the one where you have sent a registered letter with that man's name inside it, is that correct?

ROYCE: That's correct, yes.

JONES: So we'll be checking back with you in October. How specific are you in this letter to yourself? You said you knew the name but you weren't prepared to reveal it. Have you revealed it in the letter?

ROYCE: The exact name is in the letter. Absolutely.

November 11, 1982. Headline in the Otago Daily Times (Dunedin):

SOVIET PRESIDENT DIES SUDDENLY

The Soviet President, Leonid I. Brezhnev has died, the official Soviet news agency Tass reported yesterday.

November 15, 1982. Dunedin, New Zealand. Radio Station 4ZB.

Host: John Jones. Guest: Emory Royce. Time: 10.14 a.m.

JONES: There was the "ten dot ten" prediction associated with a very senior world leader coming to an end and you sent yourself a registered envelope that contains the name of that world leader. You do have in your hand the letter that was sent, we have someone here to check it, from "Eyewitness" (T.V. program), we have Kevin Ramshaw. Kevin, if you'd just like to check that that is all signed, sealed and delivered, dated when it should be dated. Emory, if you could open it up and tell us what's inside.

RAMSHAW: That seems to be correct.

JONES: Kevin seems happy with that one. (pause) This is the "ten dot ten" prediction. (Envelope is slit open.)

ROYCE: It's a very short letter. Would you like me to just read it out to you?

JONES: Yes, please.

ROYCE: The letter is dated March 22, 1982, and is addressed, "To Whom It May Concern. Future prediction on the March 10, 1982 Phil Henry show. The end of a senior world leader coded 'ten dot ten.' Explanation: multiply 10 by 10, equals 100. Add up the number equivalents of the letters in the name 'Brezhnev,' equals 100. Signed, Emory Royce."

It needs only to be added that I have a copy of that letter and that it was read out correctly.

Skeptical readers who suppose that I will now produce a concise explanation for Emory Royce's prediction, correct to within ten days, of the death of Mr. Brezhnev are going to be disappointed. Even worse, this was but one of four successful prophecies, described as follows in the New Zealand "Eyewitness" TV report on the night of November 15, 1982.

EYEWITNESS: The Soviet leader's death was only one of four predictions made in March. He also forecast Dunedin would NOT get an aluminium shelter, that men would lose their lives in a naval disaster associated with military conflict, and that the government would be rocked by a mid-year scandal involving Mr. Muldoon and Works Minister Mr. Quigley.

Although I have respectable credentials as a debunker of psychic duds and hoaxes, I would also like to be sufficiently openminded to accept a true scientific anomaly if I should meet with it. The amount of correct detail in these four prophecies seems to rule out the usual explanations in terms of retrofitting to ambiguities and chance coincidence.

Saving Successes and Forgetting Failures?

Of course, psychics and astrologers often make so many predictions that some are bound to get confirmed, and these successes are saved while all the failures are forgotten. Ever since James Randi, and other skeptical writers have started keeping tabs on the psychics' New Year prophecies, a sorry record of untestable and otherwise mostly wrong predictions is the general pattern. But while this is undoubtedly true about prophecies in general, it does not apply to the present case. I have had many long discussions with Royce, members of his family, his friends, his employer and workmates. He made no other predictions that anybody recollects. As the picture emerges it was his untested personal belief that he could

foresee the future, combined with the challenge of the radio interview, that prompted him to announce these future prophecies. It is, at least, a verifiable fact that he made only five predictions on that March 10 radio program, one of which had no time boundaries on it and must be deleted as a test case. So we have a 100% success rate if we accept that the other four prophecies were fulfilled.

I do not believe that Royce is truly precognitive, but I will here present the evidence as neutrally as I can. If this case is not a valid scientific anomaly, I hope a rational explanation can be put forth in the future, if not by myself, then by someone else. Suggestions for further lines of inquiry will be gratefully received.

Could the Brezhnev Prediction Have Been Tricked?

The use of a registered letter for the Brezhnev prediction must make any psychic investigator think immediately of magicians' methods for transferring information from outside to inside an envelope. Because I was personally involved in this case at a very early stage, I was present when the registered letter was received at the Post Office and took it immediately into my possession. Without going into the details of my procedures, since I have been previously scolded for revealing too many magician's secrets, let me ask readers to accept my conclusion that the letter in the envelope contained the name "Brezhnev" when it was mailed shortly after the March radio program! Thus, there was no need for any trick since the crucial piece of information was already there.

Let us consider the remaining three predictions in more detail. All the following (and earlier) quoted material is taken from audio tapes owned by the Radio and TV stations.

The General Belgrano and the HMS Sheffield

In my view, the sinking of these two ships is the weakest case in the episode. I note that Royce did not name the ships in his original forecast.

(March 10, Radio Interview)

ROYCE: I get a man-made disaster involving some sort of structure bathed in water, or surrounded by water, poisonous gases spreading some distance, affecting life in a serious way . . . it could be associated with something nuclear and there's going to be a war scare there.

(June 28, Radio Interview)

ROYCE: That prophecy matches up with the double naval disaster in the Falklands of the sinking of the Argentine cruiser, General Belgrano and the British ship, the Sheffield. Quite clearly these were structures surrounded or bathed in water and clearly it was a man-made disaster. There were gases, smoke, heat all involved in them, and of course the threat of nuclear war was discussed immediately after that because it was known that the British did have nuclear weapons on their ships and Argentina was estimated to be only one or two years away from actually having its own nuclear weapons.

Of course, predicting a naval disaster would not be so striking if the battle of the Falkland Islands was already underway, but the prophecies were made three weeks before Argentina's surprise invasion of the islands on April 1.

Cancellation of the Smelter

(March 10, 1982)

ROYCE: I'm getting no smelter in Dunedin.

To explain the significance of this prediction, an international consortium, with the support of the New Zealand government, was then hoping to install a major aluminium factory at nearby Aramoana. The project had been broached as early as 1974, and by the early 1980s had become the centerpiece of the government's economic strategy.

There was a small but active environmentalist movement in Dunedin against the smelter, and one Otago economics professor was openly skeptical of its viability. However, the headlines in the Dunedin newspaper leading up to March 10 were consistently optimistic.

Feb 6 NEGOTIATING ON SMELTER
(Top level discussions with a third overseas company to join the project.)

Feb 25 SMELTER STILL GOING AHEAD
(A commitment from a new third partner is on the verge of being made.)

Mar 10 SMELTER GROUP STILL CONFIDENT (date of radio interview)

Mar 31 POWER PRICE ONLY PROBLEM

The first signs of a reversal did not occur until late April..

Apr 23 SMELTER POWER PRICE REJECTED

This was followed by a quiet period.

Jun 16 ALL EXCEPT PRICE (is agreed to)

And then the final collapse.

Jun 24 POWER OFFER TOO LOW
SMELTER PROJECT APPEARS DOOMED

It took four more months for the government to abandon its official optimism.

Oct 19 SMELTER UNLIKELY

Since then, the smelter has dropped out of the news and is history. We may be tempted to say that either there will or will not be (yes or no) a smelter and that Royce therefore had a 50-50 chance of being correct, but given the prevailing knowledge at the time, the smelter seemed highly probable, so its cancellation was equally improbable.

Mid-Year Scandal Involving the Prime Minister.

In New Zealand the party holding the majority of seats in Parliament elects the Prime Minister who in turn chooses the Ministers of various portfolios and who make up his Cabinet. In 1982, the National Party was in power, Robert Muldoon was the Prime Minister, and Mr. D.F. Quigley was the Minister of Works and Development.

There was a political flare-up in June of 1982 involving Messrs Muldoon and Quigley which Royce identifies as the fulfillment of the fourth prophecy involving a scandal in the government. I find this case the most difficult to assess. The word "scandal" seems too strong, with its implications of vice or corruption, but the case also hinges on how well we accept Royce's time zone (middle of the year) and the code number (14.13) as identifying marks.

(March 10, 1982)

ROYCE: I'm getting. . . some sort of scandal in the government seems likely, this to happen this year around the middle of the year and I get a code word associated with a person there, a code number rather, "fourteen dot thirteen."

On June 7, Mr. Quigley made a daring public speech saying that Mr. Muldoon's "think big" strategy for the country's economic development was not understood and not supported by the majority of New Zealanders. Although the criticism was probably overdue, Prime Minister Muldoon abruptly gave Mr. Quigley a choice between retracting the speech or going to the back benches in Parliament. On June 15 the Otago Daily Times headlined, QUIGLEY SURRENDERS CABINET POSITION. The story began, "Mr. D.J. Quigley, a leading Cabinet Minister resigned from the cabinet yesterday rather than compromise his personal standard of honesty. He made it clear he felt unable to accept an ultimatum from the Rt. Hon. R.D. Muldoon to apologize for his controversial speech on the Government's growth strategy, and had effectively been sacked."

Headlines over the next few days showed that Mr. Muldoon had lost the confidence of his own party. THINK BIG DEBATE STIFLED reported heavy criticism of Muldoon from the Young Nationals. MULDOON INVITED MINOGUE TO QUIT recounted that liberal MP M.J. Minogue, another member of the National Party, had been "expressly invited" by the Prime Minister to resign from Parliament for having independent views. OVERWHELMED BY SUPPORT was Quigley's reaction to the flood of mail coming into Parliament. SHEARER QUIET OVER REFUSAL disclosed that another Cabinet Minister had cancelled a speaking engagement for fear that his words might be held against him by the Prime Minister. DIRECTIVE DENIED was Mr. Muldoon's answer to an alleged paper telling Ministers what they could or should say about the Government's growth strategy. LEADERSHIP TEST FOR MULDOON foresaw that Mr. Muldoon might lose his leadership in a caucus of the National Party coming up a month later while 150 YOUNG NATIONALS RESIGN IN PROTEST is self explanatory. Although Mr. Muldoon eventually survived this crisis, the repercussions echoed on for another month or two before it settled down in the media.

Here is how Royce decoded the prophecy in his follow-up radio interview

on June 28.

JONES: You mentioned "fourteen dot thirteen," the code name associated with "some sort of scandal" in the government around the middle of the year.

ROYCE: . . . The "fourteen" was actually a code for the combination of the two last names involved in the political scandal--"Muldoon" which has seven letters in it and "Quigley" which has seven letters in it. I said it would be about the middle of the year; in fact, it was either on or within a day or two of June 21.

JONES: Isn't it very easy, though, to say "seven letters in Muldoon, seven letters in Quigley"? Do you have any proof?

ROYCE: . . . I think if we look at the internal evidence that you'll find it quite compelling. For example, who would you say is the third party in the Muldoon-Quigley shake-up and obviously you would have to say Minogue, and again, that's a seven letter name, so that the "fourteen" encodes the cause of the total event and the "thirteen" encodes the result or the effect. There you find that thirteen stands for the number of letters in the name "Robert Muldoon" or in the name "Prime Minister" and to me that means that this affair, involving the Minister of Trade, Mr. Quigley, will have repercussions on Mr. Muldoon's leadership for some time to come.

While Royce strains here to find extra matches (e.g., Minogue is another seven letter name), the main events do occur within two weeks of the middle of the year, and the match between the number 13 and the two ways of identifying the Prime Minister seems commendable.

FINAL COMMENTS

One of the special qualities of the Zetetic Scholar and CSAR, I feel, is the effort to look at both sides of the paranormal debate, and to recognize scientific anomalies that merit further study.

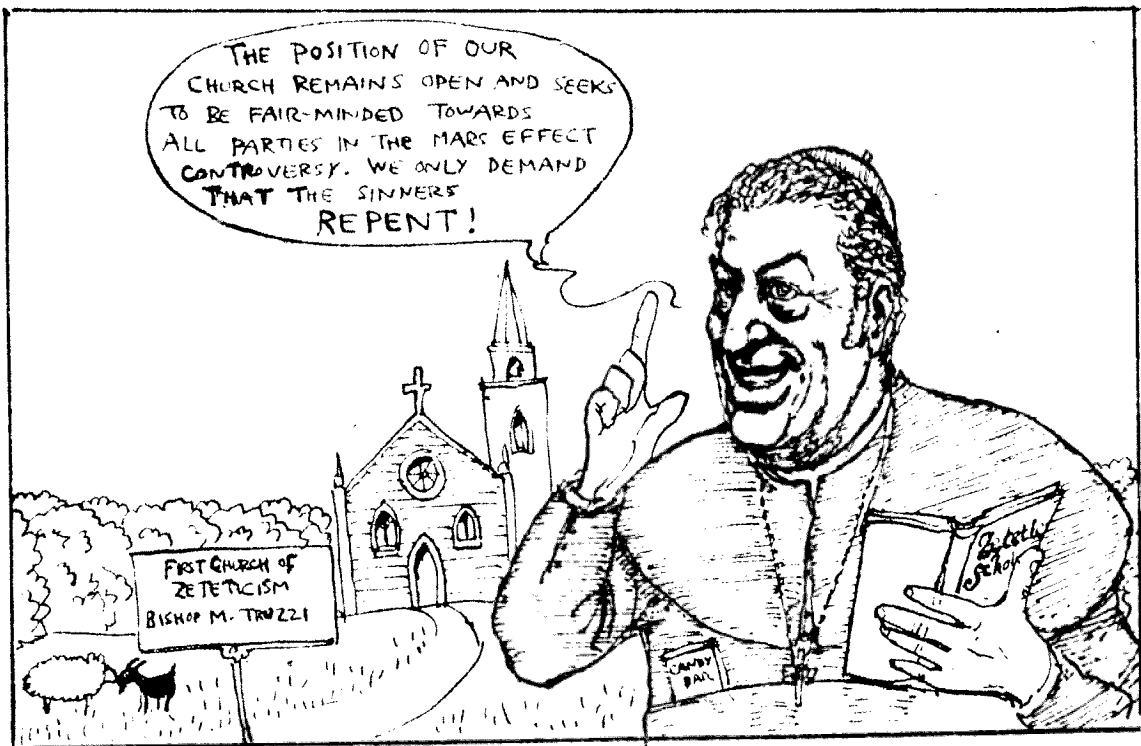
I was involved in this case at a very early stage and expected the predictions to receive no more than a weak chance level of confirmation. I was soon intrigued to read about the burning and sinking of the two ships in the Falkland Islands. This mood turned to high surprise, however, when the Dunedin smelter was abruptly cancelled, and the Quigley fiasco broke out in June, after which Mr. Muldoon's rating in the polls has never been reliably ahead of his rivals.

Knowing, however, that the name "Brezhnev" was in the registered letter, I had at least one fairly tight prediction that would surely, come October, undo the impression created by the first three, but to my complete amazement, the Soviet leader died within ten days of the designated time zone.

It is noteworthy, I think, that these four predictions are not selected out of a much larger list of failed prophecies, but are the total set of testable predictions involved. It is regrettably impossible to calculate an exact significance level or "chance probability" of four events occurring with a goodness-of-fit equal to, or better than, these four actual events, but a subjective estimate places that probability well beyond the .0001

level, and some might go much further. This judgment is bolstered by the widespread attention these predictions garnered on New Zealand radio and TV, not to mention overseas accounts, for example, in the Dutch newspapers. Emory Royce agreed to offer a new set of public predictions at the opening of 1983, but a series of schedule conflicts between Royce and the 4ZB radio team prevented this from taking place. By April, 1983 the mystic decided to wait until next year. Until this opportunity for a replication trial comes up, I shall continue to reflect on a possible rational explanation of this alleged case of precognition. The whole thing is just preposterous!

THE MARS EFFECT CONTROVERSY, I:



[M. Truzzi]

-- Piet Hein Hoebens

MORE ON THE MARS EFFECT CONTROVERSY

CURRY ON CSICOP'S "REAPPRAISAL" RE THE MARS EFFECT

I am writing about a point related to my article in ZS 9, "Research on the Mars Effect." Your readers will be aware that none of the principals in the CSICOP replied to or commented on my article, despite the fairly serious and documented charges therein. The main reason -- apart from irrelevant and ad hominem allegations that "Curry is an astrologer, " which don't deserve a reply -- that has been privately circulated is that I supposedly failed to consult with CSICOP before or during writing.

I should therefore like your readers to know that I wrote to George Abell on March 6, 1981, saying (in part)

I am preparing, for submission to the Zetetic Scholar, a report on the CSICOP vs. Gauquelin's conflicting claims re the U.S. "Mars effect" replication. The material I have so far is from the Skeptical Inquirer, plus Gauquelin's (unanswered) letters to Kurtz over the past eight months, plus a couple of short statements by Rawlins.

Is there anything you and/or the Committee would like to add or bring me up to date on?

I would like to point out that this invitation was plainly extended not just to Abell, but to the CSICOP.

Abell replied on March 14, and recommended that I write to Paul Kurtz directly. I did not do so, considering that my initial letter had been plain enough on that point. This may have been a mistake, especially considering what has been made of this omission by others. In any event, Kurtz wrote to me (enclosing considerable material) on July 2, saying that Abell had passed along my letter. I replied on July 16, and Kurtz wrote again on July 27.

Furthermore, copies of a draft of my article, accompanied by an invitation to respond or comment, went out to (among other CSICOP members) Abell, Kurtz, Marvin Zelen, Ray Hyman, and Ken Frazier. Despite a further invitation a month later (Sept. 15 and Nov. 16 respectively), nothing was received, and the article went ahead unchanged, published in January 1983. If there had been any serious errors in my text pointed out by Kurtz or anyone else, I would have appreciated beforehand being informed; I would have gladly corrected them, or at the least acknowledged them in my replies to commentators. But in any event, neither corrections nor comments were forthcoming. It seems therefore fair to conclude that my text remains substantially correct. (In retrospect, I would have liked to include a fuller account of Rawlins' involvement; but that was not the principal purpose of the document.) It also seems fair to comment that stonewalling is a legitimate military tactic, and a common political one; but it hardly amounts to good science.

Abell, Kurtz and Zelen have recently published a "reappraisal" of the Mars effect experiments, in which they admit many of their errors [The Skeptical Inquirer, 7: 3, Spring 1983]. The question which readers must decide for themselves is, does it go far enough?

-- PATRICK CURRY
London, England

ANTONY FLEW ON THE "MARS EFFECT" CONTROVERSY

As an original, albeit necessarily remote and inactive Fellow of the CSICOP, who is now due to be spending several months of the present and the next five years on this side of the Atlantic, I believe it is time for me to say that I can no longer resist the conviction that CSICOP has made a dreadful mess of its dealings with the gauquelins. That this now appears so clearly to be the case is made all the more lamentable by the fact that CSICOP has done and continues to do so many excellent and enormously necessary things. For, until and unless this dreadful mess can be satisfactorily disposed of, it is bound to get in the way of the doing of these vital jobs.

Again accentuating the positive, two points made by commentators on Patrick Curry's contribution to Zetetic Scholar No. 9 need to be underlined, and perhaps developed. The first is made by Piet Hein Hoebens. He suggests that "the authors of the KZA may initially have taken it for granted that a sceptical investigation of any 'paranormal' claim would automatically result in a swift and unambiguous confirmation of sceptical predictions. When the 'Mars Effect' failed to oblige, they were taken by surprise and had to improve a strategy to protect scepticism from premature 'falsification'" (p. 70). Certainly it is in any particular case overwhelmingly likely that sincere and competent investigation will collapse the pretensions of the paranormal; that is, after all, what has been found to happen on almost all previous occasions. Yet what both sceptical inquirers and zetetic scholars are in business to ensure must be: not that every paranormal claim is shown to be without foundation; but that these claims are sincerely and competently investigated--and let the chips fall as they will.

The second point is a much less clearly formulated hint. H. Krips suggests that the Gauquelin's theory is scarcely a theory at all: "A particular lack in the Gauquelin's theory is the absence of a satisfactory mechanism to explain the 'Mars Effect' and other correlations which they have observed" (pp. 64-4). Surely the near impossibility of thinking up any mechanism which might be operating to bring about the 'Mars Effect' should be seen as a reason for hesitating before awarding to such statistically significant correlations that diploma label? It is an occasion to remind ourselves that statistical significance at no matter what level never entails the significance of any causal connection: it is, however, importantly, an index only of the possibly quite enormous unlikelihood of the observed correlation being no more than a statistical freak.

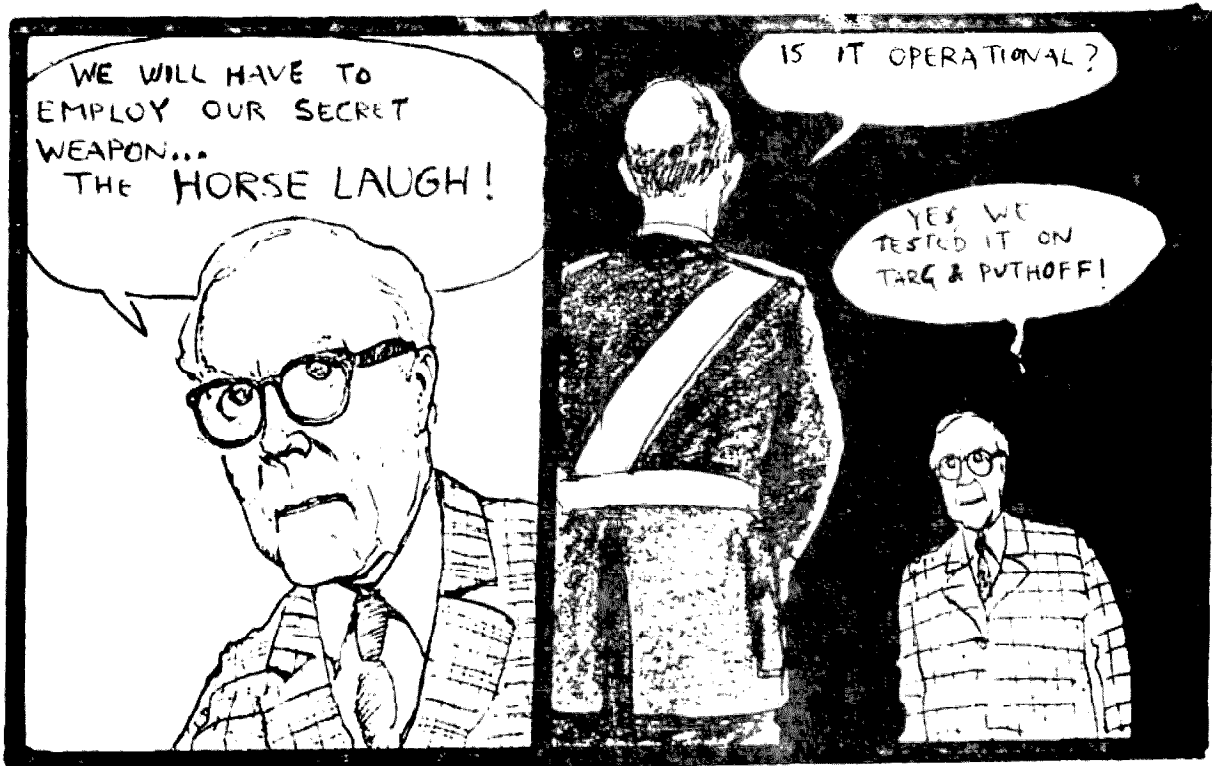
[Having received advance copies of contributions to Zetetic Scholar No. 10 by Marcello Truzzi and Richard Kammann, I was delighted to see that they both take up both the points which I pick out, above, albeit without bringing out the parapsychological connection.]

This is something which we have had to remember when confronted with often formidably impressive evidence for the occurrence of psi-gamma (ESP) correlations, and most especially when these occur under "precognitive" conditions. All ordinary means of information acquisition are, if it genuinely is any sort of case of psi-gamma, ruled out by definition; and, even when such correlations occur under "non-precognitive" conditions, no one can think of any unordinary mechanisms which could bring about information transfers. But in the special

"precognitive" case, all causation, and not just all ordinary means of information acquisition, is in fact, whether implicitly or explicitly, ruled out by definition. For genuine "precognitive" correlations must not, by explicit definition, be brought about: either by some common earlier cause of both the "anticipations" and the "fulfilments"; or by the "anticipations" somehow producing those "fulfilments." Whereas, to suggest that the "fulfilments" themselves cause the "anticipations" is simply incoherent. To do that they would have to be able: both to make things which had already happened not to have happened; and to make things which had not happened to have happened. And if that is not self-contradictory, incoherent, and absurd, then I do not know what would be. For a first spelling out of this approach, with its implication that there never will be any regularly repeatable psi-gamma effects, see my "Parapsychology: Science or Pseudo-Science." This is in both *Pacific Philosophical Quarterly*, Vol. LXI (1980); and M.P. Hanen, M.J. Osler, and R.G. Weyant (Eds.) *Science, Pseudo-Science, and Society* (Waterloo, Ontario: Wilfrid Laurier UP, 1980); and P. Grim (Ed.) *Philosophy of Science and the Occult* (Albany, NY: Suny Press, 1982).

-- Antony Flew
Downsview, Ontario

THE MARS EFFECT CONTROVERSY, II:



[M. Gardner & P. Kurtz]

-- Piet Hein Hoebens

SOME FURTHER REFLECTIONS ON THE MARS EFFECT AFFAIR

PIET HEIN HOEBENS

In spite of several appeals for a truce, the controversy over the so-called Mars Effect shows few signs of abating. The five part Mars Effect section in ZS#10 has raised a number of important questions. The Editor has specifically invited persons associated with CSICOP to share their views with the readers of this journal. These comments are strictly *a titre personnel*.

Ad McConnell & Clark

Although I respect Professor McConnell and continue to believe that his intentions were honourable, I strongly object to the manner he has chosen to intervene in the controversy. His September 1981 letter "to all public supporters" of CSICOP can only be seen as a regrettable lapse. Apparently it has not occurred to him that his inquisitorial approach could have led to an "experimenter effect" largely invalidating his conclusions. McConnell believes that the table printed in ZS#10 reflects CSICOP reactions to STARBABY. For all I know the table may just reflect CSICOP reactions to abusive letters. The December 1981 follow-up only added insult to injury. To make matters even worse, McConnell may have helped to prevent a satisfactory solution to the problem. The controversy over M. Gauquelin's findings is an extremely complex affair. Many of the "public supporters" in 1981 did not have the remotest idea what all the fuss was about. When the first rumours of a "scandal" reached them, they had to decide--necessarily on the basis of an intuitive assessment--whether a detailed examination of the claims, counter-claims, counter-counter-claims, etcetera, would be worth their trouble. I cannot really blame those who, after having pursued McConnell's *J'Accuse*, concluded that the matter was not sufficiently serious to warrant their attention. The valid points which McConnell undoubtedly had made were completely obscured by his intemperate rhetoric. At my newspaper we stick to a tacit rule: Letters purporting to reveal the "worst scandal in history" (we receive about ten every day) have a 10,000 to one chance of belonging in the crank mail category.

Ad "The True Disbelievers"

De gustibus non est disputandum. I was puzzled when I learned that some of my friends in CSICOP find the style of Professor Kammann's paper objectionable, inflammatory and undignified. I beg leave to express my dissent. I regard "The True Disbelievers" as an eminently fair, highly readable and--given the circumstances--remarkably restrained statement from a distinguished skeptic who has gone to almost incredible lengths in his attempts to help CSICOP free itself from its Martian predicament. It is true that Kammann's verdict is hardly flattering to several prominent members of the committee, but that verdict was reached after an extensive and scrupulous examination of the evidence. I do not think that Kammann has been excessively censorious. To the contrary: he has made a

great effort to make the facts fit his "innocent mistakes" scenario. It is not his fault that the facts refused to co-operate. Even so, Kammann does not indulge in cheap moralizing at the expense of Professors Kurtz, Abell and Zelen. Rather, he portrays them as the victims of their inability to detect the pitfalls of rationalist irrationality.

Ad CSICOP

To some it may appear somewhat incongruous that the above paragraph was written by someone who, despite Professor McConnell's exhortations, remains a "public supporter" of CSICOP.

I confess to having mixed feelings about the Committee. I agree with Kammann, McConnell, Curry, Rawlins, Eysenck and Truzzi that CSICOP has made quite a mess of its dealings with that remarkable and courageous scientist, Michel Gauquelin. However, I do NOT think that CSICOP is beyond redemption; I do NOT think that the Mars Effect debacle was "the biggest scandal in the history of rationalism", and I do NOT think that this affair is symptomatic of everything that is going on inside the Committee.

According to some of its more outspoken detractors, (and here, I am not referring to Truzzi and Kammann, who some supporters of CSICOP have falsely cast in the role of "enemies"), CSICOP has cynically and systematically disregarded the lofty principles proclaimed on the back-side cover of each issue of THE SKEPTICAL INQUIRER. Having had access to many of the background documents, I have gained a somewhat different impression.

The more disturbing instances of skeptical misbehaviour have been adequately exposed and analysed. We should not ignore, however, those instances where CSICOP behaved far more creditably than participants in other scientific disputes have often done in comparable circumstances.

What first comes to mind is the comparatively respectful manner the Committee has treated the principal victim. In all fairness it cannot be maintained that Profs. Abell, Kurtz and Zelen have been guilty of a systematic campaign to discredit and vilify Gauquelin. Gauquelin was given the opportunity to argue his case in the skeptical periodicals, and the replies, while often unsatisfactory or even misleading, have been generally courteous. CSICOP and THE SKEPTICAL INQUIRER have been fairly consistent in presenting Gauquelin's work as sufficiently challenging to warrant serious investigation.

My second point concerns the way CSICOP has responded to internal and external criticisms. In general, this response has been tragically inadequate. Having been a direct witness to one of the crucial incidents in STARBABY, I am less than satisfied with the Committee's version of the events that led to Dennis Rawlins' excommunication. Even so, the facts do not really fit the "worst scandal" theory, according to which the CSICOP leadership, in a determined attempt to cover up the unwelcome truth, engaged

in a ruthless campaign to suppress internal dissent. I mention my own experiences only as an example. Since the Autumn of 1981, I have repeatedly, both privately and publicly, expressed my misgivings about the way the Committee has handled the affair. The CSICOP leadership was well aware of my friendly contacts with both Rawlins and Gauquelin. Apart from some extremely odd communications from a well-known skeptic whom charity forbids me to name here, the response to my insubordinate queries has been remarkably courteous and rational. There was no noticeable pressure on me to conform to any party line, not even after I had made plain that I continued to find Dennis Rawlins' criticisms more convincing than the purported refutations. My dissent was treated as entirely legitimate. Those who have read, for example, the correspondence between a one time chairman of the German Society Against Superstition and the eminent skeptic Carl Count von Klinckowstroem (who committed high treason by accepting some claims of dowsing) will perhaps understand why my verdict on CSICOP is comparatively mild.

Finally, there are the measures CSICOP has taken, publicly to correct at least some of the past mistakes. Kendrick Frazier's decision to publish Rawlins' merciless "Remus Extremus" in THE SKEPTICAL INQUIRER was an act of courage. As for the "Re-appraisal" Profs. Abell, Kurtz and Zelen have published in the Spring 1983 issue of the journal: the least that can be said of this remarkable document is that it demonstrates that the CSICOP leadership is not entirely unresponsive to criticisms. Too little and too late? Maybe - but much more than many of us would have expected.

My generally skeptical view of human nature does not permit me to see the Mars Effect affair as merely a series of innocent errors. It is rather obvious, I should say, that at several points considerations of political expedience have prevailed over the demands of intellectual integrity. This is usually the case where a group of fallible human beings becomes involved in a protracted controversy.

Some critics have insisted that the Mars Effect fiasco is symptomatic of the way CSICOP deals with the anomalous claims it professes to "examine objectively and carefully" and that it has showed the Committee for what it is: a pseudo-rationalist pressure group, obsessed with discrediting - if needs be by hook and by crook - any scientific finding that offends orthodox sensibilities. While I agree that the Committee frequently fails to practice what it preaches (The "clear and present danger" Professor Truzzi saw in 1976 is no less clear and present today), I am not a little suspicious of the motives of some of its most vehement enemies. Compared to some of the published attacks on the Committee which I have seen, even Mr. Klass' CRYBABY seems a model of dispassionate scholarship.

A MODEST PROPOSAL

I wish to conclude with a somewhat quixotic suggestion. The Mars Effect affair has raised questions about CSICOP's credibility. The Committee, on its part, has protested its bona fides--and has publicly corrected at least some of the major mistakes. Doubts about CSICOP's

ulterior intentions, however, will linger on. In my view, the most felicitous thing CSICOP could do to clear its name once and for all would be to become re-involved in the scientific debate over the claimed planetary effects and to propose to Michel Gauquelin (who has taken an admirably sober view of the entire business), Richard Kammann, Dennis Rawlins and others that they all join forces in a new test of cosmobiology. I suggest that, instead of the Mars Effect for sports champions, a different effect be chosen this time. I think it would be worth the trouble. CSICOP would have a chance to prove that the Mars fiasco has indeed been an isolated lapse. The advantages for Gauquelin are obvious. Finally, all of us would profit, for such a test would bring us closer to the answer to the only question that really matters: Do planetary effects exist, and, if so, how can they be explained? After all, that is what controversy was about in the first place.

THE MARS EFFECT CONTROVERSY, III:



[R. Kammann & P. Kurtz]

-- Piet Hein Hoebens

THE MARS EFFECT AND ITS EVALUATION

HANS J. EYSENCK

Truzzi (1982), in his interesting "Personal Reflections on the Mars Effect Controversy," raises a number of questions which are independent of his dismissal of many of the criticisms made by CSICOP. It would be difficult to disagree with Truzzi on these points, and we may regard these criticisms as unfounded, and as being presented in a manner which is not in the best tradition of scientific discourse. However, there are certain points in Truzzi's article which I find unconvincing, and it is the purpose of this paper to present an argument concerning the proper evaluation of the Mars Effect. The first, and most important point is that it is scientifically and logically impermissible to discuss the Mars Effect in isolation from all the other studies done by the Gauquelins on the "Saturn Effect," the "Jupiter Effect," the "Venus Effect," and the "Moon Effect"! If the Mars Effect were the only relation between excellence in a particular type of occupation and planetary position that had been found, one would regard it from quite a different perspective to that enjoined on us by the fact that it is one of a number of equally strong effects, relating to many different professions, and involving several different planets. The fact that there are several different planetary effects relating to excellence in several different professions means that the Mars Effect is not isolated, but is supported by a large body of related data which must be taken into account in evaluating both its occurrence and its meaning within the scientific context of modern astronomy.

We must, I think, go further than that and also consider the other evidence brought forward by the Gauquelins in relation to planetary effects, such as the fact that parents and children show similar planetary positions at birth, that these are additive, etc. Clearly planetary effects (Truzzi prefers to talk about "correlations," but we shall argue that this is merely evading the issue), if they can be shown to involve many varied and different phenomena, are clearly much more important, relevant and securely established, than if they only concern one single aspect of life, namely excellence in given professions.

Even more important is the demonstration by the Gauquelins and S.B.G. Eysenck of the relationship between personality and planetary positions (Gauquelin et al, 1979, 1981.) Here we have the verification of an hypothesis, not originally considered by the Gauquelins in their collection of data, but very strongly borne out when a suitable analysis was done on these data. This again extends the circle of evidence, and draws into it variables not previously considered. This inevitably strengthens the evidential value of the evidence for the Mars Effect to a very considerable extent.

Last but not least, we have the important contribution by Françoise Gauquelin (1982) in her book "Psychology of the Planets," in which she relates directly planetary positions to personality variables in a manner quite different to that adopted by Gauquelin et al. (1979, 1981). This study is particularly relevant to the claim made by Truzzi that the Mars Effect really has nothing to do with astrology, and that it is merely accidental that it was drawn into this circle through Gauquelin's need to

find some allies. Francoise Gauquelin's book makes it clear (a) that she is bitterly opposed to astrology, in all its forms, but that (b) she has found direct evidence for the accuracy of astrological predictions in her work on the relationship between personality and planetary position at birth.

Similarly, the particular planets involved in the Gauquelin's original research which gave rise to the Mars Effect, the Jupiter Effect, the Saturn Effect, the Venus Effect, etc. provided links which were predictable on the basis of the astrological symbolism involving these planets. If these effects are real, then it would not be possible, I feel, to separate these findings from astrological predictions.

We must now turn to two somewhat related claims made by Truzzi. The first one is that the very name "Mars Effect" is a misnomer. As he says, "A 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." Truzzi contrasts correlational and causal interpretations, but this is philosophically a very difficult thing to do. Ever since Hume and his criticism of causality, we have known that strictly speaking we cannot talk about "causality" in a fundamental sense; all causality is based on correlation, and fundamentally there is nothing more in causality than correlation. The closer a particular correlation, usually under very carefully controlled laboratory conditions, approaches unity, the more likely are we to speak of "causality," but strictly speaking this is incorrect and should be avoided.

We tend to talk about "causation," even in the absence of perfect correlation, when we have succeeded in embedding a phenomenon in a nomological network of theories, laws, interpretations, etc. Here, it is true, the nomological network by the Gauquelins is minimal, but as pointed out above there is such a network embracing a number of different phenomena, and they all hang together in a predictable manner suggested by astrological theory. This is annoying to those of us who have hitherto completely discounted all astrological pretensions, including the Gauquelins, but I don't see how logically we can escape from this conclusion.

Truzzi goes on to say that: "It is fundamental that a correlation may be valid while due to any number of third factors; Gauquelin has merely demonstrated (at best) the existence of the mars correlation (rather than effect)." The correlation may indeed be valid while due to any number of third factors, but so can what is interpreted as a causal effect! The astronomical red shift is usually interpreted as caused by the rapid expansion of the universe, but some astronomers argue that it is in fact caused by a number of third factors of quite a different kind. Thus this argument does not really discriminate between correlational and causal interpretation of the Mars Effect; both could be in error because of the presence of a third factor.

We next come to the second claim made by Truzzi, namely that "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 you prematurely leap to the conclusion that its validity demonstrates a causal connection supportive of astrology." I find it very difficult to accept that the Mars Effect, even seen simply as a correlation, is not extraordinary, particularly when taken in the context of the other phenomena discovered by the Gauquelins mentioned above. Here we have a whole series of observations which are completely unpredicted by any branch of modern science, which defy any kind of interpretation using the canons of modern science, and which are strong enough to be not only capable of being observed under controlled conditions, but of being replicated. It seems to me that they present more of an anomaly to modern science that did the precession of the perihelion of mercury to Newtonian gravitational theory; the "Mercury Effect," in spite of its minuteness, would not go away, although many third factors were suggested in order to explain it away, such as the hypothesis of an unobserved inner planet ("Vulcan"), (Roseveare, 1982). Truzzi's argument resembles dangerously that of the unmarried young lady who pleaded with her parents, as an excuse for her illegitimate baby, that it was only very tiny! Here, I think, the rationalists and astronomers who have attacked the Gauquelins have shown a better sense of the importance of the demonstration of the existence of the Mars Effect, and the other effects discovered by the Gauquelins. If these are real, then we certainly have a very real problem of explanation on our hands, and indeed this may lead to a Kuhnian revolution in science, just as did the existence of the precession of the perihelion of mercury! I believe that the effect cannot be argued away, that it is real, and that we should take much more seriously than has been done hitherto the task of formulation and testing theories to explain along causal lines the phenomena discovered by the Gauquelins.

Clearly these views have some relevance to decisions about future research in this field. If, as I suggest, we already have the beginnings of a nomological network, then clearly research should be directed at an extension of this network, and furthermore, it seems vital that research into one corner of this network should always be conducted in the light of knowledge obtained at other corners. Thus it seems obvious that research is most urgently needed into the relationship between planetary position at birth and personality in normal persons, i.e. individuals not falling into the category of famous sportsmen, famous scientists, etc. However, such research would clearly have to bear in mind another finding of the Gauquelins, namely that the relationship between planetary position at birth of parents and children only obtained when the birth of the child was natural; it is completely disrupted when the birth is induced. This finding can be taken into account along two different lines. If we are most interested in investigating the relationship between planetary position and temperament, then we would concentrate on obtaining subjects whose birth was natural. If we are interested in applying the traditional multi-trait/multi-method analysis to the field, then we would also study subjects whose birth was induced, to act as a kind of control group where the effect predicted for subjects with a normal birth would now be expected to be absent. This is a prediction which follows from the part of the nomological network containing data about congruence of planetary effects for parents and children, and would thus strengthen that part of the network.

It is clear that in the past, and particularly in the work undertaken by Kurtz and the CSICOP, there has been a failure to take seriously results of previous research. Thus in their study of the Mars Effect, having found that top ranking sportsmen did in fact show the Mars Effect, they added a number of top ranking basket-ball players in spite of Gauquelin's earlier findings that these did not show the Mars Effect. The inclusion of basket-ball players had the desired effect of reducing the size of the Mars Effect and its statistical significance, and this was the aspect of the investigation emphasised by CSICOP. From the point of view of the nomological network, however, one would be tempted to interpret their findings as a replication of Gauquelin's work, in that both he and they found that basket-ball players do not show the Mars Effect, while other types of sportsmen do. This would lead one to ask questions about differences between individual sports and team sports generally, and perhaps construct other hypotheses of a testable kind which would extend the nomological network. Altogether, as Eysenck *et al.* (1982) have shown in their work on "Sport and Personality," there are marked differences between outstanding sportsmen in different fields, and even in the same field. Thus runners excelling in short distance events have different personalities and body build from runners excelling in long distance events, with the former being more extraverted, the latter being more introverted. In the same way shooters differ in personality from each other, depending on whether the target is exposed for a long period of time, when introverts do better, or whether explosive and sudden action is called for because the target is only exposed suddenly and for a short period of time, when extraverts do better. Thus in planning future research an intimate collaboration between psychologists and cosmobiologists seems to be called for.

Altogether, it would seem that future research should be planned in collaboration between those who have been most critical of the work of the Gauquelins in the past and those hold a more favourable attitude. Research plans should be devised in such a way that both sides would be satisfied, and rigid rules of procedure for selection of subjects, analysis of data, etc., should be laid down beforehand, so that interpretation would not be subject to debate afterwards. The Gauquelins have undoubtedly succeeded in setting up what Kuhn would call a paradigm in this field, and the extension of this paradigm now requires problem solving of the type familiar to all scientists. After the betrayal of the most fundamental rules of collaboration, integrity and even politeness by Kurtz, Abell and other members of the CSICOP, it will undoubtedly be very difficult to engage in such collaboration in the future, but it does seem sad that grown up intelligent men should not be able to get together and participate in the solution of a problem which appears purely intellectual, and devoid of emotional content.

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MARCELLO TRUZZI REPLIES:

I am very sympathetic to much Professor Eysenck says. The case for neo-astrological causalities being present in the Gauquelins' work is greatly strengthened by consideration of the total corpus of their researches. And this context increases the scientific importance and priority their work should be accorded while also adding to the over-all extraordinariness of their anomalies. But my "Reflections" paper was intended as an examination of the CSICOP approach to the Mars Effect claim--a single claim to which CSICOP had limited its attention. My comments were made in light of that limitation in their work. Given such an atomistic look at the Gauquelins' work by CSICOP, I think my criticism stands. Given their question, they came up with the wrong answer and approach to it. This does not keep me from agreeing with Eysenck that CSICOP should have asked many other questions than they did. My central point remains: Critics of an anomaly should try to minimize the revolutionary or extraordinary character of the anomaly; that is, they should attack it in its most conservative rather than most radical form. Proponents of an anomaly will naturally seek to present the most anomalous portrait of their anomaly to get attention and importance for their anomaly; but that is not the approach critics should take to it.

Eysenck also raises the problem of when one is to call a correlation a cause anyway. He is correct in noting that the simple idea of causality no longer exists in modern science as it once did, and that there is a degree of relativity to the use of the term cause. But I probably would go even further than Eysenck on this matter. Astronomers, particularly, like to criticise neo-astrological claims by pointing out the absence of any known mechanisms between the planets and the earth that might produce the results those like Gauquelin claim are present. Thus, they are arguing against the idea of action-at-a-distance, ignoring the fact that this same argument was raised against Newton's proposal of gravitation. They overlook the systems approach now common within science. If B.F. Skinner can place a black box between stimulus and response, why can not a neo-astrologer place a black box between mars and the earth? Whether or not there is something (ignorable anyway) inside the black boxes is not relevant. Critics of neo-astrology seem to want mechanisms because they implicitly demand unification within science. But that is an empirical issue. It is theoretically possible that the nomological network of the neo-astrologers will form an explanatory and predictive system in a way quite unintegrated with the rest of science, at least initially if not ultimately.

On the other hand, the Gauquelin and Eysenck work--though presenting, I think, real and important anomalies-- still represents an extraordinary set of claims for which commensurate proof has not yet been obtained. The work is important and should be encouraged, but we need independent replications and the elimination of more "normal" alternative explanations before neo-astrology can gain scientific acceptance. And that is as it should be. True or false, the answer lies in continued investigation and more studies.

MORE ON DEFINING A "UFO"

MICHAEL MARTIN

Dr. J. Allen Hynek in "Defining the UFO: Semantics on the Rampage" (ZS #11) and one letter by Jenny Randles and another by Hilary Evans (ZS # 10) comment on my paper "Defining UFO" (ZS # 9). I will first comment on Hynek's paper and then on the two letters.

(A) Hynek first seems to raise an oblique criticism of my paper. He says that I "exhibit skill as a semanticist and perhaps as a Scholastic." He goes on later to say (without referring to me), "let us avoid splitting hairs to the point where any definition will resemble more a medieval disquisition than a pragmatic working definition." Can one plausibly infer that Hynek is suggesting that my definition is like a scholastic disquisition?

If so, this sort of innuendo is not worthy of Dr. Hynek. In my paper several serious problems were raised about Hynek's definition. He attempts to answer none of my criticisms and apparently tries to write them off as hair splitting. My own definition seems to be branded as "scholastic" despite the fact that it solves the problems of Hynek's definition and introduces structure and clarity into a murky area. Further, Hynek mentions no explicit problems with my definition. One would have thought that Dr. Hynek would be pleased that his definition had been clarified and improved upon.

Hynek also says that I never really face the basic problem: How can one define something that is admittedly unidentified from the start? But, of course, I do face the problem and make progress in solving it. One of the major points of my paper is that being unidentified is a relative notion. Something is unidentified relative to some classification scheme and not unidentified relative to others. The key problem is to say what classification scheme one is assuming in talking about UFO's. I attempt to specify this scheme in my paper.

What has Hynek learned from my critique of his definition? One gathers very little. At the end of his paper he says:

"For myself, I find it useful to think of the UFO phenomenon as that defined by the continuous flow, from many parts of the world, of reports of objects and/or sources of luminosity, perceived in the atmosphere or on the ground, whose origins and behavior remain unidentified even after competent study."

Outside of the problem that it is unclear what classification scheme is being assumed, the disjunctive clause, "in the atmosphere or on the ground," allows that abominable snowmen and other such creatures be unidentified flying objects. I raised a similar problem about Hynek's earlier definition. I am sure my present criticism will also be labelled as "hair splitting."

(B) Jenny Randles makes the following critical points in her letter:

- (1) She doubts whether an adequate definition of UFO can be given. (By implication her comments suggest that my definition is inadequate. See below.)
- (2) She argues that giving an adequate definition of a UFO does not matter in any case since it does not get us any farther in knowing what UFO's are.
- (3) She brings up a case of a mysterious luminous mass and argues that this phenomenon is in important respects like some UFO's. Her point seems to be that any rigid definition of UFO (like mine?) would exclude cases like this and prevent fruitful comparison.
- (4) She argues that on my view only UFO's that remain unidentified are of scientific interest and this is mistaken.
- (5) She proposes a working definition of UFO of her own (a 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). She invites my comments on this definition.

I will comment on these points in turn.

- (1) Perhaps no definition of UFO is adequate. But in order to show that my definition is inadequate Randles must offer telling criticisms of it. She has not. (See below.)
- (2) Of course, a definition of UFO does not get us any closer to knowing what UFO's are if this means identifying what UFO's are. Only empirical investigation can do this. But an adequate definition will clarify our concepts and improve our thinking about UFO's.
- (3) The case of the luminous mass does not seem to me to be a UFO. It is not, on my definition, since according to the report it was not a flying object; nor should it be a UFO on her definition since, according to the report, there is no evidence that it originates from the atmosphere or beyond. But this does not mean that it might not have important similarities to some UFO's. Giving a definition and making fruitful comparisons with things that fall within the definition and things that fall without are certainly compatible activities.
- (4) I do not assume that only objects that remain unidentified are of scientific interest although some words in my article may have suggested this. Indeed, objects that are identified may provide important clues about objects that are not identified. This is certainly compatible with my definition.
- (5) As far as her own definition is concerned I have two basic comments:
 - (a) As I pointed out in my paper anything is identifiable relative to some classification scheme or other. What we call "UFO" is not identifiable relative to a particular scheme. Randles' definition neglects this.
 - (b) She does not allow for the point stressed by Hynek and over-looked by the Condon Report definition that something should be considered a UFO only after it has failed to be identified by experts.

(C) In Hilary Evans' letter, Evans suggests that my definition is not one a working ufologist would feel comfortable in using and suggests a more practical formulation. By "practical formulation" I take it Evans means one that is less complex and formal. Evans suggests the following: "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, in either its nature, origin or purpose, with any known object."

This simplification would be welcome if it did capture the original idea. But it does not. First, on my definition something may be a UFO relative to one group and not relative to another. But Evans' phrase "neither he nor anyone else" indicates that this relativity is not captured by Evans' formulation. Second, the phrase "either its nature, origin or purpose" suggests that if either the object's nature or origin or purpose was known, the object would not be a UFO. But this is not obviously true. For example, if we knew that certain objects were originating from Jupiter but did not know their purpose or nature, I think that the UFO label would still be appropriate. Furthermore, as Hynek points out, this definition would rule out all cases of UFO's that are not reported.

Simplification is fine, but it often leads to inaccuracies.

REPLY BY J. ALLEN HYNEK

Dr. Marcello Truzzi, editor of the Zetetic Scholar, asked me some time ago to comment on Michael Martin's article in that magazine [#9], "Defining UFO." Martin spent some 2500 words wrestling with this problem. Jenny Randles and Hilary Evans have already published their comments, both very much to the point, in Issue # 10 of the Zetetic Scholar. Since the matter of defining the term UFO may well be of interest to our readers also, I would like to present my comments to them as well as to the readers of the Zetetic Scholar.

Martin exhibits skill as a semanticist and perhaps as a Scholastic as well, but it seems to me that he never faces squarely the basic problem: How can one define something that is admittedly "Unidentified" from the start; is this really possible in a realistic sense?

The definition of something is very much a function of what is already known about it. Take "star" for example: a definition can range from "a twinkling point of light on the vault of heaven" or "luminous source of light on the night sky" to "a celestial body whose self-luminosity is produced by nuclear fusion processes." The first definition was appropriate (and still is for poets and lovers) before we knew much about the physical nature of a star, while the latter might not be very satisfactory for all purposes, especially for someone who may never have seen a star. We could, of course, try "A star is a celestial object whose surface temperature is in the range from approximately 2000° K to 25,000° K," or, "A celestial object which is similar to the sun," or once again, "A celestial object which results from the gravitational contraction of

a large mass of gas and cosmic dust, becoming self-luminous when its interior temperature and pressure becomes sufficient to initiate nuclear reactions." And those are definitions for something we know about?

Pity one who sets about to define UFO! I know; I tried two definitions in The UFO Experience, perhaps sufficient for the purposes thereof, but a dozen others would have been possible. However, perhaps we do need to adopt something so that we are not talking completely at cross purposes, but let us avoid splitting hairs to the point where any definition will resemble more a medieval theological disquisition than a pragmatic working definition.

One thing seems to have been accepted by all those who have attempted serious definitions: a working definition must not be based on an assumed origin of the UFO phenomenon. That could be like defining stars as "lights placed in the sky by angels after the sun goes down." So, to incorporate into the definition of UFO anything implying extra-terrestrial, extra-dimensional, purely psychological, or some even more exotic origin is non-productive, restrictive and can lead only to confusion and dead ends.

We all know what stars look like, yet note how many definitions are possible for them. To attempt a "complete" definition of stars would run to pages and would include equations of nuclear reactions, radiation transfer, etc. How much more difficult to attempt such a definition if one knew nothing about stars and had never seen one! Yet many who attempt a definition of a UFO have never had a UFO experience, to the best of my knowledge.

Yet all of us have (or could easily have) read many UFO reports. There is, then, some justification in attempting a definition in terms of UFO reports, (which I once attempted with partial success [UFO Experience, pp 3-4, 10]). After all, we do not study UFOs; we study UFO reports, and if we must attempt any definition at all, it might as well be an operational definition (something like the operational definition of Science: Science is what scientists do). On this basis, a UFO is what UFOs are described to be, and to do, in a UFO report.

Now, if a report is later discarded because a normal explanation has been found for it (a balloon, a meteor etc.) the contents of the report are no longer unidentified and hence can play no part in the composite definition of UFO which must obviously apply only to things which remain unidentified.

There are many things in life and all around us that are unidentified in one sense or another, in science, in law, in medicine, and especially in the "borderland" regions of human experience: ESP, miracles, leprechauns, astral projection etc., although there is an extensive literature on all these subjects. To the extent that any of these enter into the current flow of UFO reports (say, in the responsible UFO journals over the past several years), then they must be included in the operational definition of UFO.

We cannot forget that we are fishing in unknown waters. If our nets occasionally bring up a strange looking creature, we are

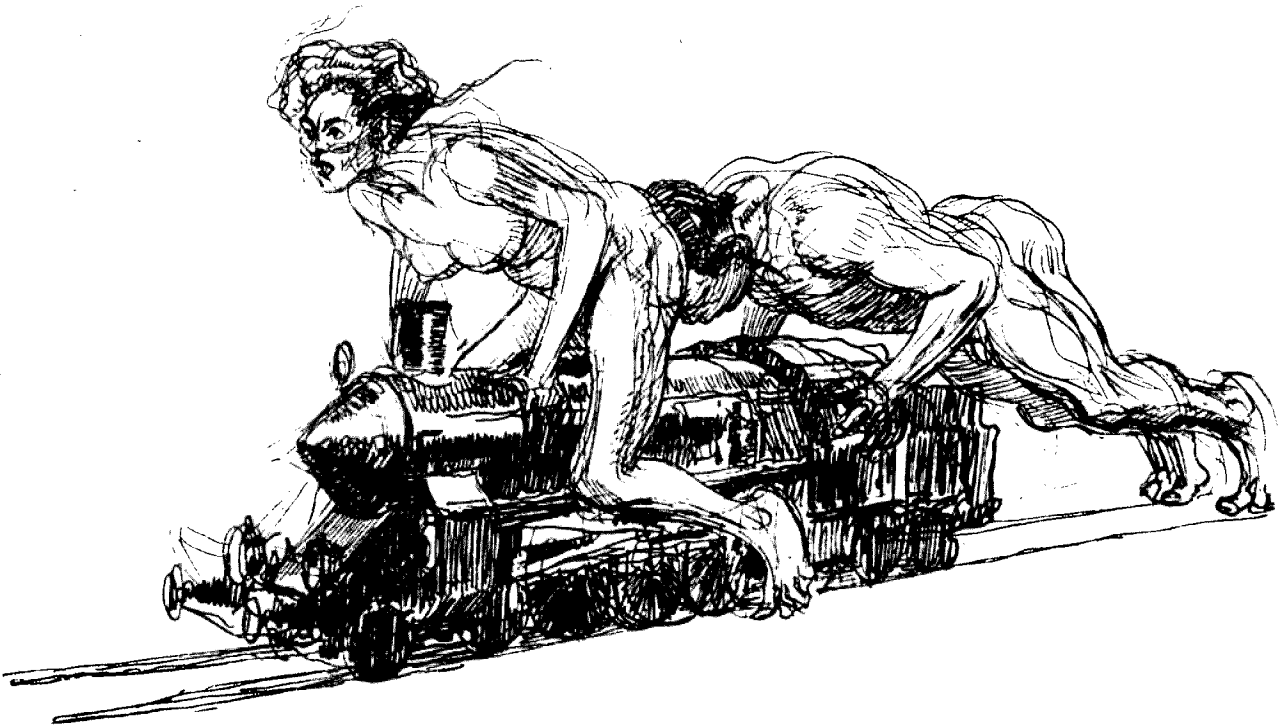
not justified in throwing it out on the grounds that it doesn't fit our accepted definition of "fish." It is clearly a part of our catch of the day, and what we bring up in our nets is, by definition, a part of the day's catch. UFO reports are our "catch."

If this approach seems far too broad, you might like Hilary Evans' proposed definition (Zetetic Scholar, #10, p. 157): "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."

Apart from the hair-splitting fact that this definition excludes the majority of UFO events (which are not reported but for whose existence we have a great deal of circumstantial evidence) it offers little scope for reports which emphasize bright lights and say nothing of "objects." Yet these are a part of the UFO phenomenon and are very frequently reported.

For myself, I find it useful to think of the UFO phenomenon as that defined by the continuous flow, from many parts of the world, of reports of objects and/or sources of luminosity, perceived in the atmosphere or on the ground, whose origin and behavior remain unidentified even after competent study.

By the way, which definition of "star" do you prefer?



IN DEFENSE OF PARAPSYCHOLOGY: A REPLY TO JAMES E. ALCOCK

JOHN PALMER¹

Parapsychology: Science or Magic? (Alcock, 1981) is the latest in a series of books by members (or ex-members) of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) attempting to discredit parapsychological research, which according to Alcock's system of belief is not scientific. Although only about half of the book deals with parapsychology as such, it nonetheless differs considerably from its predecessors in scope. Almost all the major arguments against parapsychology of which I am aware are articulated by Alcock, usually quite well. It thus provides an ideal frame of reference for the continued debate about parapsychology. This is why I have chosen to devote considerable time and effort to a detailed response to the book, and why I am so grateful to Marcello Truzzi for giving me sufficient space in ZS to develop my themes.

Unfortunately, the tone of Alcock's book is also representative of most critical commentaries on parapsychology. It is highly polemical, extremely arrogant, and completely destructive in intent. It is also personal: it is more an attack on parapsychologists than on parapsychology. Although I am not a polemicist by nature, I do not intend to be academically polite in response to seeing my field taunted and bullied. I would rather have a friendly and constructive dialogue emphasizing points of potential or actual agreement, but the attitude Alcock expresses toward the great majority of parapsychologists is so condescending that such an approach on my part would be inappropriate. Alcock has asked for a fight, and he is going to get it.

Like most good polemics, Alcock's case against parapsychology appears on the surface to be devastating. Although he does manage to make some valid criticisms, his case depends primarily on the use of rhetorical devices, of which the following three stand out:

1. Biased selection of references and outright misrepresentation of the parapsychological research literature.
2. Righteous hyperbole camouflaging specious or vacuous arguments.
3. Passing off metaphysical dogma as rationality and science.

Justifiable space limitations require me to limit my critique

¹I wish to express my appreciation to Dr. Martin Johnson for his support and helpful criticisms of an earlier version of this manuscript. Whatever deficiencies remain are, of course, fully my responsibility.

²Two critics who do not behave this way are Ray Hyman and my Dutch colleague Piet Hein Hoebens.

primarily to Alcock's discussion of experimental parapsychology, and even on this topic I will by no means be able to expose all of the book's non sequiturs and misleading statements. However, I can discuss enough of them to expose Alcock as an unfair and untrustworthy critic, at least in this book.

PART I: DO PARAPSYCHOLOGISTS BEHAVE LIKE SCIENTISTS?

If Alcock's book has one major theme, it is that parapsychologists behave more like magicians (in the occult sense of the term) than scientists. This theme is immediately evident in the title. Even the earlier chapters, which present much useful discussion about various ways people can deceive themselves, serve in large part as a setup for the allegation that parapsychologists, blinded by a fanatical belief in magical ideas, routinely commit the same errors in their work. Although parapsychology shares some superficial organizational similarities to the rest of science, according to Alcock parapsychologists' conduct is anything but scientific. The reader could easily get the impression that we cynically pretend to be scientific simply to draw upon the authority of science to legitimate our "magical" ideas in the eyes of a credulous public. The indictments Alcock brings against parapsychologists are extremely harsh, and they deal with our motives as well as our output. Such indictments demand a great deal of supporting evidence based on thorough knowledge of parapsychology and its research literature. Alcock claims to have followed the literature, at least, for over a decade. Let's see what kind of evidence he comes up with.

A. Process-Oriented Research

One way that Alcock tries to demonstrate that parapsychologists do not behave scientifically is to show that our research consists almost entirely of isolated demonstrations of the phenomena and reflects no interest in exploring how psi might be integrated with psychological and physical processes. He says, for example, that "the bulk of the parapsychological literature continues to reflect an obsession with trying to demonstrate that psi occurs" (p.142).³

Although I agree that parapsychologists could make more use of knowledge from other scientific fields in their research, statements such as the one quoted above are at best highly misleading, and obviously so to anyone who even skims the journals. A substantial amount of research has been conducted, especially during the last decade, to identify psychological factors that might enhance or inhibit psi as a basis for understanding how psi might interact with normal psychological processes, or, in a few cases, for developing predictive indices. Considerable work has been undertaken, for

³All page references to Alcock's book refer to the hardback edition.

example, relating ESP to psychological and psychophysiological measures of altered states of consciousness, based on the idea that competing "linear" thought processes may be psi-inhibitory. Relevant theoretical discussions have been published by Braud and Braud (1974), Honorton (1977), and Stanford (1979). Other parapsychologists have attempted to study individual difference variables in relation to psi. Moderately replicable relationships which have stimulated interpretive discussion include, for example, ESP and defense mechanisms (Johnson & Kanthamani, 1967), ESP and extraversion (Eysenck, 1967), and the infamous "sheep-goat" effect (Palmer, 1972). Models for the cognitive processing of psi have been proposed by Irwin (1978) and Tart (1977), and some research has dealt with the cognitive processing of psi information (e.g., Kelly, Kanthamani, Child, & Young, 1975). This list is by no means complete.

To further document the extent of process-oriented psi research, I took a survey of the full experimental reports published during the last decade (1971-1980) in the three major research-oriented parapsychological journals listed by Alcock in his Suggested Readings. Sixty percent of them included as a major element the exploration of relationships between psi and "normal" psychological or physical variables, or they involved analyses of the data clearly designed to illuminate psychological or physical processes which might be mediating the psi effect.^{4,5} The degree to which the theoretical rationale for these relationships was developed in the research reports varies widely, but only in a handful of cases were predictors selected on a purely ad hoc basis, as Alcock implies on p.128. (Cases that obviously fell in this category were not included among the 60% in the survey.) Whether based on an explicit theoretical rationale or not, the determination of psychological and physical correlates of psi contributes in an important way to the theoretical process by providing the requisite data base for later integration, interpretation, and hypothesis formulation. Alcock's cynical implication in his table on p.144 that these studies are nothing but attempts to extend the mere demonstration of psi to other scientific areas is unsubstantiated, unwarranted, and unfair.

The best example of formal theorizing and hypothesis testing in parapsychology is Stanford's theory of "Psi-Mediated Instrumental

⁴The figures in this survey could vary a few points either way, depending upon how one classifies borderline cases. But the trends I am citing here and below are robust enough to withstand such adjustments. I tried to be moderately conservative in my classifications. I would be pleased to provide details of the survey to anyone interested.

⁵There proved to be considerable variability on this dimension among the three journals sampled. If one read only the Journal of the Society for Psychical Research, one would likely describe psi research very much the way Alcock does. On the other hand, the application of his characterization to the Journal of the American Society for Psychical Research over the last decade is simply preposterous. The latter publishes many more experimental reports than the former.

Response" (PMIR), which attempts to link psi to principles of need reduction theory in psychology. In two elaborate theoretical papers he outlined this theory, demonstrated how it integrates existing data, and presented it in the form of explicit postulates (e.g., Stanford, 1974). He and his colleagues then conducted a series of empirical studies testing (and often confirming) hypotheses deduced directly from these propositions (e.g., Stanford & Stio, 1976). Yet Alcock, who repeatedly sees fit to proclaim, for example, that "parapsychology lacks anything at all that resembles a serious theory" (p.120), and "has failed to come up with testable hypotheses" (p.129), never once mentions the PMIR theory or any related research. It is also revealing that whereas he pays considerable attention to Helmut Schmidt's early research, he never once mentions his later work based on deductions from his theory (e.g., Schmidt, 1976).

These are no isolated lapses. Whereas 60% of the published research in the past decade was concerned with exploring relationships between psi and other variables (as noted above), this was true of only eight of the 27 independent experimental reports (30%) by parapsychologists or experimenters clearly sympathetic to parapsychology cited by Alcock in his book. Even this figure is much too generous, since only one of these eight was cited in a way that would sensitize the reader to its process-oriented aspects. (This was the metal-bending research of John Taylor, for which the process hypothesized was a conventional physical one.) In three cases the predictor variables were not even mentioned. In one case where they were mentioned, an experiment by Schmidt and Pantas, the citation was used to imply that psi has rarely been shown to be affected by situational variables (p.169). Although this is true for the most part of Schmidt's research, it is not a fair summarization of process-oriented psi research generally (Palmer, 1979).

Why did Alcock for all practical purposes deny the existence of any conceptually or process-oriented psi research? Most of the examples cited in this section (along with numerous others I could have cited) are either published in journals or reviewed in books which he cites in his Suggested Readings and from which he drew other references, so he must be aware of them. Alcock may not like the underlying theoretical premises of this research, consider it very sophisticated, or think that the results "add something crucial to the case for the paranormal" (p.vii), but that is not the point. Alcock's purpose in these sections is to prove that parapsychologists do not engage in the kind of research activity characteristic of other scientists. To the limited extent that he bothers to document these allegations at all, he does so by citing a minority of studies that seem to support his case and ignoring a majority of studies that refute it.

B. One-Shot Miracles?

Alcock later criticizes parapsychologists for failing to systematically follow up initial findings, describing the research as "a series of one-shot demonstrations" (p.142). It is easy to cite examples to support this claim (as Alcock does with Schmidt's initial forays into the field), but it is just as easy to cite examples to

refute it, such as the long-term research project with Stepanek on the "focusing effect" (Pratt, 1973). I found that 37% of the studies in my survey were attempts to either replicate or extend findings of previous research by the author or another experimenter. This percentage is very conservative, because it fails to include numerous instances where two or more related experiments are published in the same report, or cases in which the experiment builds upon a general line of previous research rather than upon a particular experiment or group of experiments. The possibilities for doing systematic research on psi are limited by its presently low reliability (as Alcock acknowledges), and I quite agree that the followups are often not as incisive or extensive as one might like, but to characterize psi research as "dominated by one-shot miracles" (p.143) creates a very misleading impression about both the nature of psi research and the motivation of many psi researchers.

C. Parapsychology and Physics

Given all the hoopla over parapsychologists' lack of a coherent theory and the need to integrate psi with the rest of nature, you might think that Alcock would have at least a little something nice to say about the recent efforts of some paraphysicists to try to account for psi by extensions of modern quantum physics. Not for a moment. One of his complaints is that "quantum mechanical arguments ... remove the focus of argument between proponent and critic from the mundane world of statistical analysis and experimental design to a plane where it is very difficult for the non-physicist to debate" (p.116). Apparently Alcock objects to any theorizing that is too technical for him to understand! Moreover, the "observational theories" of psi, based upon quantum mechanics, have been subjected to empirical test using standard principles of experimental design and statistics (e.g., Bierman & Wiener, 1980). Alcock then goes on to imply that parapsychologists really shouldn't worry about theorizing after all, because there is no good evidence that there are any real phenomena to explain. The premise is highly debatable, but even if it is true, this kind of argument overlooks the fact that one function of good theorizing is to guide research in directions likely to produce sound evidence if the phenomena do exist.

In his discussion of parapsychology and modern physics, Alcock treats us to another dose of biasedly selected references, this time manifested as a series of straw men which he effortlessly strikes down. The straw men are five extrapolations from modern physics, which Alcock claims some paraphysicists cite as providing support for the belief in psi. The principles are relativity theory, the EPR paradox, time reversal, the Heisenberg Uncertainty Principle, and tachyons. It is revealing that he goes through 3 1/2 pages of discussion of these extrapolations without once referring to a paraphysicist of any stature, probably because no paraphysicist of any stature would endorse the extrapolations he attacks, at least not in the simplistic form in which they are presented in the book.

In his discussion of the EPR paradox, for example, he quotes an article by Gardner (1979) as a source for the remark that "Some proponents of psi ... argue that this paradox implies that quantum

information can be transferred virtually instantaneously from any part of the universe to any other" (p.113). It turns out that Gardner's sole reference for this remark is "paraphysicist Jack Sarfatti" (Gardner, 1979, p.39). Sarfatti is not considered a paraphysicist by any member of the professional paraphysical community I know of, and I don't know of him ever having claimed to be a paraphysicist. His lack of impact on theory in parapsysics can be documented by noting that in the Proceedings of the Parapsychology Foundation's Conference on Quantum Physics (Oteri, 1975), which is probably the most comprehensive survey of responsible paraphysical thought yet published, Sarfatti's name is not mentioned once. (Gardner at least mentions this book in his critique, albeit condescendingly). He has never published in a major parapsychological journal or presented a paper at a Parapsychological Association convention.⁶

Alcock does not hesitate to trot out the heavyweights when they agree with his point of view, though. He quotes part of John Wheeler's blast against parapsychology at a recent conference of the American Association for the Advancement of Science (p.114), but the reader never learns that a number of other distinguished theoretical physicists take a more sympathetic view toward the possible reality of psi (see, e.g., Oteri, 1975).

The two major paraphysical theories in parapsychology, both of which are special cases of the "observational theories" mentioned above, are by Schmidt and Evan Harris Walker. These theories are indeed mentioned briefly (and condescendingly) in a footnote later in the text (p.119), but they should have been the focal point of the section on parapsychology and physics if that section were to have any credibility. Both of these theories are far from fully developed, especially in terms of the derivation of their empirical consequences, and both are based on interpretations of quantum mechanics that are currently not the dominant ones in physics. They are by no means above criticism. Yet the efforts of these and other paraphysicists are sincere and often sophisticated initial attempts to integrate psi with modern physics in ways that are scientifically valid. They deserve more than the condescending brushoff given them by Alcock.

D. Criticism Within Parapsychology

Alcock concedes that parapsychologists do criticize each other's work, but he complains that "such criticism generally tends to be subject oriented", limited to "a wide diversity of belief about what constitutes 'real' psychic phenomena" (p.120). To some extent this is true, not because of the intrinsic nature of the phenomena, but because the volume of methodologically sound research with

⁶Parapsychologists are not responsible for the pronouncements of every individual who claims to be a parapsychologist or who has something nice to say about psi. The only sensible criterion for the application of this label is membership in the professional organization of the field, the Parapsychological Association. It's membership list can be obtained by writing P.O. Box 7503, Alexandria, Va. 22307, USA.

positive findings is greater in some areas than in others. Experimentalists, for example, are often skeptical about the evidence for such phenomena as psychic apparitions because they are difficult if not impossible to study in the laboratory, and thus the only evidence comes from spontaneous cases. Surely Alcock is not going to complain about this!

But granted this qualification, the literature clearly reveals Alcock's claim of little within-subject criticism to be groundless. This point can best be illustrated by examining the one class of phenomena that almost all parapsychologists accept, namely ESP and PK as demonstrated in experimental contexts. Criticisms by parapsychologists of the ESP and PK research of other parapsychologists appears quite frequently in the parapsychological journals, especially during the last five years. For instance, from 1977 to 1980 the Journal of the American Society for Psychical Research devoted over 100 of its pages to a virulent controversy about Tart's ESP-learning research, including criticisms from three separate parapsychologists and replies from Tart. My survey revealed at least 11 other articles, letters, or book reviews in these journals by parapsychologists criticizing positive claims made on behalf of other ESP or PK experiments, not to mention the frequent publication of more general methodological critiques and critiques of research on other psi phenomena. Compared to what I see in most psychology journals, this is a very good track record.

The above data are derived solely from journals listed by Alcock in his Suggested Readings, thus from material which we must assume he is familiar with but chose to ignore. Probably the most critically oriented of the major parapsychology journals is the European Journal of Parapsychology, which Alcock is apparently not familiar with. Much criticism also occurs in the process of reviewing (and often rejecting) manuscripts for journal publication or presentation at parapsychological conventions. The latter offerings must sometimes endure further criticism at the convention itself.

Alcock also complains about a lack of theoretical controversy within parapsychology. Although the field is just beginning to mature in this respect, Alcock's implication that such controversy does not exist is fallacious. The best current example of such controversy concerns the observational theories. He acknowledges this himself in a footnote on p.119 where he cites a strong critique of the theories of Schmidt and Walker by parapsychologist Stephen Braude. Alcock obviously threw in this footnote at the last minute, apparently oblivious to the fact that it contradicted a major aspect of the thesis he was trying to develop on the very next page. Other examples of theoretical controversies or controversies with theoretical overtones include how psi is distributed in the population, whether, and if so how, altered states of consciousness are psi-facilitory, the psychological effect of feedback, the nature of out-of-body experiences, and, of course, survival of death.

Despite the existence of theoretical controversy within parapsychology, it is true that most criticism within the field is based on methodological rather than theoretical considerations. I see nothing wrong or unscientific about criticism within parapsychology

being based on something other than the "results go[ing] against someone's pet theory" (p.120). Alcock's implication that theoretical controversy is necessary to provoke methodological criticism is ridiculous and an insult to any scientist who takes pride in maintaining the methodological standards of his or her discipline.

But Alcock apparently sees a greater role for theory in the evaluation of data than simply as a catalyst for methodological critiques. On p.123, he moans that "different writers give opposing views on the reality of some aspects of the paranormal, not because these aspects do not 'fit in' with some theoretical overview, but for some reason they have chosen to be critical of the research in those particular areas ... " (*italics his*).⁷ This seems to imply that the "reality" of a given paranormal phenomenon should be evaluated (I presume he means in part) by how well it fits in with some theory. For reasons I will discuss later, it does not surprise me at all if Alcock believes this, but I was always taught that in science the "reality" of something is supposed to be determined exclusively by such empirical criteria as the adequacy of the methodology leading to the relevant observations and the reliability of those observations. In other words, theories are evaluated by their correspondence to data, not the other way around. Was I misled? If so, then perhaps science and religion are not so different after all. More on this in Part III.

E. The Experimenter Effect: Parapsychology's "Catch-22"?

Most scientists agree that valid scientific hypotheses must be falsifiable. A key point made by Alcock in attempting to support his thesis that parapsychologists behave unscientifically is his contention that the psi hypothesis is unfalsifiable. His chief example is our supposed use of the "experimenter effect" (EE) to "explain away" nonsignificant results. If an experiment fails to provide evidence of psi, so the argument goes, the parapsychologist simply dismisses the experiment post hoc on the grounds that the experimenter was a skeptic or for some other reason not psi-conducive.

This seems to be a particular sore point with Alcock, and some important albeit subtle distinctions get lost in the emotionalism. Let me begin by clarifying what I think it is fair to say most responsible parapsychologists are and are not saying about the EE. They are saying that:

1. Some experimenters seem to be consistently more able than others to get positive results in psi experiments.

2. One important factor distinguishing successful from unsuccessful experimenters seems to be "belief". Experimenters who accept the psi hypothesis are more likely to get positive results than are skeptical experimenters.

They are not saying that:

⁷The reasons, as noted above, are methodological.

1. There is something intrinsic in the nature of psi that makes it impossible for skeptics to experience it directly or obtain it in their experiments.

2. Negative results, especially by skeptics, really lend support to the psi hypothesis by demonstrating the EE. Every parapsychologist I know recognizes the lack of repeatability as a weakness in the case for psi, although some contend that there is enough repeatability to make the case convincing in spite of, not because of, the failures.

It is true that parapsychologists frequently cite the EE as one possible explanation of why a study failed to obtain significant results. Given the considerations discussed below and on p.63, I see absolutely nothing wrong with this. On the other hand, the term "explain away" implies that the EE is being offered as the only or the objectively preferable explanation of the failure. But parapsychologists almost never make this claim. The only reference Alcock cites that could remotely support such a contention concerns the failure by Beloff and Bate to replicate the experiments of Schmidt (p.136-137). Although Beloff and Bate do say that their "failure in no way detracts from Dr. Schmidt's success", they were referring to the fact that their results in no way demonstrated flaws in Schmidt's procedures, not that their failure to replicate should be ignored in assessing the validity of the hypothesis Schmidt was testing.⁸ For Beloff and Bate to have concluded from their finding that Schmidt's experiment was faulty would have been unjustified and, in my opinion, unethical; all that is known at this point is that Beloff and Schmidt obtained different results, for an unknown methodological reason.

On the other hand, the EE is one topic about which I can at least see how Alcock might have been misled by the literature. In correspondence with me on this topic,⁹ he alluded to a comment of my own in which I offered something akin to the EE as a possible explanation of a failure to replicate a ganzfeld experiment (Palmer & Aued, 1975). Specifically, I said "the most likely villain, in our judgment, is the social psychological factor." Although I intentionally expressed this as a personal opinion and later acknowledged that "any one of a number of other situational or experimental variables could have differentially affected the results of the two experiments", my expression of a subjective preference for this interpretation could perhaps be misconstrued on casual reading as a claim of objective preferability. I agree with Alcock that the expression of such personal opinions should best be left out of formal reports. But I emphatically do not agree that mentioning such possibilities per se is inappropriate, especially when (as in my case) they arise from clear methodological differences in the two studies or

⁸Beloff has confirmed to me the accuracy of this characterization of his position.

⁹I received a very courteous and thorough reply from Dr. Alcock to a set of questions I raised concerning passages in the book I found unclear.

otherwise have heuristic value for further research.

The issue here is really quite straightforward. Both sides agree that there is a relationship between the attitudes of the experimenter and the results of psi experiments. This has even been demonstrated in some systematic investigations in which experimenter belief was treated as an independent or predictor variable (e.g., Taddonio, 1976). The question is how to interpret it.

Alcock's explanation seems to be that "believers" conduct incompetent experiments that allow for artifacts (or they cheat), whereas skeptics are presumably free from these deficiencies. (Readers of the last issue of ZS may begin to question this latter assumption, if they ever accepted it in the first place.) Parapsychologists usually offer one of two "psi" explanations. One view is that believing experimenters are better able to put their subjects at ease and to inspire confidence, thereby helping the subject to focus on the task and overcome possible resistances to psi. The other view is that in most psi experiments it is the psi of the experimenter rather than that of the subject which is responsible for the results. Since psi is really quite rare in the population, only some experimenters have it, and those who do are "believers" because they have it.

First of all, if psi exists as a subtle human capacity poorly under the subject's control, then at least the former of these psi hypotheses is plausible based on what we know about other subtle psychological processes that are not considered to be paranormal. For example, some drug research has shown that the effect of placebos on patients' recovery is influenced by whether or not the administering physician believes in the drug's efficacy (e.g., Uhlenhuth, Rickels, Fisher, Park, Lipman, & Mock, 1966). This is a special case of the well known "experimenter bias" effect of Rosenthal. The general thrust of experimenter bias research is to suggest the effect of subtle verbal and non-verbal cues by the experimenter on the attitudes, feelings, and motivations of the subjects, in line with the first psi hypothesis. It clearly does not support the kind of incompetence or fraud assumed by the skeptical hypothesis (Rosenthal & Rubin, 1978). There is some direct evidence that factors of the former kind might also influence psi results (e.g., Honorton, Ramsey, & Cabibbo, 1975). The "experimenter psi" hypothesis, while perhaps lacking the same degree of superficial plausibility as its counterpart, is more than just an ad-hoc rationalization. A body of empirical research exists which directly and indirectly supports it, and hypotheses have been developed to account for it within the framework of already existing psi theory (Kennedy & Taddonio, 1976; Millar, 1978).

In neither case is the research evidence consistent enough to be compelling, but it is strong enough to justify taking these hypotheses seriously. Moreover, neither of these psi explanations necessarily imply that it is intrinsically impossible for skeptics to obtain positive results. Assuming that belief is a correlate of psi, it is very unlikely that it is a direct cause of variability in psi scores. It is much more likely that it mediates or simply covaries with some other processes in the subject, experimenter, or both that are the more immediate causal agents. If the latter could be identified, they likely could be controlled in such a way as to allow

skeptics to more readily get positive results.

But the bottom line is that noone really knows how to interpret the EE. What should be apparent, however, is that the solution to the problem is research, not rhetoric.

F. Ignoring "Normal" Interpretations

At several points in the book, Alcock accuses parapsychologists of "ignor[ing] competing 'normal' explanations for whatever [they] might observe" (p.144), the implication being that we do so deliberately. It is true that parapsychologists often do not bring up such "normal" explanations when interpreting their results in the "Discussion" sections of experimental reports. The reason, of course, is that the researcher seeks to eliminate such artifacts in the design phase, so there usually is no need to deal with them in "Discussion". Of course, parapsychologists are human and do not always find every flaw, but I think anyone who reads the research literature with an open mind will see that in most cases parapsychologists are sensitive to such artifacts and do their best to eliminate them. Ironically, in one case where a parapsychologist did deal with possible artifacts in "Discussion" (an experiment by Tart to be discussed in Part II), Alcock chastizes him for not dealing with these artifacts in the design phase. Talk about Catch-22s!

G. Ad Hominem Attacks

Alcock is not satisfied to merely attack the research of parapsychologists as reflected in their published research reports. In a later section of the book, entitled "A Skeptical Approach", he uses the lack of consistent replicability in parapsychology as an excuse to not only condone, but actually call for ad hominem attacks against parapsychologists. Now obviously, evidence of dishonesty or chronic incompetence on a researcher's part should cause us to discount any research by that individual, regardless of how elegant the research reports may look. Personal factors are also sometimes relevant when direct links to a particular piece of work can be established. But ad hominem arguments in the hands of crusading critics with axes to grind can rapidly degenerate into something that science cannot afford to tolerate, especially when such critics intentionally or unintentionally mislead the reader about the relevant facts.

Consider the case of Helmut Schmidt, who is treated shamefully throughout this book. On p.177 we read the following sentences:

If professor X attests that he observed a 'psychic' perform levitation under 'totally controlled conditions', is it not proper to assess his credentials as an observer? If he has been 'taken in' before, should we not be leery of accepting his word this time? Gardner (1977) named several leading parapsychologists who have been 'gulled' in the past -- Helmut Schmidt ... was much impressed by the psychic ability of Uri Geller ... (p.177).

Unless a reader takes the unlikely step of checking the Gardner reference, he or she will automatically interpret this to mean that Schmidt was "taken in" by a fake performance of Geller which he personally witnessed; i.e., evidence of poor observational skills. But this is not the case. Gardner's reference is to remarks made by Schmidt at the end of a review chapter on PK for Edgar Mitchell's anthology Psychic Exploration (White, 1974). In this section, from which Gardner quotes certain passages out of context, Schmidt cites observations by other scientists who had witnessed Geller; there is no indication that he had seen Geller perform anything himself. Although he expressed genuine excitement about what he was hearing from these other scientists concerning the dramatic kinds of phenomena Geller seemed able to produce -- remember, this was in the early days when little was known publicly about Geller -- he did not say that he was convinced Geller possessed genuine PK ability, only that he and other such subjects "may" possess it. Is this the kind of evidence on which we are supposed to completely discount Schmidt's research -- not with metal benders (concerning whom Schmidt has never published a single investigation), but with people (many of whom were ordinary off-the-street volunteers) and animals whose task was to get a random number generator to give out 1s instead of 0s?

Of course, the passages quoted by Gardner do expose Schmidt as a "believer" in the psi hypothesis, which is certainly understandable given the results he had been getting in his own research. Considering all the fuss about the EE, it would appear that this "belief" is sufficient to invalidate any of his research in Alcock's mind. But the obvious fact, in psychology at least, is that most experiments are conducted by researchers who "believe in" the hypotheses they are testing. I have rarely seen this even discussed, let alone proposed as a basis for rejecting their work or of demanding replication by "skeptics" before it is taken seriously. Alcock frequently sets parapsychology up against a romanticized concept of orthodox science that bears little resemblance to reality.¹⁰

Another victim of Alcock's "skeptical approach" is Hal Puthoff, who is described as a "practicing Scientologist" (p.178). The clear implication is that Puthoff is not to be trusted because of his involvement with this group, a controversial quasi-religious organization whose doctrine espouses, among many other things, the reality of psi. But Puthoff has told me that he is not a practicing Scientologist, nor is this claimed by John Wilhelm or Ray Hyman, the two references he cites in this connection. According to Puthoff, he took several Scientology courses many years ago and has not "practiced Scientology" since then. This is consistent with what Wilhelm and

¹⁰Since we cannot trust the results of scientists who believe in their hypotheses unless they are replicated by skeptics, it seems to follow that if scientific research in any field is to be interpretable, we must have reliable information about who is who. I am sure Alcock would agree that, at a minimum, scientists should be required to reveal their "true beliefs" in their research reports. But how could we be sure they are telling the truth? Lie detectors, perhaps? But who could we trust to administer the tests? CSICOP? The brain boggles.

Hyman report. Now if the Scientology organization had infiltrated the SRI research or was gaining some tangible benefit from it, there would be cause for concern. I am aware of no such evidence. Based on the available information, Alcock's insinuation bears more than a slight resemblance to the attempts by Sen. Joseph McCarthy to discredit left-wing intellectuals in the 1950s by citing previous flirtations with the Communist Party or ostensible front organizations.

Is science so fragile and decrepit that it needs to resort to this kind of muckraking to defend itself against parapsychology? Throughout the rest of the book Alcock literally inundates the reader with conventional scientific arguments which are at least sound enough to support the claim that psi has not been established conclusively, a conclusion accepted by the overwhelming majority of scientists. If these arguments are as devastating as Alcock seems to think they are, why does he need ad hominem attacks?

Finally, I would remind Alcock, and some of his fellow CSICOP members, that such ad hominem attacks can be a double-edged sword. It is no secret that CSICOP is penetrated to its core by a militant and doctrinaire atheistic humanism that has strong ideological reasons for wanting to see parapsychology discredited. If these individuals believe that ideological biases disqualify parapsychologists from being taken seriously, then they must admit that they themselves are disqualified on the same basis.¹¹

H. Overstating the Case

Several times throughout the book Alcock criticizes parapsychologists for failing to adequately present opposing (i.e., skeptical) viewpoints, ignoring failures to replicate, omitting references to shortcomings of particular studies, etc., in their review articles and popular books. This is one instance where I think that to some extent he has a legitimate gripe. For example, I agree that the skeptical viewpoint is not adequately represented in the Handbook of Parapsychology (Wolman, 1977). I am not alone among parapsychologists in feeling, for example, that the book would have benefited from the inclusion of a chapter written by a responsible critic summarizing the skeptic's case. This does not detract from the book's considerable merit as a sophisticated statement of the pro-psi position, but it would have been a better book had the skeptical view been adequately summarized. I also feel that most popular books overstate the case for psi, both in terms of its existence and its metaphysical implications. This includes many of those written by parapsychologists, although not so much as those written by others.

But Alcock is hardly in a good position to criticize anyone else of biased reporting. As one more example, on p.123 he properly chastizes O'Brien for failing to mention that Layton and Turnbull had

¹¹I do not mean this to imply that I favor using such affiliations to try to discredit the work of any critic, nor do I condone the few instances when this may have been done in the past.

failed to replicate a positive psi experiment. Yet two pages later, he cites a study by Wilson (1964) as having failed to find the "sheep-goat" effect, without mentioning that Wilson did find it in one of the two experiments he reported.

The fact is that books and articles written by skeptics are at least as one-sided on the average as those written by psi proponents. Alcock tacitly admits this when he divides his "Suggested Readings" into "Viewpoints critical of" and "Viewpoints sympathetic toward" parapsychology. The only scholarly publication I know of which is truly balanced is the one you are reading.

I. Parapsychology and the Media

One point on which most parapsychologists and their critics agree is that the mass media present a highly sensationalized and distorted picture of the subject matter of parapsychology. Much of the hoopla centers around self-proclaimed "psychics" whose skills at public relations and show-biz eclipse whatever paranormal powers they may or may not possess.

Alcock devotes six pages in his book to this problem. Most of what he says in those pages I agree with, as I think would most parapsychologists. Recently, the Parapsychological Association conducted a survey of its Full Members, asking them to evaluate the current status of the field. According to a press release distributed to the media, "Unanimity among the respondents was especially evident in expression of concern that their task as serious researchers is made difficult by working in a field overrun with frauds, pseudo-scientists, psychic entertainers, fortune tellers and the like. Attracting a lion's share of the media's attention with their various claims and activities, these non-scientists have incurred an unflattering public image that responsible researchers in the field must counter."

Such an outcome would come as quite a shock to anyone whose only source of information about parapsychologists' attitudes on these matters was Alcock's book. On p.186, he proclaims that "The parapsychologists themselves seem disinterested in trying to separate the wheat from the chaff...". With reference to a quote by Beloff which expresses concern about the impact of occultism and media misinformation about it on the young, he then states without qualification or any documentation that "Beloff's view is not representative of parapsychologists in general." I think I know parapsychologists better than Alcock does, and I would be very surprised if the great majority did not share Beloff's sentiments.

But what have parapsychologists actually done about this problem? Frankly, not enough, but more than Alcock gives us credit for (or probably knows about). For example, parapsychologist Keith Harary recently spent two years earning subsistence wages and literally risking his life trying to help refugees from various religious cults. Bob Morris has offered to consult with the American Association of Retired Persons about how to protect the elderly from psychic fraud. At least two psi researchers have presented papers challenging claims

of psi training made by such organizations as Silva Mind Control (Stanford, 1975) and Transcendental Meditation (Mishlove, 1980). I have tried to do my bit by cooperating with Randi on a few of his investigations. Even Fate Magazine, which is too risqué even for most parapsychologists, has a quite good record of publishing articles debunking fake "psychics" (Clark & Truzzi, 1981).

One reason parapsychologists have not done more is that most academics are not temperamentally suited for this kind of debunking activity. Alcock himself acknowledges this when he complains how difficult it is even to get skeptical academics to speak out.

Another reason is that, because any kind of serious study of parapsychology (other than pure debunking) has generally been blacklisted by the academic elite, parapsychologists must often depend for both moral and financial support on elements of the general public highly partisan in favor of occult ideas. Given this state of affairs, I think it is remarkable that parapsychology has been as resistant to occult influences as it has been. If the situation ever were to change, skeptics might be surprised to discover that they had an aggressive ally in combating that substantial element of the "psychic scene" which we all agree is nonsense.

J. Conclusion

Do parapsychologists behave like other scientists? Every profession has its black sheep, and I will not deny that there is room for improvement in many of the areas Alcock mentions. But granted these qualifications, the answer is still a resounding "yes", especially when one considers the constraints imposed by the elusive nature of the process under study. Alcock creates the opposite impression through a series of rhetorical devices, the most prevalent of which are biased selection of references and misleading summary statements.

In fairness, I should mention that, immediately following an apparent attempt to justify this biased selection by appeal to irrelevant criteria (see p.42 above), Alcock does invite the reader not to trust his (Alcock's) judgment, but "to turn to the various works listed under Suggested Readings at the back of [the] book" (p.vii). I would urge the interested reader to take him up on it. The list includes a generally excellent selection of books and journals sympathetic toward parapsychology. Reading this material may or may not convince you that psi exists, but it certainly will convince you that Alcock's representation of psi research is, to put it charitably, misleading.

PART II: THE CASE AGAINST PSI

Alcock could concede every point I raised in Part I and still argue that there is no evidence for psi. In Part II, I will focus more directly on Alcock's case against the existence of psychic phenomena.

A. What Constitutes Evidence for Psi?

Before we can decide what kind of case Alcock makes against psi, we must come to grips with the question of what we mean by evidence for psi. This requires that we get into some relatively complex conceptual and statistical issues, which Alcock addresses in his chapter entitled "Parapsychology and Statistics."

How do parapsychologists define psi? There are many definitions, some of which are controversial. One which I think most of us would accept is the following: Psi is a statistically significant departure of results from those expected by chance under circumstances that mimic exchanges of information between living organisms and their environment, provided that, a.) proper statistical models and methods are used to evaluate the significance, and b.) reasonable precautions have been taken to eliminate sensory cues and other experimental artifacts. This definition is not as precise as would be ideal, but it will suffice for the present discussion.

Alcock begins his critique with a tortuous exercise in elementary logic culminating in the hardly profound conclusion that "statistical evidence is never, of itself, 'proof' of anything" (p.148). Who ever said that it was? Parapsychologists only consider statistical evidence to be "proof" (Alcock's term, not mine) of psi if they feel reasonable precautions have been taken to eliminate alternate hypotheses, as indicated by the above definition. Alcock cites none of us as saying anything to the contrary. This is just one more example of Alcock's rhetorical device of beating down straw men, which he does with especially great frequency in this chapter.

It also should be evident from the above definition that Alcock's assertion that the so-called "psi hypothesis" is unfalsifiable is incorrect.¹² It is falsified whenever results from a psi experiment conform to the expected chance distribution. Such cases count as strikes against the psi hypothesis, unless or until boundary-defining hypotheses that would render them irrelevant (e.g., a non-artifactual interpretation of the EE) are independently established. Alcock often seems to be confused about this point. For example, on p.169 he accuses Schmidt of making a "non-falsifiable" claim for the presence of psi because he speculated that the experimenter rather than the subjects (who were cockroaches) might have been the source of a significant psi result. (*italics added*)

It is evident from his discussion that Alcock's confusion concerns the nature of psi as a construct, a point which admittedly is often not clear in the writings of parapsychologists themselves. It is important to recognize that psi is a descriptive construct, not an explanatory one. It is a label that we apply to a certain class of anomalies for which we lack a satisfactory explanation. Psi is an

¹²I do not like the term "psi hypothesis", because it implies that one is explaining an anomaly rather than merely affirming one (see next paragraph). However, this is not the place to introduce new terminology.

accurate description of what Schmidt found in his research. His claim of psi would have been falsified had the outcome conformed to "chance", i.e., been nonsignificant. His speculation about whether he or the cockroaches were the source of the psi was based on implicit theories intended to explain an aspect of the already established psi effect. The validity of these interpretations -- a hot topic in parapsychology these days -- of course requires additional articulation and experimentation before it can be evaluated properly.

Alcock then goes on to question the appropriateness of chance models in parapsychology on the grounds that they rarely hold in nature. Although it is true that there is no good theoretical reason to expect farm dwellers and city dwellers to have the same mean IQ (to use Alcock's example on p.150), there is a very good theoretical reason to expect a properly functioning Schmidt random event generator (REG) to produce an equal number of 1s and 0s within specified margins of error. If the machine is not functioning properly (i.e., it is biased), this is a problem of experimental control, not a reason to abandon chance distributions. The latter are the foundation of the great bulk of research in the social and behavioral sciences, including experiments that employ control groups. Is Alcock really willing to throw out all those babies with the bathwater?

Of course, different specific chance distributions must be used in different circumstances. In ESP card tests, for example, chance models (and the corresponding statistical formulas) must be modified when the randomness of the target sequence is restricted by there being an equal number of each kind of target in the deck. Further modifications are needed if subjects receive trial-by-trial feedback of targets. Adjustments must also be made for multiple analyses when, for example, an investigator looks for displacement effects as well as direct hits. Although Alcock concedes that modern parapsychologists are aware of these adjustments, he contends that "some of the classic studies ... often referred to as providing the best case for ESP, were run without taking account of such problems" (p.154). Alcock cites no reference for this remark, and the only studies I can think of to which the criticisms he cites might apply in a nontrivial way are the very early Rhine experiments (Rhine, 1934/1973), which no responsible parapsychologist has taken seriously since the 1940s. The trivial ones that applied to Rhine's later work were addressed very early in the game (e.g., Greenwood, 1938).

It is true that most statistical models used by parapsychologists assume some kind of random distribution of targets. There of course are isolated exceptions, but by and large parapsychologists are very conscientious about seeing that these assumptions are met or (in rare cases) apply appropriate corrections to the statistical analysis if they are not. Since 1955, most distributions not emanating from REGs have been derived from the thoroughly tested tables of random numbers published by the RAND Corporation (1955). Most researchers who use REGs frequently run control tests on them to assure they are functioning properly.

On p.149, Alcock asserts that "Parapsychological researchers rarely use control groups, and instead usually compare the outcomes of a psi experiment with what one would expect if chance alone were

operating." At the very least, this statement is highly misleading. As noted earlier, many psi studies explore relationships between psi and other variables. Many of these studies manipulate the latter as independent variables, predicting (or at least exploring the possibility) that one treatment will show more psi, or show it in a different direction, than the other. Even those studies using correlational procedures contradict the second clause of Alcock's statement. The statement is simply part of Alcock's attempt to deny the existence of conceptually oriented psi research.

However, it is true that parapsychologists rarely employ control conditions as a means of detecting artifacts. They prefer to deal with potential artifacts directly by eliminating all they can think of from their procedure throughout. As a general rule, it is not at all clear that formal control conditions would be more successful in identifying artifacts, nor does Alcock provide us with any insights on this point. If a parapsychologist is aware of a possible artifact, he or she eliminates it directly; if the parapsychologist is not aware of it, how could he or she set up a meaningful control against it? For a further discussion of this issue, see Palmer (1981).

Some evaluation procedures in parapsychology have built in controls that serve some of the same functions as formal control conditions. One place where Alcock creates a highly misleading impression by overlooking this fact is in his evaluation of free-response ESP studies such as the SRI remote viewing work and the Maimonides dream experiments. He implies that the analysis procedures used in these studies did not provide a baseline that would control for coincidental correspondences between targets and responses. As an illustration of the evaluation procedure in the Maimonides studies, he cites an example mentioned by Romm (1977) "in which the sender was installed in a room draped in white fabric and had ice cubes poured down his back. A receiver who reported 'white' was immediately judged to have made a 'hit' by an independent panel." He then goes on to quote Romm that "'miserable', 'wet', or 'icy' would have been better hits" (p.165). Maybe, maybe not. The point is that in the evaluation procedure, a hit was assigned not because "white" was subjectively considered the best possible description of the sender's situation in that trial, but because it described his situation during that trial better than it described his situation in other trials. If "white" were a freak correspondence, one would have expected that over the series of trials similar freak correspondences would have occurred between transcripts and targets designated for other trials, causing them to be regarded erroneously as hits and thereby washing out the effect. In other words, for any given trial, the other trials in the experiment provided the baseline Alcock demands of his control conditions, but you would never know this reading Alcock's misleading descriptions of the procedures in question.¹³

¹³It would appear from the biographical sketch accompanying her article that Romm's academic specialty (if she has one) is English! Her only evident qualification for the role of scientific critic is arrogance, with which her article literally overfloweth.

Of course, no control procedure will be effective if it is misapplied, as is alleged to have occurred in some of the SRI remote viewing experiments, where according to some accounts the judging materials were not adequately randomized. But this does not mean that the procedure itself is faulty.

B. Other Statistical Nonsense

The above section by no means uncovers all the flaws in Alcock's chapter on "Parapsychology and Statistics". Two of the residuals can be disposed of expeditiously, so I will do so here.

1. On pp.150-151, Alcock tries to devalue the results of psi experiments by noting that the magnitude of the effects are small, their high levels of statistical significance due to the large number of trials. Although the magnitude of an effect is indeed important in applied contexts, it is not necessarily important when the issues are theoretical, as in most psi research. Some of the most important experiments in modern physics, for example, deal with effects of very small magnitude.

2. Beginning on p.157, Alcock tries to use the results of Oram's matching of sections of random number tables, in which Brown found a significant quartile decline (QD) effect, as a reason for rejecting the corresponding effect in PK dice studies. Apart from the obvious problem of the incomparability of the two target generation procedures, whatever credibility the QD effect still has as evidence for PK is based on the fact that it was discovered repeatedly over a series of experiments, some from different laboratories (Stanford, 1977). Unless Alcock is prepared to argue that the QD is somehow intrinsic to certain classes of "random" matchings, Oram's one-shot exercise is hardly a relevant analogy for his purposes.

C. Bundles of Sticks

When you challenge "skeptics" to give you their one decisive argument against the existence of psi, you often get some version of "Show me one conclusive experiment that rules out all 'normal' explanations of the results." Alcock never makes such a statement directly, but it is obvious at several places in the book that he strongly sympathizes with this kind of thinking. He flirts with it as early as the Foreword. On p.6, he cites with obvious approval Hansel's thesis that ESP has not yet been demonstrated because parapsychologists have failed to provide a "conclusive experiment", which Hansel defines as an experiment whose "result may be due to [no] cause other than ESP" (Hansel, 1979, p.20).

Alcock himself provides the rebuttal to this kind of argument when he later stresses that "in the case of psi, it is never possible, as I have said repeatedly, to conclude that the putative phenomena is responsible for the non-chance results because one can never be certain that one has eliminated all possible contaminating variables" (p.161; italics added). Earlier, referring directly to Hansel, he observes that "even if such cheating [by the subject] is eliminated,

there is always the possibility of fraud on the part of the experimenter" (p.139; italics his). How can parapsychologists be asked to provide a "conclusive experiment" when such an experiment is impossible? It is ironic, to say the least, that one who complains so bitterly about parapsychologists' allegedly applying nonfalsifiable criteria readily embraces such criteria himself when proposed by someone who agrees with his point of view.

Let us turn now to the more serious skeptical argument, which concerns replicability. (In fairness, this is the argument upon which Alcock places the most stress.) First of all, I think one must concede that, although there is some replicability in parapsychology (which, echoing Hyman, Alcock backhandedly admits on p.136), there is not enough, and what there is is not widespread enough, to support the claim that the evidence for psi is conclusive. If this were all Alcock claimed, I would have no quarrel with him. Even if he claimed that the evidence was only suggestive, or even weak, I would disagree with him but have to concede that his position was within the bounds of reason. But Alcock's claim is "that there is not even a prima facie case for the existence of psi" (p.146), that "There is no evidence that would lead the cautious observer to believe that parapsychologists ... are on the track of a real phenomenon ..." (p.196; all italics his). Now this is certainly macho, but is it plausible?

In science generally, but particularly in the social and behavioral sciences, conclusions about the validity of propositions are based not on single experiments, but upon groups of experiments none of which are considered decisive by themselves. In fact, this is implicit in the very concept of replication. In psychology, for example, one frequently finds books or review articles in specialized journals (e.g., Psychological Bulletin) in which the published experiments on a given topic are reviewed, their relevance to particular theoretical propositions discussed, and conclusions about the status of the propositions in light of this evidence drawn, often rather tentatively. Rarely are the experiments reviewed even replications of each other, except in the broadest sense of the term. This process, which is central to the adjudication of competing knowledge claims in psychology, is sometimes called the "bundle of sticks" approach, because the sticks together are considered to be much stronger than each one is separately.

Of course, the "bundle of sticks" approach is also central to the case for psi. Alcock addresses it all too briefly on p.143, where he dismisses parapsychologists' use of this approach as giving in "to the combined appeal of a collection of weak studies." The key word here is obviously "weak", and I can only assume that this is how he justifies denying parapsychologists the use of a principle so central to other fields of science. It is true that an implicit but important qualification of the "bundle of sticks" criterion is that the individual sticks must meet some minimal standard of strength. One thousand testimonies of seeing the Virgin Mary from celebrants at a drunken orgy would hardly provide much evidence for the lady's presence. Unfortunately, what constitutes a sufficiently strong experiment to be included in the "bundle" is hard to specify, but there are minimal standards of adequate methodology upon which most psychologists (and parapsychologists) would agree, even though they

might not be able to fully articulate them.

As an example of a weak experiment in psychology, consider a study by Alcock and Otis (1980). In the book, Alcock concludes from this experiment that "it would seem ... that believers in the paranormal, at least in the student population, tend to be ... less skilled in critical thinking than are skeptics" (p.53). The methodology of this "experiment" was so poor that even the authors had to admit that the results cannot be considered as providing conclusive evidence. It is indeed surprising that anyone would be even the slightest bit impressed by them.

Alcock's "evidence" consists of one marginally significant ($p < .05$) and unreplicated difference between two extreme groups, reflecting a relationship between a truncated version of an attitude scale developed by Schmeidler and something called the Critical Thinking Appraisal Inventory. As his supposedly representative sample of "the student population", he chose one class of introductory psychology students. Apart from the obviously biased nature of the sample, surveys have repeatedly shown that psychology professors are among the most hostile of all academics to the paranormal. It is not at all unlikely that some of the subjects in the experiment were aware of this. Students with the greatest skills at "critical thinking" are also likely to be the best students, and thus the ones most likely to identify with their professors' attitudes or be seen as identifying with them -- not because of the intrinsic merits of the attitudes, but because they happened to be held by their role models for critical thinking! Just because students have critical thinking skills does not guarantee that they will always use them. If only a few "goats" had been influenced by the above mentioned factors, it would have been sufficient to destroy the marginal level of significance obtained.

It would have been a very simple matter to at least include a few items in the questionnaire concerning the reasons why subjects held their beliefs. Yet Alcock, a man of considerable experimental savvy so far as other kinds of psychological research are concerned, reported no attempts whatsoever to ascertain possible biases of this kind. This lapse is all the more remarkable when one considers the distribution of attitudes actually reported. Although a recent survey of psychology undergraduates at the same university where the experiment was conducted found that only 3% were skeptics (p.25), at least 25% of Alcock's original sample in the experiment not only were skeptics but "rejected all the phenomena described by the seven items [on the attitude scale] (Alcock & Otis, 1980, pp.479-480; italics added). Alcock must have found this outcome noteworthy, or else why would he have bothered to mention it? No comparable elaboration was offered about the "sheep" sample. Wouldn't the cautious investigator be at least a little suspicious that his sample might have gotten a bit of brainwashing sometime before the experiment? Since Alcock never mentions this possibility in his report, we must assume that he either never thought of it or didn't consider it worth worrying about. This uncritical attitude was apparently shared by the editors of Psychological Reports, or they would have demanded more rigorous methodology before accepting the paper.

If the reader has found the above criticism to be excessive,

he or she has at least partly grasped my point. Although Alcock's study would hardly qualify as a model of methodological excellence in social science research, I would not characterize it as "weak". It was good enough to be published in a reputable, refereed professional journal, which is the closest we have to an operational definition of minimally acceptable methodology. It deserves some weight in assessing the validity of Alcock's hypothesis.

I tried to make this experiment appear weaker than it was through the use of some very simple rhetorical devices. The trick is to latch onto any conceivable loophole you can find in a study, however obscure. Then use hyperbole to blow the "defect" out of proportion, making it appear that the study is completely worthless because of it. Finally, portray the experimenter as a biased and/or incompetent fool for overlooking such "obvious" flaws. This kind of thing is generally rather easy to do, especially when the author is "foolish" enough to mention possible weaknesses of the study in his or her report (as in the case below).

My model for the above critique of Alcock's experiment was Alcock's critique of an experiment by Tart concerning the apprehension of a target number during an "out-of-body experience" (OBE). It is the only psi experiment he discusses in any detail. The critique appears on pp.129-131. I will only quote selected excerpts here, to highlight his use of the rhetorical devices mentioned above.¹⁴

"Tart's (1968) original 25-page article reveals that the 'experiment' was so loosely controlled that even its author had to admit that the [result] cannot be considered as providing conclusive evidence ... It is indeed surprising that anyone would be even the slightest bit impressed with what was reported to have occurred. [Then follows a description of the experiment, in which a subject sleeping with EEG electrodes attached, correctly identified a five-digit number placed on a shelf out of reach above her bed during what she reported upon awakening to be an OBE. He then quotes Tart:] 'I monitored the recording equipment ... and kept notes of anything she said or did. Occasionally I dozed during the night ... so possible instances of sleep talking might have been missed.' If he was interested in sleep talk, why not use a tape recorder? ... Now pay particular attention to what Tart, a man of considerable experimental savvy so far as 'normal' psychological research is concerned, did next, not before the experiment but following it: '...I inspected the laboratory carefully ... to see if there was any way in which this number could have been read by non-parapsychological means ...' [Alcock then describes how Tart surmised that the subject could have cheated using reflections from a clock face (which could only be seen if the target was illuminated by a flashlight) or through reaching rods or mirrors, but doubted that these occurred.] Apparently, the editors of the Journal of the American Society for Psychical Research shared this doubt, or

¹⁴ Alcock goes on to criticize Tart for failing to note the alternate interpretations of his study in his subsequently published popular book. This is a different point, which I have already addressed (see p. 51 above).

they would have demanded more stringent controls before accepting the paper... [Alcock then notes that Tart moved the clock for the next session, but the subject had to discontinue the research for personal reasons.] One might wonder about the probability of such personal difficulties occurring just as changes were being made to decrease the probability of cheating." (pp.130-131; italics his).

Alcock's attempt to make Tart look like a fool is hardly subtle. Although Tart's strategy in this instance may not have been exemplary, neither was it inappropriate for an initial investigation. Parapsychologists are constantly being assaulted by people claiming psychic powers, very few of whom can demonstrate any. It is understandable that an investigator would not worry about trying to set up completely foolproof conditions until there was reason to believe there may be real phenomena present. When such evidence appeared in this case, Tart consulted a magician friend, which resulted in the counter-hypotheses and provisions for tighter control.

The insinuation about why the subject quit also is not very plausible if you stop to think about it. The subject had only been successful on one of four nights previously, so failure on the fifth night would hardly have aroused suspicion. A cheat ambitious and clever enough to use the kinds of methods Tart hypothesized would want to hang around to see if there were some other way to fool the "gullible" experimenter.

This initial, exploratory study obviously is not conclusive. In fact, it is below average in terms of methodological rigor of psi research -- one more example of Alcock's biased selection of references (yawn). On the other hand, the only alternate hypothesis on the table is sophisticated fraud by a woman who, far from trying to establish credentials as a psychic, agreed to participate in the research anonymously and has never to Tart's or my knowledge had her name publicly identified with it. Tart's study deserves some weight in our deliberations, just like Alcock's study does.^{15,16}

In summary, Alcock's total dismissal of the evidence for psi rests implicitly upon the use of rhetorical hyperbole to support the characterization of all inconclusive psi experiments as evidentially worthless experiments. Since there are no conclusive psi experiments, it then follows that there is no evidence whatsoever for the existence of psi. The logical fallacy in this line of reasoning should be evident. In fact, it is nothing more than Hansel's "conclusive experiment" argument in disguise!

D. Conclusion

¹⁵I will let Alcock defend his study in his rebuttal, if he wants to.

¹⁶This in no way is meant to imply that one should not attempt to make one's experiments as methodologically sound as possible. The sounder the methodology, the more weight the study should receive.

So where does all this leave us in terms of a more realistic assessment of the case for psi by the "bundle of sticks" criterion? The number of experiments showing positive evidence for ostensible psi effects is enormous. Also, many trends exist which show moderate repeatability (especially in terms of directional consistency), hang together conceptually, and make psychological sense -- that is, if we are permitted to think of psychology independently of physicalist dogma (see p.66 below). These experiments vary in quality, and a certain percentage have undoubtedly misinterpreted artifacts as psi, but many are good enough to be published in professional psychology journals were the claims less theoretically (and metaphysically) controversial. Alcock never seriously addresses this evidence in his book. A detailed discussion of it is beyond the scope of this article, but comprehensive summaries, albeit from a pro-parapsychology perspective, can be found in Krippner (1978,1979) and Wolman (1977).

Exactly how one assesses this evidence depends upon a number of factors, many of which are subjective. The latter include, of course, the a priori probability one attaches to the psi hypothesis. The minimum that can justifiably be claimed for this evidence is that parapsychologists have established a prima facie case for psi. Whereas rhetoric perhaps can undermine this modest conclusion, reason cannot.

PART III: PARAPSYCHOLOGY, SCIENCE, AND METAPHYSICAL DOGMA

A. Metaphysics and Intolerance

Science is supposed to be a process in which its practitioners observe nature in as unbiased a manner as possible, develop theories to economically and satisfyingly explain those observations, predict new observations from the theories, and finally either modify the theories, redefine their range of applicability, or discard them, based upon the results of tests of these predictions.

This process requires that the scientist approach nature with a simultaneously open-minded and critical attitude. Although "objectivity" may be an unattainable ideal, it is still the ideal.¹⁷ For this reason, metaphysics and science are rarely a good mix, not because metaphysical ideas are intrinsically bad or even useless to science, but because metaphysical thinking tends in practice to be dogmatic. The scientist who approaches nature with a set of rigid metaphysical beliefs is likely to observe, or be willing to observe, only those aspects of nature that conform to these beliefs, and to tolerate only those theories that are in accord with them.

Implicitly throughout the book, but most explicitly on p.106, Alcock accuses parapsychologists of mixing metaphysics and science. I have no doubt that metaphysical biases have adversely affected the

¹⁷I use the term objectivity here to refer to the mind-set of the scientist.

research and theorizing of some parapsychologists, and that this indeed has been a major problem for the field. My purpose here, however, is to show that exactly the same difficulty contaminates the writings of some skeptics, and specifically that Alcock's book is an excellent illustration of the problem.

Despite occasional flirtations with objectivity, Alcock's metaphysical prejudices are exceedingly transparent. For example, he begins his chapter on "Magic, Science, and Religion" with the following blockbuster:

In the name of religion human beings have committed genocide, toppled thrones, built gargantuan shrines, practiced ritual murder, forced others to conform to their way of life, eschewed the pleasures of the flesh, flaggelated themselves, or given away all their possessions and become martyrs. (p.7)

The fact that some of the same atrocities have been committed in the name of secular ideologies is not mentioned.

Equally transparent is Alcock's intolerance of ideas that do not conform to his own worldview, ideas which, of course, include those considered by parapsychologists. For example, he repeatedly criticizes parapsychologists for merely entertaining paranormal interpretations of otherwise unexplained or inadequately explained experimental outcomes. On p.165, I am taken to task for suggesting that the blind judging of a free-response ESP test (see p. 56 above), in which the results differed significantly as a function of who served as the judges, was "possibly influenced by¹⁸ paranormal processes" (entire phrase italicized by Alcock). On p.158, Schmeidler is chastized for daring to suggest that psi "could" have been used to select a suitable entry point into a random number table in an experiment by Oram where the resulting sequence proved to be nonrandom. The most blatant example, however, is the discussion of Morris' review of research designed to demonstrate psi in plants. Although Alcock explicitly concedes that Morris doubted the validity of Backster's original positive findings because of subsequent failures to replicate by other investigators, he nonetheless criticizes Morris for merely saying that "we still cannot rule out" the experimenter effect as an alternate explanation for Backster's success (p.123). Morris neither stated nor implied that Backster's research makes a positive case for either plant psi or the EE. If an emerging science is to have any chance of reaching maturity, it must be given the right to entertain hypotheses, even ad hoc hypotheses, which, if established, would resolve a problem or crisis, or perhaps even lead to a breakthrough. For a discussion of the role of ad hoc

¹⁸ Alcock goes on to accuse me of not sufficiently considering alternate normal interpretations of this finding. He fails to mention that I do consider one such artifact in my discussion (Palmer, Khamashta, & Israelson, 1979, p.341). If he has others in mind, he should have the decency to state what they are so they can be addressed, not just drop hints and rhetorically ask the reader to "judge for himself" if some unspecified error had been committed.

hypotheses in the history of science, see Lakatos (1970).

Alcock is no more tolerant when it comes to possible explanatory frameworks for psi. On p.170, he describes Schmidt's theorizing about the "goal-oriented" nature of PK as "magical thinking" because Schmidt postulates no "intermediate steps" between source and effect. The same epithet is applied on p.129 to Stanford's conformance model, without the slightest acknowledgement that a.) Stanford adopted this model because he felt forced to conclude that his earlier, more mechanistic PMIR model failed to adequately account for much of the data it was intended to explain, b.) that he pointed out testable implications of the model, or c.) that he recognized that the model must be subjected to experiment as a means of verification.¹⁹

Alcock's complaint seems to be that the models of Schmidt and Stanford do not postulate intervening mechanisms. But this is true even of some aspects of respectable physics. For example, what "intermediate steps" would Alcock propose to account for the instantaneous action at a distance effect described by d'Espagnat (1979) in his discussion of quantum mechanics? Teleological thinking has played an important role in modern biology (Hull, 1974). Finally, one would be hard pressed to find much discussion of intervening mechanisms (as Alcock seems to be using the term) in "black box" behaviorism, which until recently was the dominant force in orthodox experimental psychology. Theorizing occurred to some extent in this tradition (and to a greater extent in its neo-behaviorist offspring) by the use of hypothetical constructs or intervening variables (e.g., reinforcement) to account for functional relationships between stimulus and response variables, but "reinforcement" is no more or less of a mechanism than is "observation" or "will" in the goal-oriented psi theories. I have heard behaviorism called many things, but never magic.

Surely Alcock must be bothered by something more than a lack of mechanism. The next section addresses what I think is a more important irritant.

B. Does Psi Contradict Science?

At several points in the book, Alcock implies that if psi were true it would contradict (orthodox) science. On p.191, for example, he asserts that "If psi exists, science as we know it cannot." The point is stated even more crisply by Alcock's favorite philosopher, Mario Bunge: "Faced with a choice between these 'hard' sciences [among which he includes economics!] and primitive superstition [i.e., parapsychology], we opt for the former" (Bunge, 1980, p.17).

¹⁹With reference to the emergence of theorizing in parapsychology along the lines of Stanford's model, I stated in a recent address, not cited by Alcock, that "those who wish to destroy us ... will seize upon what they see as a new opportunity to link parapsychology to popular occultism" (Palmer, 1980, p.210). I see it has not taken long for my prediction to be confirmed.

Obviously, if faced with that choice we would choose science. Just as obviously, we are confronted with no such choice.

Pretend for a moment that someone suddenly comes up with the repeatable psi experiment and on March 1, 1983, at 12:00 noon, some authoritative tribunal of scientists declares that psi is an established fact. What would happen? At 12:01 would the laws of orthodox psychology and physics suddenly stop operating? Would it no longer be possible to teach hungry rats to run mazes for food, or would light no longer travel at 186,000 miles per second? Or, as Alcock suggests on p.191, would the laws currently governing these phenomena suddenly be replaced by some sort of psychic principles? That would hardly be likely. First of all, science would not abandon its conventional laws for the very simple reason that they work, and they work very well. Secondly, if Alcock were to examine the parapsychological literature in search of relevant psychic principles, he would discover that parapsychologists have little if anything to say about what motivates rats to run mazes or why physical energies behave as they do. Parapsychology can no more contradict psychology and physics than botany can contradict geology: although there are certainly points of interaction and interface, they deal with different classes of events. As Alcock himself observes, "Parapsychological anomalies ... do not get in anybody's way" (p.111). If physics wanted to incorporate psi in its domain (which it by no means would be required to do), it would need to come up with a unified theory that dealt with currently defined physical phenomena at least as well as its present theories do. Such a theory would need to be much more sophisticated than anything parapsychology currently has to offer.

In summary, the existence of psi per se poses no threat to science. Its most likely effect would be to stimulate other scientists to enrich the theories in their own domains by attempting to incorporate psi data. In physics, such attempts might resemble the present theoretical efforts of paraphysicists, but from a broader perspective. Psi is not a threat to the validity of current scientific laws and theories, only to their universality. To put it another way, the only threat is to those who want to believe that the current theories of orthodox science provide a complete general description of reality. But this is primarily a metaphysical urge, not a scientific one, and it is rooted in dogma.²⁰

C. Science and Dogma

If there remains any doubt that Alcock is trying to use

²⁰I wish to stress that I both recognize and appreciate the impulse within science generally, and physics in particular, to seek a coherent, unified theory of nature. What I am objecting to is the perversion and transformation of this noble impulse into a metaphysical statute which denies legitimacy by fiat to any theoretical framework that is not subsumed by the current occupant(s) of the throne.

metaphysical dogma to censor scientific theorizing, it can be eliminated by noting his enthusiastic embrace of eight principles formulated by Bunge to distinguish "science" from "pseudo-science".²¹ I wish to focus specifically on his eighth principle, which proclaims that pseudo-science "has a world-view admitting elusive immaterial entities, such as disembodied minds, whereas science countenances only changing concrete things" (p.117). This is meant to be more than a statement of historical fact; it is an attempt to shackle science to a particular metaphysical doctrine, namely materialism. Because "pseudo-science" is a highly value laden term, it constitutes, in effect, an attempt to censor any theorizing that is incompatible with this dogma.²²

Based on a recent book (Bunge, 1980), I suspect that Bunge would try to justify this censorship in the psychological sciences on the grounds that materialist constructs have been more productive of scientific knowledge in these fields than have mentalist constructs. This premise is highly debatable when the whole range of psychological topics are considered, not just those (e.g., the effects of brain disease) which psychobiologists understandably like to focus on. Psychobiology and its derivatives may ultimately prove capable of dealing with the complexities of human thought and behavior -- let's hope something does -- but it's much too early to hold the party, as Bunge himself admits. Mentalism for the most part has failed in this task as well, but concepts that have failed at one stage of history often succeed at another (Feyerabend, 1975; Lakatos, 1970). It is also far from clear that the failure of mentalism can be blamed on the intrinsic nature of its constructs. The mentalist "mind" is admittedly not a very useful concept, but neither is the physicalist "brain"; both are capable of differentiation and quantification, and independently so. Both can generate research. Much of modern cognitive psychology, for example, employs highly differentiated constructs that bear no clear relation to brain physiology; is all this pseudo-science too?

But the important point is this. If what we mean by science is the systematic, empirically based search for theoretical constructs that help us to explain existing knowledge and generate or predict new knowledge, then there is no legitimate reason why we cannot simultaneously entertain both materialist and mentalist concepts and derive whatever benefits each has to offer. I see no reason why they have to be either compatible or commensurable. Only if constructs are viewed as competing descriptions of "reality" must we make a choice between them. Again, this is metaphysics, not science.

²¹This discussion is based on Alcock's summarization of Bunge's paper. I have been unable to obtain a copy of the paper myself.

²²There is another term, "proto-science", which is sometimes used to label scientific activity that has not reached the level of maturity of the physical sciences, e.g., in terms of quantification. I do not think it would be unreasonable to apply this label to parapsychology, as well as to most if not all of orthodox psychology. But that is much different than "pseudo-science".

Any self-respecting scientist should resent being asked to sign a loyalty oath to any metaphysical doctrine, regardless of what he or she thinks of parapsychology or to which dogma his or her research and theorizing happen to conform (if any). Scientific theorizing requires creativity, and creativity requires freedom. Science can only reach its maximum potential if such freedom exists and diversity of thought is actively encouraged.²³

This is not to say that science has no relevance to metaphysics, or even to religion. Materialists have a right to their "religion" too, and if they want to use some current scientific theories as the basis of their metaphysical worldview, then no one should object. But let us not forget that this is still a leap of faith. What we should object to is any implication that science can prove one worldview to the exclusion of others; that is simply beyond its province.

Just as science should be allowed to provide data of comfort to materialists, it should also be allowed to provide data that might be of comfort to those with more spiritual proclivities. Indeed, this has been and still is a major *raison d'être* for parapsychological inquiry, and I think it is a perfectly legitimate one. One public service that basic science can offer to justify its use of tax dollars is to provide unbiased scientific evidence for those of us who prefer or need to base our faith on empirical data. It should be obvious that to perform this service effectively, science should, collectively at least, be neutral on these issues itself.

D. Conclusion

I have tried to show in Part III how Alcock has used implicit metaphysical arguments to deny parapsychologists the freedom to employ certain theoretical concepts, and why these arguments are not only illegitimate but also dangerous for science generally. I think that modern parapsychological theorizing is inspired more by quantum physics than by magic, but even if I am wrong, that is no justification for censorship. Metaphysics is not a threat to science when it provides ideas, only when it dictates them.

I cannot think of a more fitting way to close Part III than by quoting an unwittingly insightful remark from Alcock:

In summary, then, important or central beliefs often prove highly resistant to the effects of disconfirming information. It is often easy to observe this resistance in others. It is extremely difficult for us to be aware of it in ourselves (p.59; italics his).

²³On these particular points I have drawn inspiration from the writings of Paul Feyerabend, especially Science in a Free Society (Feyerabend, 1978).

PART IV: SOME CONCLUDING THOUGHTS

I think both the most accurate and the most important thing we can say about whatever it is that parapsychologists study is that it is an enigma. I also think it is an important enigma for science to address. If the interpretations of most parapsychologists are anywhere near correct, the theoretical and practical implications could well be staggering. Although, as I discussed in Part III, such an outcome almost certainly would leave the rest of science intact, it would open up whole new vistas for scientific exploration, and it doubtlessly would have considerable impact on our conception of humanity. If, on the other hand, psi turns out to be just artifacts, as the skeptics contend, we still stand to learn a great deal about how such artifacts might affect research in other sciences, particularly psychology. It may be true that parapsychologists have not yet made a compelling case for their pet interpretations of the enigma, but neither have the skeptics made a compelling case for theirs. Those with a genuine desire to make sense of the world will not be satisfied until a credible verdict is in.

"Psi", whatever it is, is a very difficult beast to study. This study is made more difficult by the polemical nature of the psi controversy, which diverts the attention of investigators on both sides from the kind of incisive research that might more quickly yield a solution. This situation will remain until the upper hand is gained by those who humbly seek the answer to the enigma rather than by those who arrogantly proclaim to have already found it. This in turn requires that metaphysics (whether it be materialism or mentalism) and ideology (whether it be Spiritualism or Humanism) take a back seat to science.

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SCIENCE, PSYCHOLOGY, AND PARAPSYCHOLOGY: A REPLY TO DR. PALMER

JAMES E. ALCOCK

Although I did not expect any accolades from Dr. Palmer, I am quite frankly somewhat taken aback by the tone of his review. Indeed, I wonder if the description he applies to my book, "highly polemical, extremely arrogant, and completely destructive in intent" is not a more apt description of his rather savage review. He has obviously put considerable thought and emotion into the preparation of this review; I shall attempt to reply only to the former and try to overlook the latter. Before so doing, however, I would like to state for the benefit of readers who have not read my book for themselves that other reviewers have come to a different conclusion about its tone. For example, Jones (1982), writing in Canadian Psychology, wrote:

"To his credit, he refrains from name-calling and derogation of believers and thereby manages a high-minded treatment of the subject (p.61)."

In Contemporary Psychology, Singer (1982) wrote:

"The tone of the writing is moderate, professional, and cautious. There is no sarcasm or belittlement (p.688)."

Lest Dr. Palmer challenge the credibility of these reviewers because they share my skepticism, let me quote from the review written by parapsychologist Robert Morris (1982) for the Journal of the American Society for Psychical Research. Although he was in disagreement, even strong disagreement, with a good deal of what I had to say about parapsychological research, he nonetheless was able to state:

"In summary, despite the criticisms and reservations expressed above, I found the book one of the most thought-provoking I have read in a long time. I can recommend it for laypersons and professionals alike. It makes many good points and some of its criticisms cannot be dismissed lightly (p.186)."

Enough said in self-defense. I shall now turn to Dr. Palmer's critique. I shall not be responding to every point he raised, for I have neither time nor energy to respond in excruciating detail to a 30 page review. I trust, however, that Dr. Palmer will bring to my attention any serious omissions on my part. I have organized my response in terms of a series of "issues" which for me encapsulate the thrust of Dr. Palmer's myriad objections.

Each "issue" corresponds more or less to one of Dr. Palmer's

sub-headings, and I will go through them in the order which he chose to present them. (Unfortunately, since I do not know what page numbers will be assigned to his review when it appears in Zetetic Scholar, I cannot refer Dr. Palmer's comments by making reference to the relevant page or pages in his review).

ISSUE#1: DO PARAPSYCHOLOGISTS BEHAVE LIKE SCIENTISTS?

Dr. Palmer suggests that I am arguing that parapsychologists behave more like magicians than scientists. With regard to the "magic" of parapsychology, I simply must insist that whenever one finds oneself talking about events occurring on the simple basis of one having wished them, without any understanding of the causal chain involved, (if indeed one exists), then one is talking about something which sounds very much like natural magic. I pointed to an excellent example of this sort of thing in my book (page 129) when I quoted Stanford's comment that psychokinesis ("PK" for short) occurs without mediation through sensory guidance and probably without any form of computation or information-processing by the organism. That smacks pretty much of magical thought to me, and to defend the position that PK can occur in that way, perhaps one must logically go to the length that Beynam did (quoted on page 193 of my book) in arguing that indeed parapsychological treatment of PK should attempt to explain the magical laws of sympathy and homeopathy as well. What amazes me most in all this is that people are willing to make such recommendations, not on the basis of something like evidence that some person can under carefully controlled conditions, and in the presence of neutral or skeptical experts, cause a delicately balanced needle under a bell jar to move, but rather on the basis of statistical departures from chance in an experimental situation in which such a departure gives no more weight to the PK or ESP hypothesis than it does to the hypothesis that fairies sit on some people's shoulders and do their bidding by magical means.

This kind of thinking comes up again and again in the parapsychological literature. However, I did stress in my book that there is great variability among parapsychologists, and some are almost as skeptical as I am about parapsychological claims. Such people are to be considered in quite a different light from those who go around arguing that the existence of psi is no longer at issue, and that attention now needs to be directed to exploring its characteristics. There is no reason why one cannot approach the study of parapsychology scientifically. Unfortunately, when good scientific sense might suggest that the researcher is getting nowhere, magical thinking can creep in and offer some sort of feeling of success in the pursuit of this putative psi force.

While on the subject of magical thinking, (which, incidentally, is related to but quite distinct from magical belief; we all are guilty of the former from time to time, while we may be

able to minimize or eliminate the latter from our systems of belief), I am disappointed to see that Dr. Palmer rather cavalierly dismisses the first several chapters of my book:

"Even the earlier chapters, which present much useful discussion about various ways people can deceive themselves, serve in large part as a setup for the allegation that parapsychologists, blinded by a fanatical belief in magical ideas, routinely commit the same errors in their work."

My point was that we are, all of us, prone to magical thinking from time to time, and that we are, all of us, often unable to recognize that our thoughts, experiences, and beliefs are often, if not always, to some degree distorted vis-a-vis reality. I pointed out that the rise of scientific methodology was in essence a response to the recognized need to try to protect one's conclusions about nature from personal biases:

"...we often persist in drawing illusory correlations and in falling victim to the illusion of validity even when the illusory character is recognized. Good researchers have learned not to trust their own judgements; experiments are run using control groups in order to provide an objective basis for judging the effects of experimental treatment relative to non-treatment... We are all prone to see relationships among events where none exist, and such is the basis of much erroneous belief. The cautious student of nature must not fall too quickly for the causal attributions that come so readily to him (p.104)."

[Note that in his section on "What constitutes evidence for psi, which I shall address in greater detail later, Dr. Palmer states:

"However, it is true that parapsychologists rarely employ control conditions as a means of detecting artifacts. They prefer to deal with potential artifacts directly by eliminating all they can think of from their procedure throughout. As a general rule, it is not at all clear that formal control conditions would be more successful in identifying artifacts, nor does Alcock provide us with any insights on this point. If a parapsychologist is aware of a possible artifact, he or she eliminates it directly; if the parapsychologist is not aware of it, how could he or she set up a meaningful control against it?"

The whole point behind using control groups is to attempt to minimize or eliminate the influence of extraneous variables, suspected and unsuspected. Obviously, control groups cannot guarantee freedom from extraneous influences, (because of possible interaction effects between the independent and any extraneous variables, for one thing). However, to decide that one can get along without them certainly shows self-confidence that scientists in other areas of human behaviour lack. More

about this later.]

I stressed the point that even if psi or ESP (or whatever term one wishes to use) does NOT exist, we should expect people, even OURSELVES from time to time, to have experiences which SEEM to be extraordinary, which SEEM to defy rational explanation. No, Dr. Palmer, it was not an "unwittingly insightful" comment of mine that you chose to quote in this regard, in your rather sarcastic way - I very much meant exactly what I said, and I meant it to apply to you, to me, to all of us:

"...important or central beliefs often prove highly resistant to the effects of disconfirming information. It is often easy to observe this resistance in others. IT IS EXTREMELY DIFFICULT FOR US TO BE AWARE OF IT IN OURSELVES." (p.59, emphasis in original).

I admit that this applies to me, Dr. Palmer. Are you ready to admit that it might apply to you as well? We individually like to think we can explain our beliefs, but as I said in my chapter on beliefs, this is often just justification or rationalization or attribution after the fact. We, each of us I'm sure, like to think that our beliefs are based on rational evaluation of the evidence. This is precisely why the procedures of science are so important: as I mentioned above and in my book, they evolved in the attempt to protect ourselves from the biases and prejudices of each individual's belief system. This is why I am so reluctant to accept the proposition put forth by some who claim the title of parapsychologist that psi is not amenable to ordinary scientific methodology or procedure, that the requirement for replicability should be relaxed, etc. (To his credit, Dr. Palmer is not one of those who do this.)

I would like to point out something else: Because I was very concerned that my own observations about parapsychology might be taken to reflect an outsider's bias, I deliberately chose, frequently throughout the book, to quote directly from the writings of prominent parapsychologists to back up my arguments. I note that Dr. Palmer accused me of not hesitating to trot out the heavyweights when I was criticizing paraphysics; I also note that he did not challenge my use of parapsychological heavyweights to back up many of my other criticisms: He attacked my comments about replicability, but in my book I have cited the comments of several parapsychologists who make the same point I do. He attacks my comments about the experimenter effect and the same thing applies. Indeed, almost every criticism I have made will find agreement with one or another parapsychologist, I'm sure.

ISSUE#2: IS PARAPSYCHOLOGY WITHOUT ADEQUATE THEORY AND DOES IT CONSIST IN THE MAIN ONLY OF ONE-SHOT MIRACLES?

Dr. Palmer takes issue with my statement that "the bulk of the parapsychological literature continues to reflect an obsession with trying to demonstrate that psi occurs (p.142)." He also argues that I have systematically failed to discuss research carried out in the past decade which is theory-directed and which does attempt to relate psi to psychological and psychophysiological variables, and that I am unfair as characterizing parapsychological research as dominated by "one-shot miracles". I must begin with a qualified mea culpa. First, I admit that I was too harsh in my comment about "one-shot miracles". Second, I did not discuss specific parapsychological theories to any extent, primarily because, as Dr. Palmer suggested, I did not consider them to be very important or very well developed. This applies to the process-oriented research that he mentioned as well. Nonetheless, in retrospect, I do believe that discussion of such would have been useful, and in my mind would have strengthened my criticism of parapsychology rather than eroded it. Let me at this juncture point out what a leading parapsychologist has said on the subject, and see to what extent his words back up my own. This is what Rex Stanford said in his Presidential Address to the Parapsychology Foundation in 1973:

"There are a number of secondary reasons why parapsychology lacks suitable conceptual development. One is that much of the research, sometimes termed process-oriented, seems to have been aimed at either simply producing a marked or strong scoring trend of some kind or simply predicting a difference of some kind between two groups of subjects or test conditions. Seldom has research been carried beyond this point. Seldom have researchers moved beyond finding an initial effect to ask how or under what specific conditions the effect comes about. (p.143)"

Later on the same page, Stanford added:

"Correlations of psi performance with such things as attitude or personality variables have been some of our major findings, but the valuable leads from these areas have not been followed up to enable us to make inferences about causal factors. This level of analysis in our experimental work is much better than nothing, but it is not the kind of thing that will make neutral or hostile scientists from other fields take notice of us."

As for theory, Dr. Palmer states that the best example of formal theorizing and hypothesis testing in parapsychology is Stanford's theory of "Psi-Mediated Instrumental Responding (PMIR)" which tries to relate psi to need-reduction theory in psychology. He chastises me again for my statement that

"parapsychology lacks anything that resembles a serious theory". Well, what about Stanford's FMIR model? (I think that Stanford prefers the term "model" rather than theory"; I would concur in this choice of a descriptor). It would again appear to describe what is essentially magic (since I wouldn't want to bias the discussion by my summary of the model, let me simply cite Rush's summary in Advances in Parapsychological Research I, pp. 66-67: "...a human or other organism employs ESP and PK capabilities, usually unconsciously, to realize desires and satisfy needs. With respect to PK particularly, the model assumes that a FMIR incident can involve extrasensory information input and consequent advantageous psychokinetic action without conscious awareness of either the information or the related act." Again magical processes are being described.

As for one-shot miracles, although it is possible to find examples of individuals who attempt to conduct a systematic inquiry into putative paranormal processes, that is not the typical case, as Martin Johnson, in his Presidential Address to the Parapsychology Association in 1976 (as reported in Research in Parapsychology, 1976, page 235):

"The picture of parapsychological research today, as I see it, is characterized far too much by small pieces of research usually without any organic relationship to precisely formulated ideas. As I stressed in my Presidential Message, which was issued in early June 1976, there seems to be excessive extrinsic motivation behind the small, piecemeal projects: to get the results published, to be able to have something to communicate at the next P.A. convention, or to secure a travel grant!" (This is not, of course, as Johnson went on to point out, peculiar only to parapsychologists).

There's something else in this section of Dr. Palmer's review that I would like to take issue with. On page 119, I inserted a footnote in which I cited philosopher Stephen Braude, a parapsychologist, whose comments about another set of contemporary theories, the so-called "observational theories" bear repeating:

"...the conceptual underpinnings of the [observational theories] are exceedingly weak at best and... the theories themselves seem largely nonsensical and lacking in explanatory power." (Braude, 1979, p.343).

Dr. Palmer's response to this is to sarcastically comment that the inclusion of this footnote was obviously a hurried addition on my part, that I did not seem aware that Braude is a parapsychologist as well as a philosopher, and that the addition of this footnote goes against my subsequent argument that parapsychology is without the kind of internal competition that competing theories create in a discipline. First of all, I was well aware of Braude's considerable involvement in

parapsychology, and Dr. Palmer's accusation of a ridiculous lapse in this regard is just another example of his condescending rudeness. As for criticism within parapsychology, I did not argue that criticism as such does not exist. Rather, as I wrote on page 120:

"Indeed there is a wide diversity of belief [within parapsychology] about what constitutes "real" psychic phenomena. Leading parapsychologists disagree among themselves. Some, such as Adrien Parker and John Beloff, are almost as critical as the most critical skeptics. Others accept some phenomena and scoff at others...."

I did not suggest that parapsychology lacks criticism; I argued that because of the lack of competing theories, criticism amongst individuals tends largely to be based on personal beliefs about what phenomena are real and which are not. Braude was not attacking observational theories from the point of view of a competing theory which could also account for the "data"; he was simply making a damning comment about the nature of that theory. That in no way contradicts the point I am making.

By the way, how can Dr. Palmer so easily ignore the REAL point of contention here, the adequacy of the theories as "theories"? Talk about polemics and circumlocution!

ISSUE#3: DOES MODERN QUANTUM MECHANICS LEND SUPPORT TO PARAPSYCHOLOGY?

In his section titled "Parapsychology and physics", Dr. Palmer turns on his charm once again, and sarcastically suggests that "Apparently Alcock objects to any theorizing that is too difficult for him to understand," this on the basis of my comment to the effect that the injection of quantum-mechanical arguments removes the debate about parapsychology from the level of experimental design and analysis to a level where most critics are unable to follow. As for your personal comment about me, Dr. Palmer, once again you are off-target, for I have a degree in physics, (which involved taking more than twenty courses in mathematics and physics at the university level, the majority in physics, including two or three that dealt exclusively with quantum mechanics). I say this only to point out that I am not bemoaning the abstruseness of quantum mechanical arguments. What upsets me is the reaction of people lacking a background in physics who are intimidated by even the term "quantum mechanics", and decide that parapsychology's claims must be genuine if modern physics lends support. They cannot judge the strength or weakness of the claims once the debate is moved into the quantum-mechanical arena. Of course, if quantum mechanics is really relevant, then it should be discussed; yet it is intellectually dishonest for people to go around telling others who are not cognizant of quantum mechanics that parapsychological ideas are given support by

modern physics and quantum mechanics, when the truth of the matter is that various paradoxes in quantum mechanics are being viewed by some, who appear to have a prior belief in psi, as "making psi possible". We do not know how these paradoxes will be resolved; and while one might speculate about their relation to this putative psi, it is improper to use them as support for the psi hypothesis, as is so often done.

However, at this point I have another concession to make. Because I have been so annoyed at the nonsense that has been repeated so often by parapsychologists who obviously know nothing of quantum mechanics, I came down harder than I should have on those psychic researchers who are physicists, i.e. the paraphysicists. Some of them, (but not all of them, by any means) show respectable caution in their discussions of physics and psi. Dr. Palmer is correct in saying that the "simplistic arguments" of which I accused such people of making do not fairly represent their views. I maintain, however, that these arguments are those that are repeatedly offered to the public by parapsychologists who make reference to the supposed support for parapsychology forthcoming from physics.

Incidentally, Dr. Palmer in his response has once again failed to discuss my point. He rails about the unfairness of my comments but does not explain just how it is that quantum theory is going to provide a useful explanation for the putative power of the mind to influence rolling dice, objects whose behaviour is at a macro level rather than a quantum level. Dr. Palmer himself does not hesitate to draw inspiration from the wonderful and wierd world of quantum mechanics. In his 1979 Presidential Address to the Parapsychology Association, he discussed how key-bending might be explained quantum-mechanically. The central notion is that the atoms at the neck of the key are straining to go somewhere:

"Let's assume further that, again under normal conditions, half of the potential states of each atom are such as to create a tendency for the key to bend upward, and half for it to bend downward. On the average, we would expect the number of atoms in each state to be equal, their vectors canceling each other out, leaving the key unbent... the effect of the PK, according to the model, is to bias the distribution of possible states in a direction favouring the bending of the key... (1979, p.192)."

Terrific. Does this kind of magical speculation contribute anything to the discussion of "key-bending"? Is it not more important to try to demonstrate that key bending can occur without the use of the usual human strengths, without the conjurer's artifice? Is not Dr. Palmer's speculation in the same league as trying to explain how the little boy who climbed up the Indian mystic's rope disappeared, without having any solid evidence that the Indian rope trick ever really took place? Parapsychologists who make such egregious use of quantum mechanical arguments are severely uninformed and should stick

with subjects with which they are better acquainted.

Dr. Palmer ends this section with reference to Schmidt and Walker and their observational theories, and how "they should have been the focal point of the section on parapsychology and physics if that section were to have any credibility." Let me remind the reader once more of philosopher/parapsychologist Stephen Braude's assessment of these theories, which certainly would seem to recommend against treating them as the central focus of any discussion on the subject:

"...the theories themselves seem largely nonsensical and lacking in explanatory power (1979, p.349)."

Dr. Palmer accuses me of trotting out the heavyweights. Well, there, I did it again, but as I did often throughout my book, this one happens to be a parapsychologist!

ISSUE #4: IS THERE CRITICISM WITHIN PARAPSYCHOLOGY?

As I have already discussed above, I pointed out in my book that "Leading parapsychologists disagree among themselves. Some, such as Adrien Parker and John Beloff, are almost as critical as the most critical skeptics. Others accept some phenomena and scoff at others...." I did not suggest that parapsychology lacks criticism; I argued that because of the lack of competing theories, criticism amongst individuals tends largely to be based on personal beliefs about what phenomena are real and which are not.

ISSUE #5: THE EXPERIMENTER EFFECT.

Dr. Palmer begins his discussion of the experimenter effect by apparently affirming the need for falsifiable hypotheses in science. He then tackles my argument that parapsychology seeps with unfalsifiability. He states that parapsychologists are NOT saying that there is something intrinsic in psi that makes it impossible for skeptics to experience it or obtain it in their experiments. My claim was that when skeptics fail to replicate a parapsychological experiment, their failure is explained away, by many parapsychologists at least, as being due to the experimenter. Maybe he/she could not induce the proper air of relaxation in the laboratory or whatever, or maybe there was an influence of some kind of negative psi. Note what the editorial in the September 1938 issue of the Journal of Parapsychology had to say in this regard:

"Regardless of the above actual experimental results, it should be recognized that the mere possibility that the experimenter himself may be a factor in the determination of the test results is enough to defeat the (already dubious) argument that negative results prove anything about ESP. Thus it should

be clear to all that the failure on the part of certain experimenters to confirm the ESP hypothesis carries no logical weight against the many confirmations obtained by other experimenters."

Dr. Palmer goes on to say that parapsychologists usually offer one of two "psi" explanations for the accepted relationship between the attitudes of the experimenters and the results of the psi experiments. One of these has to do with putting subjects at ease, etc. The other? Let me quote from Dr. Palmer:

"The other view is that in most psi experiments it is the psi of the experimenter rather than that of the subject which is responsible for the results. Since psi is really quite rare in the population, only some experimenters have it, and those who do are "believers" because they have it."

And later,

"The 'experimenter psi' hypothesis...is more than just an ad hoc rationalization. A body of empirical research exists which directly and indirectly supports it, and hypotheses have been developed to account for it within the framework of already existing psi theory."

I won't ask about the already existing psi theory. I will only express my astonishment that Dr. Palmer can begin this section strongly criticizing me because I stated that the experimenter effect allows parapsychologists to explain away skeptics' inability to replicate psi experiments, and then go on to tell us that the "experimenter psi" hypothesis suggests that the believers are the ones who have psi and thus get results. Therefore, I presume, the skeptics, lacking psi, don't get them! The mind boggles, for now Dr. Palmer is saying essentially what I said. Yet, when I said it, it really seemed to upset him!

He tosses in a final barb, an action to which I have become quite accustomed in his review:

"What should be apparent, however, is that the solution to the [experimenter effect] problem is research, not rhetoric."

Agreed. Let's have no more discussion of experimenter psi by parapsychologists until they have gone out and done the research to which he alludes, whatever that might be.

ISSUE #6: DO PARAPSYCHOLOGISTS IGNORE "NORMAL" EXPLANATIONS?

Dr. Palmer agrees that parapsychologists often do not bring up competing normal explanations in the discussion sections of their papers. The reason, he has the gall to say, is that "the

researcher seeks to eliminate such artifacts in the design phase, so there is usually no need to deal with them in the Discussion". It is astounding to hear this directly from a leading parapsychologist: Individual experimenters are so certain of their ability, and their lack of unconscious bias in the direction of their hypotheses, that they can design experiments which involve, by their own admission, a very elusive phenomenon, an ability or power which may or may not be due to the psi of the experimenter, and yet be so certain that they have ruled out all competing normal explanations that they don't even bear discussion! What can I say? Dr. Palmer is being more damning of parapsychology than I was in my book!

ISSUE #7: AD HOMINEM ATTACKS

I stand accused of McCarthyism! Not because I have gone about getting people fired from their jobs, not because I want to use the rule of law to enforce loyalty to my values, but because I said that Hal Puthoff is a practicing Scientologist! Consider what Dr. Palmer has to say about Hal Puthoff and Scientology:

"But Puthoff has told me that he is not a practicing Scientologist, nor is this claimed by John Wilhelm or Ray Hyman, the two references he [i.e., Alcock] cites in this connection."

Well, first of all, in a letter to Dr. Palmer (to which he refers in his review), I pointed out that I had in my bibliography made an error in the entry for Ray Hyman's statement in this regard. The bibliography refers to the November-December 1977 issue of The Humanist, while it should have read the May-June issue. (Professor Hyman had articles in each of these issues). In the May-June issue, Hyman wrote:

"Puthoff, who has made it to the level of a Class III Operational Thetan in the Church of Scientology, had previously obtained funds to study the Backster effect- the alleged ability of plants to sense by extrasensory means the thoughts of humans (p.16)."

As for John Wilhelm, I did not say that he made any reference at all to Hal Puthoff. I said that in his book, The Search for Superman, he stated that there are a number of practicing Scientologists at the Stanford Research Institute (SRI), where some very prominent parapsychological work has been carried out. Indeed, his words were:

"...despite the large number of Scientologists involved in the SRI research, I found no evidence that any covert conspiracy attempted to subvert or bias the results. But SRI's search for psi WAS marred by severe inconsistencies in method and reporting (p. 279, emphasis in original)."

Dr. Palmer has a lot of other things to say about my "ad

hominem" attacks. I tried hard not to be ad hominem, for I abhor that as much as does Dr. Palmer. It is curious, however, to note the ad hominem nature of some of Dr. Palmer's remarks about me, and his comment about Romm in his footnote 13:

"It would appear from the biographical sketch accompanying her article that Romm's academic specialty (if she has one) is English! Her only evident qualification for the role of scientific critic is arrogance, with which her article literally overfloweth."

ISSUE #8: ONESIDEDNESS OF PSI ARTICLES.

I don't have much to say here. Parapsychological articles rarely make reference to skeptical viewpoints or articles. In my experience, that is not so with skeptically written books. I am not referring to the position that the book or article takes, as Dr. Palmer seems to imply, but rather to the extent to which the writer informs the reader of opposing views and points to sources where one can read about those views. The skeptical literature wins this one hands down.

ISSUE #9: PARAPSYCHOLOGY AND THE MEDIA

What I had to say in my book has not been effectively challenged by Dr. Palmer. He states that he agrees with most of what I had to say in this context. Maybe parapsychologists are more concerned about the misrepresentations of psi by the media than I am aware. I'll even go so far as to take his word for it. I was disappointed to hear that parapsychologists are less resistant to the intrusion of occult ideas into their field than they might otherwise be because, as Dr. Palmer says, "... parapsychologists must often depend for both moral and financial support on elements of the general public highly partisan in favor of occult ideas."

ISSUE #10: WHAT CONSTITUTES EVIDENCE FOR PSI?

If anything disappoints me about Dr. Palmer's reading of my book, it is his comment in this section about my "tortuous exercise in elementary logic culminating in the hardly profound conclusion that statistical evidence is never of itself 'proof' of anything." Regrettably, my point was missed completely, so let me explain: Parapsychologists unfortunately rely heavily on the classical hypothesis-testing model used by psychologists. This model leads people to concentrate on trying to reject null hypotheses, and to ignore, unfortunately, the size of effects or the power of the test involved. The model itself is not so much at fault as is the way it is used. I am extremely critical of the way it is employed in psychology, for it leads to slavish devotion to the size of the level of

significance, and this level of significance is directly affected by sample size, and is not descriptive of the magnitude of the effect. In my advanced statistics course, I have a difficult time teaching fourth year honours students in psychology to forget much of what they learned about statistics in earlier courses. The fact of the matter is that there is a great deal wrong with the way that most psychologists employ statistics. The same applies to parapsychologists. The use of the null-hypothesis approach and the ridiculous and naive reliance on significance levels as a guide to "how great are the odds against the proposition that these data occurred by chance" is a very serious flaw in parapsychological analysis. So often, there is outright chortling about a p-value of the nature of p less than a decimal followed by 24 zeroes and a 1. So? All that indicates is that it is extremely unlikely that the sample of data came from a specified population, with a specified mean. As I said in my book, any inference beyond that is made outside the statistical model. It is absolutely naive and incorrect to suggest that an ESP hypothesis is supported just because a chance model is rejected. We do not know if the chance model was appropriate; we certainly do not know what factors besides ESP might have led to departures from the model if it was appropriate.

Under "Other statistical nonsense" Dr. Palmer again goes on to demonstrate a shockingly weak understanding of the role of statistics in the evaluation of data. He tells us that:

"the magnitude of an effect is indeed important in applied contexts, it is not necessarily important when the issues are theoretical, as in most applied research. Some of the most important experiments in modern physics, for example, deal with effects of very small magnitude."

That is certainly true about physics, but physicists don't go around rejecting null hypotheses at the $p < .000000000000000001$ level and then deciding that the results were not due to chance and therefore, it is likely that the quark exists. Rather, they make very specific and falsifiable predictions.

It is just as naive and incorrect, and here I am perhaps being a bit ad hominem and aggressive, of Dr. Palmer to write in his review, in this same section that:

"It should also be evident from the above definition [i.e. that psi is a statistical departure of results from those expected by chance etc.etc.etc.] that Alcock's assertion that the so-called "psi-hypothesis" is unfalsifiable is incorrect. It is falsified whenever results from a psi experiment conform to the expected chance distribution."

What absolute rot! This indicates a total misunderstanding of statistical analysis. One can reject the null hypothesis, or one can fail to reject it. However, the statistical process

does not allow one to accept the null hypothesis. After all, we need to know something about the power of a test before we could even begin to evaluate the likelihood that the non-rejection means anything at all. Does Dr. Palmer mean to suggest that he is unaware of all this? Unfortunately, I must admit that many in my own profession, psychology, act as though they too are unaware of this. As for the assertion that this makes the psi hypothesis falsifiable, it just is not so. Can I prove that Santa Claus does not exist by showing that he's nowhere to be found in Toronto or New York? Of course not. Unless you can tell me how to demonstrate that Santa does not exist, then the Santa-exists hypothesis is also unfalsifiable.

Further, it always amazes me to see the extent to which post-hoc data-probing can rescue what appears to be uninteresting data; one may look for displacement effects, or one may talk about the possibility of experimenter effects, or one may posit something else. Tell me, Dr. Palmer, about how to set up a test to demonstrate that psi does not exist, if indeed it does not.

Now Palmer goes on to explain that psi is only a descriptive concept, not an explanatory one, that since Schmidt found departures from chance in his cockroach experiments, by saying that psi was operating, he was simply giving that departure from chance a label. What nonsense! He goes on to say that Schmidt's speculation that the cockroaches might have been the source of this departure from chance was "based on implicit theories intended to explain an aspect of the ALREADY ESTABLISHED psi effect." So, Palmer implies, Schmidt simply reported a departure from chance and then speculated, or resorted to a "theory" (which, pray tell?) to point to the cockroach as the cause of the statistical departure, which, incidentally was in the opposite direction to that which Schmidt predicted.

Dr. Palmer has the audacity to state that I am often confused about this point, and gives as an example my comment that Schmidt's speculation that the experimenter rather than the cockroaches in his cockroach study might have been the source of the significant psi result is unfalsifiable. Tell me, Dr. Palmer, just how it might be falsified. Psi, we are told, knows no bounds of time or space; it is not affected by shielding. How do we ever find out, if psi exists, whether or not it is Schmidt or the cockroaches that have the putative psi. Even if the effects aren't found without Schmidt around, one could argue that the cockroaches have psi, but only use it when Schmidt is there, etc etc.

In footnote 12, Palmer tells us that he does not like the term "psi-hypothesis" because "it implies that one is explaining an anomaly rather than merely affirming it." Tell me, Dr. Palmer, what you meant when you used the term "ESP-hypothesis"

(p71, Advances in parapsychological research II). Any one tempted by your rhetoric in this instance should read some of your writings (eg your chapter in Advances II) and they will readily see that you use words such as ESP, PK, precognition (all manifestations of "psi" as far as general usage in parapsychology is concerned) in a reified way.

I stressed in my book that it is not one's religion that is at issue (if Scientology is a religion). What is at issue is that a set of results which are not replicable by skeptics, and not even by some believers, were produced by people with a belief system which includes commitment to psychokinesis and astral projection. We shouldn't do anything to interfere with their right to do research or to write or to speak; we should simply insist that we want independent replication of their results, (just as we should be insistent with other people as well, for that matter). In general, researchers cast a more jaundiced eye at research carried out by people with a vested interest, theological or otherwise, in the outcome, at least when the results lie in the direction of this interest. We would be unlikely to accept the claim made by the Maharishi that he can teach people to levitate or to become invisible if the only studies which bore this out were carried out by his followers.

Surely it is an injustice to those who really suffered at the hands of McCarthy to bandy about the term "McCarthyism" in this context. In no way do I wish to see anyone muzzled; all I am saying is that in order to assess an individual researcher's impartiality, which is particularly important when replication poses such difficulties, one needs to consider the individual's belief system as demonstrated by prior behaviour and writing. If I claim to have once gone for a ride in a UFO, serious UFO investigators might be expected to be more cautious in their interpretation of a subsequent report by me that I saw another UFO, even though both reports might have been accurate.

ISSUE #11: THE PSI CONSTRUCT

Palmer's definition of psi is a rather curious one. First of all, if psi is only defined in the way that he does in this instance, then it is much easier to argue that psi exists, for he equates it to a statistically significant departure of results ... etc. Then all we can say is that parapsychologists and sceptics tend to differ in the explanations offered for these statistical departures, the former suggesting that some new kind of energy, or some new principles unknown to contemporary science (with the possible exception of quantum mechanics) are involved. The latter suggest that such departures more likely reflect experimental or statistical artifact.

I do hope that Palmer intends to stick with this definition. How the miracle-workers, the key-benders, etc., can fit their

craft in with this definition is not obvious.

ISSUE #12: BUNDLE OF STICKS

[I am running out of energy, as I go through Dr. Palmer's diatribe, and so my comments shall be shorter and fewer from here on in]. Dr. Palmer uses my article on critical belief as an example of what one might do if one wants to hold a study up to ridicule. Unfortunately, again he has missed the most serious kind of concern - the question of construct validity. He discusses methodological questions, while were I in his shoes I would ask, "How do we know that the scale that is supposed to measure critical thinking really does so?" Is there indeed such a trait, and can we expect the way in which a student responds to a series of test items to really reflect his/her ability or inclination to examine issues in a critical way in general. In psychology, particular in the "softer" areas, we are very lax about such things, and unjustifiably so. Therefore, I would say mea culpa to the charge that my study can be criticized. However, I am not trying to demonstrate the existence of some new power or energy or mind-force, and thus no great damage to our total pursuit of truth would occur even should the conclusions of my study be incorrect. However, if we accept that there is evidence that, for example, the soul leaves the body and floats around the room, the implications of such a claim, if true, are enormous for science and humanity, and thus one needs be ever so much more concerned about the quality of the study and the quality of its evidence. In an area such as parapsychology, where there is a vital question about the existence of the phenomenon, rather than a question about how accurately we are measuring it or whatever, we cannot be so casual as we might be in mainstream psychology, since much more is at stake, in terms of our understanding of the world.

I must say that I am rather surprised that Dr. Palmer springs to the defense of the Tart out-of body (OBE) study. This study deserves to be "exposed" because it is discussed so much by people trying to convince others of the reality of OBE's, and because while Tart originally attached all sorts of caveats to the interpretation of his results, these caveats disappeared by the time he discussed the results in his book. I do not think that this study of Tart's merits ANY weight in the discussion of OBEs, simply because either we have something absolutely remarkable going on, or we have someone cheating. Since Tart admits dozing off during the sessions, and since he admits that there was opportunity for cheating, then this "study" of his is without any usefulness at all. Suppose that we were to consider the merits of a study of someone who claims to live without ever eating (there actually is such a person at this time, a woman who goes about giving talks about the nourishment of the spirit, etc.). If the authors of the study state that for five days the woman was observed to eat nothing at all, although

they realized later that they had dozed off for a few hours each day, and it might have been possible for her to have brought in food in her handbag, although that was deemed unlikely, would we really be wise to modify upwards our subjective probability that people can exist without need for food? As for the bundle of sticks approach, it surely would not matter how many studies of how many women or men have been done; if each study is marred by serious methodological flaws, I can't see that one is really going to accept the proposition that, well, "there must be something to this idea that one can exist without food, because, although individually weak, the collectivity of studies surely points in that direction." That strikes me as hokum.

ISSUE #13: METAPHYSICS

Science is in a sense like a cauldron of competing theories about nature. Over the years, scientists have come to more or less agree on a number of criteria which are important and useful in adjudicating competing claims while at the same time helping us to protect ourselves from individual and collective self-delusion. I recall hearing how, when wildlife officials destroyed the wolf population in order to protect a deer herd on a northern Canadian island, they were chagrined to find that the whole herd eventually perished because its numbers grew greater than the capacity of the environment to sustain it. Without the wolves to weed out the weakest, they all died. So it is in science, I would argue. Criticism is absolutely essential in order to give truth a chance to emerge from a morass of speculative hypotheses and often conflicting data.

Dr. Palmer states that my book is an excellent illustration of the problem of contamination by metaphysical bias - (should I be surprised? It seems I've done everything else wrong!). I hope very much that I am not as dogmatic as he would have the reader believe. I do regret the sentence that introduced my chapter 2 which he cites in his review, for he is correct in stating that it seems to imply a strong bias against religion. That is unfortunate, for I respect not only people's rights to their religious beliefs, but I also think that such beliefs have inspired many people to do a lot of good for humanity. What I had wanted to indicate was simply that religion has been and remains one of the very most powerful influences in human society, and is capable of pushing people to irrational extremes. True, other belief systems such as politics can do the same, and indeed extreme political movements have much in common with (some) religious movements.

He argues, naturally, that I am intolerant of ideas which go against my worldview. I would respond that I am intolerant of those who wish to clothe themselves in the mantle of science, yet want the luxury of rewriting its rules when it suits them.

ISSUE #14: DOES PSI CONTRADICT SCIENCE?

My view of this is simple: Either psi exists or it does not. If it does then any science which denies it is at the very least incomplete. If it does not, then a great deal of time, energy, emotion and money is being wasted. If psi exists, its ramifications for science will be staggering, hence the cautious approach that scientists take in their evaluation of parapsychological claims. I am not opposed AT ALL to the study of psi; I would just like to see some agreement as to when one is willing to say that enough is enough-- in other words, by what criteria we decide that the case is so weak that we give up the quest, at least for now, instead of trying to proselytize and attract new believers to the fold? I see no possibility that parapsychologists will ever be able to say to themselves that they were wrong, that they were on a false scent. The various mechanisms that they have developed for dealing with absence of results prevents them from ever deciding that psi does not exist, IF it does not. (Actually, I should be more precise: some erstwhile parapsychologically oriented people, such as Antony Flew, Christopher Evans and John Taylor have radically reversed their views and become skeptics. This happens only rarely, though.)

With regard to Dr. Palmer's conclusions, I do not agree that modern parapsychology is more influenced by quantum mechanics than by magic. In my view, parapsychologists have seized upon quantum mechanical paradoxes in an effort to persuade themselves and others that the notion of psi might be accommodated into mainstream science, and very advanced mainstream science at that. What quantum mechanics could possibly have to do with macro level behaviour is not clear, although Dr. Palmer's suggestion about quantum mechanics and key-bending, described earlier, may convince the unsuspecting reader of its relevance. Should we also try to explain how some people, following the Maharishi, can supposedly levitate themselves, or become invisible? Maybe some of the paradoxes of quantum mechanics can give hope in that area as well.

Why Dr. Palmer comments that there is "no justification for censorship" is beyond me. There is nothing in what I wrote which would even suggest such censorship. Just the opposite: I wrote that parapsychologists have every right to bring their ideas into the arena of scientific debate. They simply have to be prepared to accept, graciously if possible, the scars of battle. Who knows, perhaps they'll win one day. I certainly doubt that, but such can't be ruled out. The evidence that parapsychologists have presented to date certainly does nothing to lead me to suspect that there is any such thing as psi. Despite what Dr. Palmer would lead the reader to believe, I am still willing to consider new evidence, but it should be clear to all who read my book that I feel the onus is on the parapsychologists to clean up their act, to stop playing both

sides of the street. Either they wish to use a scientific approach or they do not. They cannot claim the former and then plead that their subject matter demands relaxation of the rules of evidence.

CONCLUDING COMMENTS

I am extremely disappointed by the tone of Dr. Palmer's review, just as I am disappointed by, for example, his willingness to ridicule and trivialize my discussion of statistics despite his apparent lack of statistical savvy. (If that is ad hominem, I believe that the often-ad-hominem nature of his review should give me the right to be overlooked a couple of times in that regard).

My overall request of Dr. Palmer is to tell me how I might find out if psi does NOT exist. Now, I know that we cannot "prove" the non-existence of something, but we have given up believing in werewolves, mermaids, demon possession (well, most of us, anyway...). By what criteria might we be able to decide that the evidence is not there, that the likelihood of the existence of psi is too low to bother about?

One of the major problems with demons, soul travel, and so on is that once you let such constructs into science, it's all but impossible to drive them out again, even if they are not needed. If we accept that the soul exists and can leave the body, then that becomes a powerful and parsimonious explanatory device for all manner of experience that might otherwise yield to "naturalistic" explanation, if we were to search for such explanation. For example, we are now being told that there is "scientific" evidence which seems to indicate that when one is near death, one's personality leaves the body and travels to another domain, where dead friends and relatives are encountered. If one accepts that, then perhaps sudden infant crib death, a contemporary medical enigma of some concern, might be best understood in terms of incomplete attachment of the soul to the body. I am not being facetious. Science is reluctant, I think to admit new constructs until it is certain that existing constructs do not suffice. For parapsychological constructs to win acceptance, it must become very clear that the phenomena they are used to explain cannot be reasonably explained using the constructs already within science. I would think that the ramifications of relativity theory, what with curved space and so on, were very difficult for many scientists to accept at first, for relativistic hypotheses seemed so counter-intuitive in many cases. Yet relativity won a central place in scientific thought because this theory was testable, and it was more successful than competing theories in accounting for observation.

I want to close in saying that in retrospect, I would have liked

to have had Dr. Palmer's lengthy review before my book went to the printers, for I believe that the book could have benefitted from some of the things he had to say. He has said nothing to lead me to change my basic evaluation of parapsychology, however. I would have been more impressed had he had the strength to do what Robert Morris did in his review, and that is to rise above hurt feelings and get on with the business of discussing the issues related to science and psi. Morris' approach is likely to open doors, to foster dialogue between skeptics and believers, while Dr. Palmer seems to want to slam the door shut. (I must admit that I gave that door a pretty hefty kick myself!) Perhaps through this exchange in Zetetic Scholar, we can begin to pry the door open again, at least between ourselves.

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A REPLY TO DR. ALCOCK

JOHN PALMER

Dr. Alcock's reply features the second of the three rhetorical characteristics I had ascribed to his book: "Rhetorical hyperbole camouflaging specious or vacuous arguments" (p.37). The emphasis this time is on "vacuous". It is fair enough for him to try to score debating points and win sympathy votes by attacking my sarcasm (which he asked for), but when this becomes the tail that wags the dog the more sophisticated reader is shortchanged. For the most part, Dr. Alcock simply restates the same arguments which provoked my criticisms in the first place. He ignores a great many of my substantive points, and several of those he does address he distorts. Regarding time, I might mention that Dr. Alcock had my paper for four months before submitting his response. If he wants to be an effective advocate for the "skeptical viewpoint" with thoughtful people, he must be willing to expend as much "energy" in defending and elaborating his arguments as he did in propounding them.

Indicative of the superficiality of Dr. Alcock's approach is his frequent citation of quotes (carefully selected to support his viewpoint) as a substitute for logical argument. On p.74 he even brags about it. It is amazing to see someone who so loudly proclaims the virtues of reason be repeatedly more concerned with the authorship of comments than with their defensibility. For example, neither in his book nor in his reply does he evince the slightest interest in the ARGUMENTS Braude used to support his strong conclusion against the observational theories, nor in what the observational theorists might have to say in response. This tactic does not promote rational debate.

I will respond to each of Dr. Alcock's "issues" in order, according to their numbers. I will occasionally simplify the titles or replace them to better reflect the subject matter under discussion. I have nothing to add to my previous remarks on Issues 9, 11, and 13.

ISSUE 1: MAGIC AND SCIENCE

Dr. Alcock wastes no time launching into his favorite theme, the "magical beliefs" of parapsychologists. Alright, let's talk about "events occurring on the simple basis of one having wished them" (p.72). Let's pretend for a moment that parapsychology has come of age. Assume that conditions have been specified under which a reliable relationship has been shown to exist between a subject's mental state of "wishing" and the behavior of a random event generator. Assume that the degree of wishing has been operationally defined both behaviorally (self-report) and physiologically. Assume that this relationship can be defined with great precision by linear equations with mathematical constants accurate to five decimal places. Assume further that a network of theoretical constructs exists which consistently allow accurate predictions to be made about the effect of externally applied influences on this relationship. Finally, assume that no "causal chain" (p.72) has been discovered for this relationship or even been credibly proposed.

Surely Dr. Alcock would have to agree that this qualifies as science, even though the relationship in question still represents what he has been calling magic. If parapsychology is pseudo-scientific, the "magical" nature of its hypotheses is not the reason. Dr. Alcock gets a great deal of rhetorical mileage out of the negative connotations of the word "magic". A deeper analysis reveals that the distinction between magic and science is not nearly so fundamental and absolute as he suggests in his book.¹

I am delighted to see Dr. Alcock admit, however grudgingly, that his central beliefs, like everyone else's (even mine!), are resistant to change. The more relevant question, how much the thought processes of each of us in our analyses of parapsychology have been contaminated by these beliefs, is something I am quite content to let readers judge for themselves.

ISSUE 2: PROCESS-ORIENTED RESEARCH

It is not clear from his reply whether Dr. Alcock wishes to stand by his statement that "The bulk of the parapsychological literature continues to reflect an obsession with trying to demonstrate that psi occurs" (p.75). In any event, he takes a significant step in undermining this claim when he refers to some number of parapsychologists who he says argue that "the existence of psi is no longer at issue, and that attention now needs to be directed to exploring its characteristics" (p.72). Whatever number he had in mind would be considerably amplified if it included those parapsychologists who, like myself, accept the second clause of this statement but not the first. I refer anyone who might agree with Dr. Alcock that theory and process-oriented research (which he elsewhere criticizes us for lacking) should await adequate proof of the phenomena to p. and to a paper I wrote on the subject (Palmer, 1973). I would be glad to send a copy to anyone interested.

I am in general agreement with the statements of Stanford and of Johnson which Dr. Alcock quoted. I stated in my paper that parapsychology was "just beginning to mature" (p.45) with respect to theory and that "followups are often not as incisive or extensive as one might like" (p.43). I feel that a careful study of these quotes reveals that they in no way undermine the charges which I leveled at Dr. Alcock. As Stanford points out in his own highly critical review of Dr. Alcock's book, "Despite [my] remarks about a need for more systematic research, it would be misleading to conclude with Alcock that this field lacks such research. Research of a process-oriented character has occurred throughout the history of experimental psi research" (Stanford, 1983, in press). Moreover, Stanford (personal communication) agrees with me that the level of sophistication of such research has improved since he wrote his Presidential Address in 1973.

The point of my remarks in this section of my paper was to

¹Since Dr. Alcock claims to know so much about physics, perhaps he can explain to us what "causal chain" is involved in gravity.

show that Dr. Alcock has misrepresented the nature of modern parapsychological research through biased selection of references and misleading generalizations. I got the sense from reading his reply that his strategy was to more or less concede my points while trivializing their importance. Let me say in this connection that I consider misrepresentation of this magnitude to be a serious matter, especially when it occurs in a book written by a supposedly authoritative tenured professor at a major university and is likely to be considered a definitive text on parapsychology on many college campuses. The impression one gets of psi research from reading Dr. Alcock's book is of a random series of mindless attempts at miracle-mongering, with no interest whatsoever in experimental designs, understanding the process under study, or discovering factors that might limit or otherwise affect its manifestation. This is a major distortion of the record, even granting what Stanford and Johnson say.

It is also very important to note that a major objective of Dr. Alcock's book was to show that parapsychology meets all eight of Bunge's criteria of pseudo-science, among which are included lack of testable hypotheses and no overlap with other fields of research. If he had reviewed the literature fairly, the most Dr. Alcock could have credibly concluded on these points was that parapsychology represents poorly developed science, or perhaps proto-science. By misrepresenting the research as he did, he was able to appear credible in saying that parapsychology is pseudo-scientific in these respects.

I have no doubt that Dr. Alcock could have discussed process-oriented research in a way that "would have strengthened [his] criticism" (p.75). Based on the way he handled the research he did address, I must say, in retrospect, that the truth was probably better served (in a relative sense) by his not having made the attempt.

ISSUE 3: QUANTUM PHYSICS

Dr. Alcock is obviously eager to get back at me for my nastiness, and what better way than to take a few shots at my Presidential Address to the Parapsychological Association, in which I was brash enough to suggest that quantum physics might have some relevance to parapsychology. He quotes several sentences of my address out of context to support his claim that I was trying to propose "how key-bending might be explained quantum-mechanically" (p.78), which indeed would have been presumptuous for a psychologist. Had Dr. Alcock wanted to be fair, he might have quoted the following sentences as well, which would have put the remarks he does cite in perspective.

"Quantum physics is not of value to contemporary parapsychology because it gives us a real or imagined ally in the battle with our critics. It is not valuable because it yet explains psi in any satisfactory way. It is valuable because it has inspired us to generate sophisticated models and conceptions of psi that are radically different from those of the past. In short, its value has been primarily heuristic, and it is from this standpoint that I wish to approach it" (Palmer, 1979, p.190).

And later:

"Again, let me stress that I am not proposing a 'quantum theory' of psi, but using quantum theory instead as a heuristic device for the development of models that are appropriate for parapsychological data" (p.191).

My point simply was that since quantum physics deals with the physical world probabilistically, it might be able to inspire mathematical models that could deal simultaneously with the existence of psi and its elusiveness, as well as account for possible macro-events by summation of micro-events. I readily admit that my "key-bending" model, which I really intended as a metaphor to get my idea across, was crude, and a practically useful and testable model along these lines obviously would have to be developed by someone with a more extensive background in math and physics than I (or probably even Dr. Alcock) possess.

Nevertheless, I refuse to accept Dr. Alcock's implicit suggestion that the development of such models is inappropriate scientific activity. I also stoutly defend the right of any scientist to "draw inspiration from the wonderful and wierd world of quantum mechanics" (p.78) or from anywhere else, even (God forbid!) from magic. The treatment rendered my address is vintage Alcock, and I am glad that the reader who has not seen his book has been given such a good illustration of the kind of misrepresentation, condescension, and censorship of ideas which drove me to write a response.

It was my perception of just this kind of intolerance on Dr. Alcock's part which prompted the criticism which he addresses at great length at the beginning of this section of his reply. His discussion only serves to reinforce my point. He tries to sound open-minded by stating that "Of course, if quantum mechanics is really relevant, then it should be discussed" (p.77), but this quote is later negated when he finds the magically fatal quote of Braude sufficient grounds "to recommend against treating [the observational theories] as the central focus of any discussion on the subject" (p.79). Okay, let us grant that we still lack an adequate quantum mechanically-based explanation of psi. Does that mean we should give up the effort? (After all, even Dr. Alcock acknowledges that "We do not know how ... [certain] paradoxes [in quantum physics] will be resolved" (p.78)). Is it not Dr. Alcock who complains that parapsychology is not adequately integrated with the rest of science? Or does he only want such integration if it is on his own terms? I agree that one must be cautious in drawing implications from quantum physics, and I deplore sensationalization of paranormal claims in the media just as much as Dr. Alcock does. His breast beating on these points is just so much diversion. The issue BETWEEN US is Dr. Alcock's manifest attempt to deny legitimacy to quantum mechanical thinking in parapsychology.

ISSUE 4: CRITICISM WITHIN PARAPSYCHOLOGY

The first sentence of my section on "Criticism Within Parapsychology" (p.44) begins, "Alcock concedes that parapsychologists do criticize each other's work ..." Dr. Alcock notes in his rebuttal

that "I did not suggest that parapsychology lacks criticism" (p.74). He thus answers a charge I did not make and ignores the charge I did make, which concerns his denial of TOPIC-SPECIFIC criticism.

ISSUE 5: THE EXPERIMENTER EFFECT

It is amusing to see that Dr. Alcock had to go all the way back to 1938 to dig up a quote to more or less support his contention that parapsychologists abuse the experimenter effect (EE). The rest of his discussion on this subject is sheer obscurantism. There is a big difference between a.), simply saying that experimenters who cannot get results are not psi-conducive and leaving it at that, and b.), acknowledging the replicability problem which the EE implies, developing testable hypotheses to account for the EE, and then setting about testing these hypotheses, which, IF confirmed, would strengthen the evidence for psi and might eventually lead to an improvement of the repeatability. The former is pseudo-science; the latter is science. I submit that it is the latter which better represents the attitude and behavior of MOST parapsychologists.

Finally, although there is evidence that experimenter attitudes are correlated with the outcome of psi experiments, this relationship is by no means an established fact, and researchers identifiable as not being "believers" have published positive results of such experiments (e.g., McBain, Fox, Kimura, Nakanishi, & Tirado, 1970). I would encourage other fair-minded scientists who have the fortitude to face the social stigma involved with reporting positive results in this area to conduct and report the results of their own psi experiments, whatever the outcomes.

ISSUE 6: IGNORING "NORMAL" INTERPRETATIONS

On p.73, Dr. Alcock quotes and then attacks several sentences of mine on p.56, where I argue that formal control conditions might not be the most appropriate way to deal with all kinds of experimental artifacts. I can see that standing naked these statements might look like an apologia for sloppy methodology, and Dr. Alcock milks this fact for all it is worth. I refer those who would like to pursue the matter more deeply to the one sentence in this paragraph which he does not quote, where I cite a paper of mine which explains in detail my reasons for taking this position with respect to the demonstration of psi anomalies. I would be glad to send a copy to anyone who wants one.

On p.80, Dr. Alcock reaches his crescendo of bombasticity in attacking the comments I made in the section of my paper entitled "Ignoring 'Normal' Interpretations" (pp. 49). The point beneath all this hot air is such an obvious distortion of what I meant that I feel somewhat guilty consuming valuable journal space in refuting it, but some things must be done for the record.

Like any other scientist, a parapsychologist tries to anticipate all possible artifacts and to the extent possible design his or her experiment in such a way as to rule them out. These precautions are discussed in the "Method" section. Only if these

procedures are considered inadequate or questionable, or if a breach of protocol occurred during the experiment, is it necessary to deal with them further in "Discussion". (Nonetheless, such additional discussion occurs more frequently than I perhaps implied in my previous paper.) It is always understood (unless the researcher is stupid enough to claim that the experiment is "conclusive") that there could be other interpretations of which the researcher is not aware, but this pro forma disclaimer is not customarily included in research reports. Such an omission does not imply that the researcher thinks he or she is omniscient. Researchers are responsible for addressing all potential artifacts of which they are aware, and unless Dr. Alcock means to suggest that researchers be required to discuss the possibility of their own dishonesty or incompetence, I think that MOST parapsychologists who publish in our leading journals discharge this responsibility rather well.

The purpose of my original discussion was NOT to suggest that parapsychologists think they are omniscient or that they never overlook possible artifacts, but to challenge Dr. Alcock's insulting and unsubstantiated insinuation that we intentionally suppress discussion of normal explanations of which one could reasonably infer we were aware. If this was not his intent, perhaps he will explain to us in his reply how researchers are supposed to discuss interpretations of which they are NOT aware.

ISSUE 7: AD-HOMINEM ATTACKS

I obviously struck a nerve with my reference to Sen. McCarthy. Of course, any analogy breaks down if it is pushed far enough, but I think this one still has merit. If Puthoff's employer took the logic of Dr. Alcock's "skeptical approach" seriously, he would fire Puthoff on the spot as unqualified to undertake the research he is payed to perform, simply by virtue of his alleged involvement with Scientology.

But what really prompted my analogy was Dr. Alcock's playing fast and loose with the facts. Scientology has a very unfavorable public image, and linking someone's name to it can have much more damaging impact than linking it to just any wierd religion -- or even to the English profession! The quote Dr. Alcock cited from Hyman is in fact the one I was referring to in my paper, and it does NOT support Dr. Alcock's claim that Puthoff is a "practicing Scientologist". Now some might think I am being picky here, based on reasoning like, "Puthoff was involved with Scientology in the past and he is often seen in the presence of known Scientologists, so he must be a Scientologist", but my point is that when someone's professional reputation is at issue, undocumented presumptions should be scrupulously avoided. Yet Dr. Alcock refuses to retract or even qualify his reference to Puthoff as a Scientologist, even after his error has been pointed out to him. I find his complete insensitivity to this issue distressing.

Modern science, not at all to its detriment, has never sanctioned discussion of a scientist's personal religious or metaphysical beliefs and/or affiliations as legitimate criticism, whatever their relation to the scientist's research and whatever the

replicability of that research. As Dr. Alcock himself admits on p.85, replicability is necessary REGARDLESS of the scientist's beliefs. Dr. Alcock's treatment of Puthoff is a good illustration of why this policy of modern science has been a wise one.

ISSUE 8: LITERATURE BIAS

Of course! Critics have to refer to the literature they criticize in order to criticize it. Summing it up in a reading list (as Dr. Alcock did) is nice, but hardly proof of objectivity. If he is referring to something more, it is not clear from his remarks.

ISSUE 10: STATISTICS AND FALSIFIABILITY

If I have missed the point of Dr. Alcock's exercise in logic, I must confess that I am still missing it. I thought I had addressed his arguments in my paper, and the complete lack of any reference to these remarks in his reply makes me wonder if he missed my points. He begins by noting on p.83 that statistical significance "is not descriptive of the magnitude of the effect." True enough, and perhaps Dr. Alcock could even provide examples of parapsychologists having made this silly mistake, but I fail to see what relevance this has to the issue at hand, which I thought was the appropriateness of a statistical model prescribing the acceptance or rejection of a null hypothesis at a prespecified alpha level, i.e., the existence of an ostensible psi effect rather than its size. Dr. Alcock then goes on to accuse me of suggesting "that an ESP hypothesis is supported just because a chance model is rejected", when I clearly stated in my paper that parapsychologists only accept an ESP hypothesis when the chance model is "appropriately" rejected AND "reasonable precautions have been taken to eliminate sensory cues and other experimental artifacts" (p.54).

Next, Dr. Alcock appears to tackle one of my two points under the heading "Other Statistical Nonsense" (p.57). Since his remark does not in fact address my point (which is related to the point I made in the beginning of the preceding paragraph), I can only conclude that the nonsense remains nonsense.

Dr. Alcock then becomes "a bit ad hominem and aggressive" (as if he had not been so before) and challenges my statements regarding the falsifiability of the psi hypothesis. He first reminds us that "the statistical process does not allow one to accept the null hypothesis" (p.83) and then goes on to demand what he just said was not allowed by asking for "a test to demonstrate that psi does not exist", which he later (p.87) says is impossible. I must say that I found this whole section on statistics to be extremely muddled.

However, there does seem to be a point buried here. There indeed are many ways to escape "falsification" if the null hypothesis fails to be rejected. Appealing to a lack of statistical power of your test is only one route. In some cases, you can argue that the experimental manipulation did not work well enough to create the necessary conditions for the effect to appear. Dr. Alcock, in the

second experiment described in his now famous paper with Otis, gets out of it by simply dreaming up an alternate interpretation consistent with his hypothesis and then saying that "further research" is needed (Alcock & Otis, 1980, p.282).² This kind of thing occurs in science all the time. Individual falsifications are rarely considered fatal, especially in those sciences which depend upon statistical evaluation of evidence.

So in what sense can I claim that the psi hypothesis is falsifiable? An investigator sets up a psi experiment which he or she thinks is sufficiently powerful, both experimentally and statistically, to detect psi. The researcher predicts that psi will manifest in a certain way, usually represented by some significant departure from a "chance" model. If this fails to occur, he or she concludes that the psi hypothesis has been falsified. In other words, the hypothesis is formulated so as to be amenable to critical test, and such a test is carried out. But this is not enough. The investigator must also acknowledge the falsification as a strike against the hypothesis as well as requiring limitation of its generality. As I have said before, I think MOST parapsychologists abide by these latter criteria.

Admittedly, unless it can be shown that the number and distribution of significant psi effects do not depart from what would be expected from a chance model, application of this falsification criterion is unlikely to ever kill off the "psi hypothesis" entirely, which is what Dr. Alcock is really interested in. I think this latter issue is best addressed outside the context of falsification, so I will postpone a discussion of it until later.

Dr. Alcock then labels as "nonsense" (p.64) my distinction between descriptive and explanatory constructs in parapsychology. (Incidentally, the same distinction was arrived at independently by Hovelmann in his paper which appears elsewhere in this issue of ZS. I will have more to say about my distinction in response to this paper.) Homing in on my discussion of Schmidt's cockroach experiment, Dr. Alcock tries to argue that any hypothesis about the source of psi in this experiment (or, I presume, in any psi experiment) is unfalsifiable because of the assumed ubiquity of psi. In response, I should note first of all that not all parapsychologists agree that psi "knows no bounds in time or space" (e.g., Otis, 1956). But even if psi were to know no bounds of any kind in PRINCIPLE, it does not follow that such bounds do not exist in FACT. If psi does exist, one is literally forced to assume limits to account for the rareness of its manifestation. Indeed, such constraints have been demonstrated time and time again in all those process-oriented experiments which Dr. Alcock finds so unimportant. It is these constraints which allow testing of hypotheses in parapsychology that go beyond the mere "existence" of psi. In the Schmidt case, for example, one could test predictions of the sort that, if Schmidt indeed was the source of the psi, results in future studies should covary with Schmidt's mood or

²Readers of Dr. Alcock's book only learn of the first, successful experiment.

psychological state. As I noted above, one can always escape individual falsifications of such predictions, and Dr. Alcock is technically correct that the "experimenter psi hypothesis" can never be conclusively disproven. But if predictions based on this hypothesis were to be consistently falsified relative to predictions based on competing hypotheses, the former would eventually be abandoned. This is how science operates in the real world.

ISSUE 12: BUNDLES OF STICKS

Given all Dr. Alcock's blustering both in his book and in his reply about parapsychologists wanting to change the rules of evidence (which I deny), it is noteworthy to see him propose that more rigor must be applied in evaluating psi experiments than research in more orthodox areas. In his book, he gloated that the evidence for psi could be demolished using the criteria of orthodox science. Now he seems to be conceding that at least in this respect it is orthodox science which must plead for a change in the rules (or at least in the application of the rules) when psi data are at issue.

The purpose of my lengthy example in this section of my paper was neither to defend Tart's study as being good nor to attack Dr. Alcock's study as being bad, but to illustrate how the rhetorical tactics used against Tart could be used to effect against virtually any social science experiment. I concede that the force of my illustration was diminished by Dr. Alcock's selection of such a relatively easy target. It would be interesting to see him apply his axe to the studies cited by Beloff (1980) as being particularly evidential or to some of the better process-oriented work, but his glib comments on Beloff's paper (Alcock, 1980) suggest that the outcome would be essentially the same. Indeed, since he would have to conclude that all these studies are as "worthless" as Tart's to support his contention that there is no evidence for psi, I consider it axiomatic that he would use the same tactics against them as he used against Tart. THAT is why I brought the issue up.

I accept Dr. Alcock's stated reason for choosing Tart's study as the single experiment to evaluate in depth. Nonetheless, the fact remains that he did nothing to remove the implication, in fact he clearly left the implication, that this study is representative of the degree of methodological rigor characteristic of psi research. This impression is even stronger in his reply. Just for the record, leaving subjects alone in a room with a target (especially an unsecured target) is NOT standard procedure in ESP research.

Dr. Alcock completely fails to address the issue of how he can justify his extreme claim that there is no evidence whatsoever for psi. As for "bundles of sticks" per se, his reply reinforces the impression that he considers any conceivable alternate explanation sufficient to render an experiment totally worthless as evidence for psi, a position that would appear to deny DEGREES of evidentiality and to be unfalsifiable. But instead of directly addressing these issues, which are absolutely central to his thesis, he tries to bluster his way through by heaping more abuse on Tart's experiment. This is not a very wise strategy, for if I simply concede (which I will do for the

sake of argument) that Tart's experiment is too weak even to be in the bundle, his case collapses like a house of cards. He is then left having to fall back on the naked presupposition that all psi experiments suffer from what he calls "serious methodological flaws" (p.87). But this presupposition is based on nothing even approaching a serious evaluation of the better evidence. Moreover, he never defines what a "serious flaw" is. It could (and, if necessary, probably would) apply to anything from gross sensory cues to failing to conduct a strip search of one's college sophomore research subjects to look for hidden radio transmitters. Thus the claim is not only unsubstantiated, it is unintelligible. Dr. Alcock's contention that there is no evidence at all for psi, whatever the truth of the matter, is scientifically worthless.

For the case on behalf of a more positive interpretation of the evidence, I refer the reader again to parapsychological publications such as those listed in Dr. Alcock's bibliography.

ISSUE 14: OPEN INQUIRY

Dr. Alcock apparently lacked the "energy" to meaningfully address any of the philosophical points I raised in Part III of my paper, but he seemed to have plenty of energy when it came to my charges that he opposes open-minded inquiry in this area. First of all, to avoid any possible misunderstandings, let us be clear that the issue is not whether parapsychologists have the right to express their views publicly or to conduct psi experiments without being arrested by the police. The issue is the LEGITIMACY of OPEN-MINDED inquiry into psi anomalies within the community of scientists. It is my contention that Dr. Alcock opposes such inquiry in any MEANINGFUL sense.

My charge of "censorship" concerned conceptualization and theory in parapsychology, and I stand behind it 100 percent. It is perfectly clear both from Dr. Alcock's book and his reply that he considers any conceptualizations of putative psi phenomena that do not coincide with his mechanistic-materialistic worldview to be pseudo-scientific and thus scientifically illegitimate. As far as I can tell, this covers any paranormal explanation of such anomalies that ever has been proposed or could be proposed. What is gained by parapsychologists "bring[ing] their ideas into the arena of scientific debate" (p.) if such ideas are simply to be brushed aside with mindless epithets like "magic". "Letting the cranks have their say" is a poor substitute for legitimization of open-minded inquiry.

Earlier, Dr. Alcock says "I am not opposed AT ALL to the study of psi; I would just like to see some agreement as to when one is willing to say that enough is enough ..." (p.88). Now this latter is a rhetorical question if there ever was one. It is perfectly obvious from Dr. Alcock's book that he feels this time has already arrived. If his bankrupt claim that no evidence whatsoever for psi, i.e., no genuine anomalies, have been found after 100 years of investigation, of course it would be time to throw in the towel. Dr. Alcock concludes his book (Alcock, 1981, p.196) by stating in effect that there is as much evidence for psi as there is for Santa Claus. Does he favor scientific inquiry into the existence of Santa Claus? He is going to

have to do a lot more than put "AT ALL" in capitals if his claim to not oppose the study of psi is to look like more than sophistry.

In his "Concluding Comments", Dr. Alcock indeed strikes at the heart of our dispute by asking me, "By what criteria might we be able to decide ... that the likelihood of the existence of psi is too low to bother about?" (p.29) I must say that I have trouble understanding why Dr. Alcock is so obsessed with seeing psi research vanish. Admittedly, the risk that the research will not bear tangible fruit is far from negligible, but that is true of most basic research in science. On the other hand, if "psi does exist" and can be tamed, the rewards would be worth the investment many times over. In any event, the amount of money and resources being "wasted" on psi research, if it indeed is being wasted, is peanuts. I can understand why Dr. Alcock is upset about the media hype, etc., but why the research? Be that as it may, his question is a fair one, and I shall give my answer to it.

First of all, I think we must recognize that what we have here is not the hypothesis "psi exists" competing with the hypothesis "psi does not exist". Instead, we have a set of putative anomalies for which two sets of explanations have been offered. One set assumes that the anomalies can be explained by relatively trivial applications of known laws of nature -- "normal" explanations. The other set assumes that the anomalies must be explained by new laws of nature or by interesting extensions of the known laws -- "paranormal" explanations. The competition is between these two sets of explanations. The competition should continue so long as neither camp achieves a decisive victory, i.e., a compelling explanation of the anomalies. (Note that the burden of proof still falls on psi proponents to make their case. Here the issue is simply continuation of inquiry.)

Dr. Alcock and I agree that no paranormal explanations have achieved this stature. We disagree in the case of normal explanations. In my opinion, with relatively few exceptions, the evidence for these explanations consists of a hodge-podge of ad hoc and often far-fetched counter-interpretations of psi experiments which frequently go beyond the bounds of criticism considered appropriate in normal science and which derive much of their credibility from a childlike faith in the universality of the currently identified laws of nature. Except for the debunking of professional "psychics", skeptics rarely put their own hypotheses to critical test, falling back on one form of the parsimony principle instead. Nomothetic research is all but nonexistent. Likewise, the kinds of explanations put forth to account for spontaneous cases, although superficially plausible, fail to come to grips with the complexity of many of these cases and rarely are "battle tested" by confronting GOOD cases with systematic, incisive research. Presumably Dr. Alcock is impressed by all this; I am not.

As a contrast, consider a case in which skeptics have won a clear victory: "vision" in bats. This victory was not achieved because skeptics succeeded in demolishing the evidence for "bat-ESP", but because scientists were able to provide such a high degree of hard evidence for a normal explanation that any paranormal explanation was rendered superfluous.

Admittedly, this is not so easy in the case of complex human

"psi". This is partly because orthodox science has yet to provide much in the way of compelling explanations of the human mind in general. However, until we can say with confidence that the existing anomalies are explained so well that it is no longer really appropriate to call them anomalies, our theoretical and research options must remain open. This does not mean we must indulge crackpot "Santa Claus" theories, but it does mean we must welcome all scientifically disciplined and potentially testable proposals, irrespective of their metaphysical underpinnings or continuity with the current paradigm. When skeptics have done as well with extant psi anomalies as they have done with bats, THEN perhaps we can talk about closing the books.

AN ADDENDUM ON "TONE"

Dr. Alcock was obviously offended by the tone of my paper, but he refuses to take any responsibility for his own rhetoric which provoked that tone. His "self-defense" consists of selected quotes from other reviews of his book. Barry Singer writes, for example, that "There is no sarcasm and belittlement" (p.71). How he reconciles this statement with Dr. Alcock's language in discussing Tart's experiment (and numerous other concrete examples of his condescending arrogance I could cite -- not to mention the general theme and tone of the book) escapes me. The quote from Morris, which is supposed to be the clincher, is not even relevant. Morris simply chose to ignore Dr. Alcock's rhetorical excesses, a decision which I respect. But I do not need to launch into a long essay. Dr. Alcock's reply speaks more eloquently to this point than anything I can say, even when one takes into account my "rudeness".

Dr. Alcock suggests that my phrase "highly polemical, extremely arrogant, and completely destructive in intent" (p.71) is an apt description of my own paper. It certainly is, and I said as much. Is this "eye-for-an-eye" approach justified? In this case, I think it is. I am well aware that scholarly critiques can get quite acid. But such critiques are customarily written with the understanding that the person or persons criticized are respectable scholars with whom one happens to have strong differences of opinion on certain issues. Dr. Alcock's book is a much different story. When I see a book the central theme of which is to portray a group of dedicated researchers and scholars as fanatical occultists masquerading as scientists, and when I see selective editing and other journalistic devices used to support that characterization, I think I can be excused for feeling some anger -- and for expressing it. If I may be permitted a crude metaphor, when someone calls you a "punk", I do not think it is appropriate (even, I dare say, among academics) to utter a response of the form, "Perhaps there is merit in what you say, but, on balance, I think the weight of the evidence ..." This is not just a matter of letting off steam and "hurt feelings" (p.90). It is essential to demonstrate, by tone as well as by substance, that you consider such condescension to be totally unacceptable behavior unworthy of a dignified response. The only way to put a stop to these tactics (maybe) is to make it clear to those who would perpetrate them that they can expect to be treated accordingly. One does not serve the cause of rationality by reacting like an Uncle Tom to its bastardization. I am confident most ZS readers have enough sophistication not to let the resulting polemics

distract them from analyzing the logic of the arguments put forth by Dr. Alcock and myself, which I hope they will do.

I make no bones about the fact that a primary objective of my paper was to discredit a book, which despite the fact that it contains a number of valid criticisms, richly deserves to be discredited. The idea that I could convert Dr. Alcock to my point of view never even crossed my mind.

One of the very few things that gives me any hope for the future of rational inquiry in this area is the emergence of a handful of fair-minded critics from both inside and outside parapsychology who seek to discuss issues in depth on the basis of mutual respect. The Hovelmann paper in this issue of ZS is very much in that tradition. Another apparent example, a book which ironically comes to many of the same conclusions as Dr. Alcock's, is ANOMALISTIC PSYCHOLOGY by Zusne and Jones (1982). A comparison of the treatment of parapsychology in these two books is instructive. Although my dialogue with Dr. Alcock has served a valuable function by giving substance to the strong differences of opinion that underly the "psi controversy", it is dialogues with persons like those mentioned above that are more likely to lead to constructive resolutions of these disagreements and to changes in a field which I am the first to admit needs them.³

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³Should Dr. Alcock ever wish to join this group, I would be delighted to let bygones be bygones.

A FINAL NOTE

JAMES E. ALCOCK

I am very disappointed that Dr. Palmer found it necessary to continue the same abusive tone that characterized his earlier response. Since readers who have any further interest in my ideas can turn directly to my book, or to the reviews I referred to in my first response to Dr. Palmer, I feel no need to add further to this debate. The defense rests.



A BIBLIOGRAPHY ON FIRE-WALKING

COMPILED BY MARCELLO TRUZZI*

On examining the feet of a Sri Lanka firewalker: "That sole is no tougher than mine. That's no tougher than mine, and yet the man can do it. I don't think I could." After seeing walk completed: "I don't know. It's got me buffaloed.... Well, what I've seen is most impressive."

-- Conjuror James Randi (on NBC-TV's "Magic or Mystery" aired on February 8, 1983).

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* with special thanks for help from Martin Ebon and George P. Hansen who supplied me with many of these items.

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CRYPTO-SCIENCE RIDES AGAIN: A REPLY TO MY COMMENTATORS

RON WESTRUM

When I wrote "Crypto-Science and Social Intelligence About Anomalies" I was preparing it for a conference on the demarcation between science and pseudo-science at Virginia Tech. At this conference, I knew, many astute and sensible arguments about what science really was would be advanced by other participants.¹ I felt, however, that some leavening of this intellectuality would be required, and so I decided (after considerable internal debate) to write the essay reviewed (in ZS #10) by the twenty-three commentators. In it I portrayed some of the "realities" which I have personally experienced as an anomalist as well as some of those experienced by others. I also mentioned in passing some observations I have made on the sociology of anomalous events, based on previous studies² and on a book upon which I have been working for several years.³ The essay format gives one more freedom but also lends itself to a number of different readings, as the varied remarks from commentators show. Nonetheless there were several points that I wished to make, badly expressed as they may have been. I would like briefly to reiterate these, before responding directly to the comments.

First, I wished to stress that the often sub-standard research in the crypto-sciences is frequently a result of the lack of logistic support. This is not an excuse for sloppy work, but simply a comment on the relationship between input and output. Second, this lack of support is a result of the attitude toward the objects studied by crypto-science. Since UFO investigations, etc., are low priority items for the scientific community, they are simply not given resources. The lack of progress in some areas of crypto-science may be due to inadequate support rather than to the intractability (or non-existence) of the objects studied. I did not argue that the crypto-sciences should be better funded, simply that not funding them had predictable negative consequences. Finally I noted what appears to me to be an unnecessary current of hostility toward researchers in the crypto-sciences and suggested that it might be due to the threat such activities pose to officially sanctioned "reality."

In responding to the various criticisms raised about these points, I will proceed in terms of ideas rather than go through each commentator's remarks separately. Although this approach may not do exact justice to persons, it avoids the repetition which otherwise would ensue. I beg the pardon of anyone whom I have inadvertently slighted.

Robert Rosenthal and H.J. Eysenck comment on similarities in the social treatments of anomalous and taboo topics. Rosenthal even suggests that topics which are both anomalous and taboo are likely to have the most problems of all. I completely agree. But anomalous/taboo to whom? Both Piet Hein Hoebens and Dan Cohen note that many anomalies unpopular with scientists are very popular to certain segments of the public; the same may be said of taboo topics. Yet "official reality" is important since it determines legitimacy relative to scientific recognition, funding, and other forms of support. Those indifferent to such legitimacy can of course turn for support

to uncritical mass appeal. But most anomalists are not indifferent to legitimacy. They want both to study their odd objects and to be treated with at least minimal courtesy by scientists. This is an unrealistic expectation perhaps, but it is a very human one.

Who qualifies as a crypto-scientist? Is an astrologer a crypto-scientist? Is a UFOlogist or a psychic? Let me suggest the following definition: a crypto-scientist is anyone who studies anomalous events with the aim of bringing them within the circle of scientific understanding. The crypto-scientist (CS) looks for enigmas to explain them. This is the opposite tack from the mystery-mongering in which many (not all) occultists engage. Thus systematic data of all kinds and alternative explanations are necessarily of interest to the CS. Sonja Grover says quite inaccurately that:

"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."

Actually data compilations are very popular with anomalists, and they certainly do modify their views. To take only a few examples, consider such compendia as the Corliss Sourcebooks or Heuvelmans's In the Wake of the Sea-Serpents. Hendry's UFO Handbook tests and refutes many popular UFOlogical ideas, including the famous "law of the times." Study of the airship wave of 1896-7 has convinced many anomalists that the whole thing was a newspaper hoax. Data collected by UFOlogists is being used to relate UFO reports to seismic disturbances, an explanation that few UFOlogists favor, and which certainly is in conflict with many "basic assumptions" among them. No one familiar with the voluminous technical reports of Michel Gauquelin regarding astrology could agree that "there is no data base with which to refine or modify views within the field." Recent Advances in Natal Astrology further demonstrates this point. CS's are not indifferent to data, but on the contrary respect other CS's who are good at collecting, processing and theorizing from it.

Do CS's have a sense of humor? Dan Cohen suggests that they don't. He says "Damnit, it does sound funny for a grown man to spend his spare time looking for UFOs or Bigfoot." I agree, but stamp collecting or bird watching seem equally odd. People enjoy all kinds of things which, if we stop and think about it, can be viewed as funny. Yet there is something in what Dan says: anomalists do seem to lack a sense of humor about what they do. I think, the reason is that they feel constantly under attack. Maybe they should "try to be a little less defensive, even in the face of hostility," but that is just the problem. CS's could laugh at themselves more easily if they were not being constantly jumped on by outsiders. The removal of the hostility which I spoke about at the beginning of my paper would do much to change this situation.

One can underestimate the humor of CS's. Morris Goran seems to have done this in regard to my remark about Galileo and Semmelweiss. This remark was meant as a joke and appropriately elicited laughter when the paper was first presented orally. On a deeper psychological level, though, the joke may reveal the sense that many UFOlogists have of being pioneers in an uncharted area. A genuinely perceptive psychological study of the motives of UFO researchers remains to be done.

What about the nonsense associated with beliefs about anomalies? Piet Hein Hoebens claims that "for every coelacanth there are a million red herrings". The junk written about biorhythms, astrology, the Bermuda Triangle, etc. is indeed objectionable. Why, Dan Cohen asks, is this nonsense not recognized and dealt with by anomalists? Well, recognizing it is one thing and dealing with it another. Most anomalists I know are none too keen on Berlitz, Van Daniken, horoscopes, etc. I myself have criticized them in my bibliographical review (with Marcello Truzzi) in ZS #2. Yet there is really very little that can be done. CS's are going to be lumped with the Van Danikens whether they like it or not--this recently happened in the essay by James Oberg which won the Cutty Sark Prize. Oberg, who should know better, simply threw J. Allen Hynek in with the National Enquirer stories on UFOs; he was applauded for doing so.

Also, I suspect some anomalists tolerate the junk for much the same reason that scientists tolerate Cosmos or Scientific American--- because it provides persons who may later become supporters.

Today's Berlitz fans may mature into tomorrow's UFOlogists. Many anomalists themselves first got interested by reading the junk. I became interested in the history of science from reading Velikovsky. (Manure may smell bad, but it often makes terrific fertilizer).

Why are anomalists needed? I claim they are needed to give attention to and collate reports of events that would otherwise remain hidden. I argue that anomalous events, if noticed, tend to be kept secret. This secrecy is doubted by Hoebens and Grover. Let me produce only a few examples to show that they are mistaken:

1) Ball lightning was described as a rare event until systematic surveys showed that it was not rare at all. Considering its size and visibility, its frequency is probably within an order of magnitude of ordinary lightning.⁴

2) About one in eight UFO sighters report their sightings. In a previous paper I showed that there were probably 300 sighters for every report in government files.⁵ Now such files are no longer kept, and the sole source for such reports is private UFO investigation. If UFOs, as a recent study by Michael Persinger seems to show, are the result of seismic forces, we will be totally dependent on private files for sightings after 1968.⁶

3) I know of only two cases of spontaneous human combustion reported in the medical literature of the 20th century. One of these reports, by the Professor of Forensic Pathology at the University of Leeds, Dr. David Gee, indicates however, that many physicians observe cases of apparent SHC without putting them in the literature. This phenomenon is so well hidden that one medical historian was unable to find any cases in the literature even though he made a systematic search for them.⁷

4) The battered child syndrome, whose existence was largely unsuspected, became well known in spite of the strong disbelief in it by many pediatricians. However, the development of compulsory legal reporting and protective service agencies have led to a million cases

(approximately) reported every year in the United States. There is every indication, from my interviews with the pioneers of this medical concept, that previous cases had been noted but not reported.⁸ I mention this example simply to show anomalous events are often kept secret.

In these instances we have numerical data to work with, but there are other indications of the suppression of anomalous events. Dr. Grover feels that scientists, at least, would not keep anomalous events secret. Actually the UFO reporting rates of astronomers and other scientists and engineers are only slightly higher than those of the general public.⁹ I personally can produce several examples of scientists keeping UFO observations secret. Another indication of secrecy is the "report release" effect, that the publication or oral presentation of anomalous reports leads to others coming forward with their own previously suppressed experiences.¹⁰ The Coelacanth case is hardly comparable since the carcass was irrefutable evidence of the animal's existence. Observations of crypto-events, however, are seldom of such an unequivocal nature. The reason for my discussion of Smith's discovery of the Coelacanth was to show that even when irrefutable evidence is in hand, such events may be difficult for the observer to accept. How much more difficult they must be when observations take place without such splendid evidence to back them up!¹¹

In these cases at least, we know that most observations of the anomaly were suppressed by the observers, in one instance to such an extent that the anomaly virtually vanished from the pages of medical journals. I did not discuss the meteorite case, since I am heeding Dan Cohen's advice that crypto-scientists should not mention it.¹² Can crypto-scientists make any contribution toward improving the visibility of these hidden events? While Patrick Grim sees crypto-science as essentially useless, I must disagree. He thinks that "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." When we know what they are, he argues, then they will become the domain of one of the already established scientific disciplines. Hence no "genuine science of UFOs, or ghosts, or spontaneous human combustion" is possible. I do agree with Grim on this, for it is precisely the difference between the sciences and the crypto-sciences.

Let me explain. We have learned to deal with uncertainties in mathematics through the establishment of probability and statistics. Similarly, in areas as diverse as particle physics and paleontology, to say nothing of more applied disciplines such as risk analysis, uncertainty of various kinds is confronted and managed. Collection and analysis of data on anomalies may or may not establish their nature (in which case they will indeed be turned over to ordinary science disciplines), but such data can be extremely valuable for someone who wishes to study them. Since it is not unusual for researchers interested in the study of these events to have great difficulty generating their own data bases, a previously established set of data can be of great assistance in guiding inquiry. I see the crypto-scientist as being rather like the explorer who returns from the jungle with a dead animal and

comes to the zoologist with the words "Well, I shot it--you tell me what it is." This work of collecting can profit from scientific guidance in crypto-science just as it does in ordinary exploration, and there is certainly nothing to prevent the scientists personally from engaging in crypto-science.

But isn't this, as Roy Wallis suggests, science as usual? In what respect does such data collection differ from ordinary science? It differs, I would argue, not in terms of underlying logic or methodology but rather in terms of the conditions of research. For reasons mentioned by many of the commentators, particularly Henry Bauer, the "long shot" nature of crypto-science means that it will be given few resources and few scientifically trained researchers. This means that the "science" involved will simply not be the same thing as the science practised by most scientists. Realistically, this means that work in crypto-science will often be undertaken as a hobby, progress will be slow, and literature will be of varying quality, including a fair percentage of junk, since boundary control in these areas will be weak. Anybody can become a sasquatch hunter or a UFO investigator, as the more serious CS's have found to their dismay. Nonetheless, even though the average quality may be low, useful research does get done in these areas, as those who have looked carefully into them can attest. Such research includes explaining many cases which in fact are not truly anomalous. Few people realize that the UFO Handbook, written by the principal investigator for the Center for UFO Studies, Allan Hendry, explained 89% of the UFO cases reported to him.¹³

This recalls another important reason for wanting competent crypto-scientists. Who is going to explain to the family terrified by apparent poltergeist events how they are to understand these experiences? Who is going to sort out for the average person, faced with an ambiguous and disturbing experience, just what it means? Although traumatic experiences related to anomalous experiences are unusual (thank God!), UFO investigators do a fair amount of psychotherapy in the process of investigation. Having carried out a fair number of investigations myself and having taught others to perform them, I feel this important social function should not be overlooked. Since science teachers are frequently called upon to explain anomalous events, I feel that a reasonable familiarity with certain branches of crypto-science (notably UFOlogy and parapsychology) would be helpful for them. Training teachers to deal with the uncertainties of such events would make them more effective in answering their students' questions and would allow them to give more informed answers to queries from ordinary citizens.

I am very much in sympathy with the views expressed by Susan Smith-Cunnien and Gary Alan Fine on the dynamics of professions and especially on the role played by clients. UFOlogy in particular has been able to justify its existence on the basis of service to the non-scholarly community. The character of many UFOlogical activities is a cross between social work and Chatauqua. Although there are, in UFOlogy--as in any kind of investigative work--some terrific cheap thrills, one finds oneself doing many things that are basically therapeutic or educational for people. Comparing crypto-science to chiropractic, as Smith-Cunnien

and Fine do, is particularly apt. One finds the same defensiveness mixed with pride ("but we do accomplish some genuine good") in both groups. And one also finds the same desire to create formal institutions for developing basic theories and legitimating the work of those in the field. There are also vast differences. UFO investigation is a craft, and nothing like the training received by chiropractors is ever given except through one-on-one apprenticeship.

"Amateur science," as Morris Goran points out, has largely taken place in scientific disciplines where there is a coherent body of knowledge developed by scientists. In principle, however, there is no reason why amateur science cannot take place in crypto areas as well. Its character may be different, however, in that the balance of power, at least initially, will be shifted toward the amateurs. As time goes on, however, power is likely to shift to the professionals, a shift which I have seen over the eleven years I have been attending UFO conventions. The shift has been such that there is a counter-movement on the part of "middle UFOlogists" against this growing professionalization. If scientific involvement increases, then there will come a time when many of the manipulations of data will be beyond the ability of the average UFOlogist to comprehend. Furthermore, in addition to barriers posed by lack of understanding, the new scientist professionals may set up organizations in which they not only have leadership positions, but can exclude those without scientific training. This situation, sadly, is already beginning to occur. The Society for Scientific Exploration, of which I am a councilor, has very stringent membership criteria designed to protect its internal processes. While thus protecting its own welfare, it potentially excludes from its ranks persons who have been studying the same anomalies for three decades, and who have laid the intellectual foundations for some of its labors.

I am astonished, I must confess, at Patrick Grim's view of the uselessness of sociology to scientific investigation in basic science areas. Especially so since he feels that philosophy of science can supply normative principles which tell scientists how to proceed! Since my knowledge of the contributions of the philosophy of science to scientific discovery is virtually non-existent, I can only discuss sociology. It is worth recalling the studies by Pelz and Andrews relating productivity of research groups to particular social configurations, analogous to the "five-year rule" for the decline in productivity of R & D groups.¹⁴ Although these studies relate primarily to applied science, I see no reason why similar social-science studies should not be made which will help us understand progress in basic scientific research. But perhaps philosophers of science have developed some principle unknown to me which proves that sociologists cannot contribute to the hard sciences!¹⁵

In any case, however, sociologists do have a definite contribution to make to crypto-science, and that has to do with understanding and changing social behavior related to social intelligence. While I concur

with Grim that sociologists have little to tell physicists about how to approach their quarks, the research matter of crypto-science is often human testimony. How to find out about the distribution of potential observers, strategies for increasing the size of samples, utilization of the press and voluntary associations, these are sociological matters. Equally important are the lacunae in the practices of various social intelligence institutions--the press, the military, the scientific community--which sociological studies have demonstrated. A considerable amount of sociological assumption is involved in much scientific reasoning on anomalies, including many conditional probability statements of the general form "Well, if they saw that, then they would do this..." These statements can be checked against actual case-studies to determine how much they are in accord with what people actually do. As I have shown in the various studies I have carried out, and will do in a more systematic fashion in my book, many statements by influential members of the scientific community on such matters do not accord with human behavior as shown by empirical study. The psychologist Paul Meehl has referred to these as "fireside inductions," and there is no reason to remain content with plausible assumptions when one can check them out.

I am also dismayed that Grim feels that sociology can explain errors in scientific practice but not scientific successes. Evidently, mistakes are seen as sociological, but insights or break-throughs are not. But if a social system can cause pathologies, then it can cause intellectual health, too, and therefore successes. A healthy scientific institution is not simply one which is devoid of the "human factor" but one in which the human factor has been utilized for maximal creativity. Unless one believes that the effect of groups on scientific discoveries is nil, how can it be otherwise?

This same method can be applied to crypto-science. Are some crypto groups more successful than others? If so, what explains the success of the successful? Is it not probable also that the relations of these crypto groups with the scientific community will have something to do with their health? Is it not probable that co-operative efforts between the scientific community and crypto groups will assist in the clearing up of some of the puzzling phenomena? And conversely, that isolation of these groups will negatively affect their productivity?

In several of the commentators, I detect a strain of "sociologists should stick to sociology," in response to my own expressed dilemmas about participation in UFOlogy. Suppose that this is true. But then this leads to the basic question: in whose bailiwick does the UFO problem fall? It is typical of anomalous events that reporting channels are non-existent; there is no obvious body of academics responsible for collating results, etc. The UFO problem is not really a responsibility of the astronomical community, nor of the Air Force, nor of the intelligence agency....The problem with a marked and static division of scientific labor is that our society may be unable to respond creatively to new challenges. If UFOs do represent extra-terrestrial intelligence, then our society is in big trouble. In this respect UFOs differ from almost all other anomalous event, with the possible exception of those related to psi. If UFOs are related to ETI,

then the kinds of scientific "laws" we could develop about UFOs would be extremely limited. The behavior of intelligent organisms is hard to predict, as is shown by the slow progress of the behavioral sciences relative to physical or biological sciences. Furthermore, intelligent life might well display strategic or even strategically deceptive behavior. Whereas ball lightning is indifferent to the presence of human observers, crypto-animals might still be crypto because they are good at evading us (who could blame them?). Highly intelligent life, however, could display deceptive behavior of a very high order. When Einstein said that "nature was subtle, but not downright mean," he had inanimate nature in mind. UFOlogy might well have more in common with Kremlinology than with physics.

I do not mean by this assertion to excuse the lack of success in figuring out UFOs by reference to the strategic deceptions of the latter. But strategic deception is a logical possibility with advanced intelligences, and it would be well to consider it.

Commenting on my remarks about ETI research being channeled into radio-telescope operations rather than UFOlogy, Andrew Neher suggests that "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." Let me suggest, and I trust Neher would agree, that an actual UFO would present less of a "thick overlay of psychological interpretation" than a radio signal. Both UFOlogy and signal detection could yield a definitive answer; what makes the latter seem more sensible to Neher is that he thinks the signals might be there, but not the UFOs. What is "sensible" thus turns out to be what we think is likely to happen. But what makes something seem likely to happen? Why do radio signals seem much more likely to be the form of contact than vehicles or robot probes?

The immediate response has been: But radio signals are so much easier to send than vehicles! In fact (it has been suggested) interstellar travel is, if not actually impossible, so time consuming and inefficient that no species in its right mind would consider using it as a substitute for electromagnetic communications. Yet the anthropomorphism of such a response should make it very suspect. As Aimé Michel commented, after reviewing a number of works on "interstellar communication,"

Almost all these solemn works are inspired by one single and solitary idea, always the same one: to wit, the crazy presumption of the human mind, which would have the immense universe teeming with non-human intelligences, always provided that---as Bergier puts it---those superintelligences have studied at the Sorbonne or Oxford or M.I.T. 16

The anthropomorphism of the "they can't get here" school is further displayed by the opposite school of thought, commonly known as the "absence of extraterrestrials on earth" argument. According to this second, equally anthropomorphic view, if there were super-intelligences, they would already be here on earth, thanks to the supertechnology they would certainly have evolved. Since there is an evident lack of ETI's on earth, there must be no ETI's. I will not attempt to critique either of these points of view here, but simply note that their violent contradiction with each other in regard to ETI transportation capabilities shows that neither's premise is "obvious" or "necessary." What both viewpoints share is a respectability due in part to their refusal to consider data based on UFO observations.

This respectability is important, as my third footnote in the original paper and several of the commentors show (Richard Greenwell, H.J. Eysenck, Stanley Krippner, and Roger Wescott). Its existence and manipulation by elites in the scientific community determines the status of ideas as well as people. "In the long run" the correct ideas will doubtless succeed. But then almost anything unpleasant, looked at with sufficient "perspective," appears less painful. Perspective is a luxury of non-participants; for those involved, perspective usually comes, if ever, after the battle is over. For scientists and non-scientists involved in crypto-science, neglect and ridicule is exceedingly painful. The crypto-scientist perhaps does not deserve to be treated as a scientist; but to treat him or her as a charlatan is a gross injustice. For the crypto-scientist works within constraints. The CS's research may be slow, inefficient, and inadequate, but it is rarely knowingly fraudulent. The charlatan does not have this limitation, and can make nature appear to say whatever is desired.

Furthermore, the prosecution of crypto-scientists encourages scientists to be sloppy. If any critique of crypto-science, no matter how sloppy or exaggerated, is seen as a service to the scientific community, then a dual set of standards emerges. There is one set of standards for dealing with genuine scientific work, and another for dealing with pseudo-science, in which crypto-science is included. This is not an idle speculation, for the 186 persons who signed "Objections to Astrology" affixed their signatures to a very unscientific document.¹⁷ When 18 Nobel Prize-winners sign a document which is demonstrably false, something is wrong. The subsequent "sTARBABY" scandal, with many of the same actors, shows the same forces at work. The urge to "get" crypto-science at all costs is expensive, ultimately, for science as well as for crypto-science.¹⁸ For ultimately, the same techniques and possibly even the same "hit men" may be used on targets within science as well as outside it.

What, then, is it reasonable for crypto-science to expect? Certainly crypto-science ought to be treated in a friendly manner, and neither lumped with pseudo-science nor persecuted. This does not mean that either sloppy work or intellectual fraud ought to be accepted in crypto-science---although, realistically, it may more likely occur in crypto-science. There would probably be less

sloppiness and fraud in crypto-science, however, if CS's got more acceptance from scientists, and did not have to resort, as has happened to more questionable associations with fringe science or occult groups. Since the work of crypto-scientists can be of value for humane considerations as well as intellectual ones, they deserve tolerance.

I am not sure how many of the arguments I have made here apply to fringe theorists such as Velikovsky. Crypto-science begins with anomalous observations, and these observations may be of value even if their interpretation turns out to be different from that placed on it by the researchers. Fringe theorists, however, have no such observations, although their theories may call attention to events otherwise ignored. The indignity of Velikovsky's treatment at the hands of the scientific community, however, does seem to raise some of the same issues of tolerance. I await the appearance of Henry Bauer's book on Velikovsky so that I can have more data to make a more intelligent decision!

"Reality...What A Concept" is the title of a record album by Robin Williams. I agree with C.L. Hardin that my deviant use of "reality" is probably closer to Robin Williams than to a proper philosophical understanding. What I was trying to get at, however, is a point that both he and Gerd H. Hövelmann overlook in their equally persuasive accounts (which I am not qualified to judge) of reality. That is, what a group takes to be real is strongly bound up with a host of cognitive and emotional interests which makes anomalies taboo as well as surprising. Arguments over anomalies are not simply academic exercises. They often involve gut issues about how much we know about the world, who is to be considered an authority on it, and what might be there that we don't know about. The shock recalled by Smith in finding a Coelacanth is merely one instance of what happens when a solid edifice of knowledge is invaded by a deviant experience. People invest emotions in what they believe to be the truth. When others disagree on which experiences are real, there is going to be trouble.¹⁹

It is with this kind of trouble that my own studies of scientific controversies and anomaly reporting have dealt. People get "shook up" when they see things that aren't supposed to be there. They get even more disturbed when they try to explain to other people what they experienced, only to find themselves doubted or ridiculed. Similarly, people get upset when someone tries to explain to them that an "impossible" event has just taken place. The whole situation becomes more complex when an entire community (or a significant portion of its members) has experiences that seem delusional to the outside world. Two examples of such situations occur at any lake with a frequently appearing "monster" and at the Yakima Indian Reservation, which I discussed in my original paper.

Last summer I spent two weeks with the Yakima Indian Nation, trying to verify the reports I had read about. I talked to roughly two dozen persons of various ages and occupations, including several fire lookouts. The "UFO" and other ("bigfoot," "poltergeist," "stick Indian") events that were related to me included a large range of experiences that went from the seemingly subjective to the almost certainly objective. At the latter end of the spectrum I would place the two dozen or so photographs taken by, among others, the staff fire control officer and an engineer sent in by the Center for UFO Studies. I am sure that I merely scratched the surface of the anomalous events experienced by members of the community and fortunate outsiders such as the aforementioned engineer. The reservation seems to be a crypto-scientists's paradise. But are the phenomena real?

An attempt to answer this question evokes what I really meant when, according to Hardin, I mis-used "reality." While it may be possible "in the long run" to decide what is real or not based on some objective criterion, in the short run we must operate on much less perfect indications. Thus at this moment in time I cannot tell whether the members of the Yakima nation (and their cameras) are hallucinating or not. Frankly, it is difficult to disbelieve fire lookouts who have spent lifetimes in the area that they are scanning and whose visual acuity is assessed on an almost daily basis by their rate of false fire alerts involving the instant mobilization of fire vehicles and personnel. Michael Persinger suggests, based on data from the reservation, that visual and auditory experiences are due to either anomalous physical events which are photographable or hallucinations induced by the same mechanisms which produce the physical events. The ultimate cause of such events, Persinger argues from the correlation between UFO sightings and earthquake tremors, is seismic. If he is right we have a ready explanation for the experiences, balls of lights, UFOs, abductions, etc. If he is wrong, then do we conclude that "drunken Indians" are responsible for what, is certainly a frighteningly high rate of UFO activity on the reservation? I don't think so.

In the meantime, how are the inhabitants of the reservation to regard their own experiences? To be sure, as Native Americans, ostracism and ridicule are nothing new to them, and so perhaps the discrepancy between their experiences and external definitions of reality are less significant than they would be for others. I cannot shake the feeling that there are some very important phenomena taking place on the reservation which we outsiders, need to know about. Yet our own definitions of what is real have interfered with our finding out about what is going on there. Furthermore, the inability (and unwillingness) of current science to explain these experiences has increased the terror of those who experience them. The members of the Yakima Nation need our science just as our science needs their observations. Thanks to the timely intervention of the Center for UFO Studies and J. Allen Hynek, many of the events have been recorded by Bill Vogel, former Staff Fire Control Officer on the reservation. An independent investigation of the events was carried out by David Akers, an engineer who works with CUFOS. The records and photographs of Vogel and Akers are now in the hands of Michael Persinger, who is trying (with some success) to link them to seismic events. Without

the existence of a crypto-scientific organization such as CUFO, this data exchange and analysis could not now be taking place. Exactly this sort of confrontation between ostensibly deviant data and scientific knowledge is the major raison d'etre of crypto-science.

Nothing that crypto-scientists do could not be done by ordinary scientists, but there just aren't enough of the latter, and they are often unwilling to do it. Someone has to answer the phone. Someone has to explain to the terror-stricken family whether it was a UFO, a bolide, or ball lightning. Someone has to pay attention to evanescent events while they are still around to be recorded, to keep track of the pulse and temperature, until the doctor comes. Crypto-scientists perform a useful service to science and to the public. They could be more useful if they got more training and more acceptance.

To sum up, then, crypto-science has a definite contribution to make in the exploration of phenomena that are not yet within the pale of science. It may also help in sorting out those phenomena which are mistakenly thought to be anomalous, but whose explanation is more mundane. Yet one's expectations for crypto-science must be reasonable. Given its limited resources, its contributions may be quite modest. It is unlikely to discover any new scientific principles, although some of the phenomena it detects may upon examination reveal such new principles. The examination in most cases however, will be carried out by science and not by crypto-science.

Before closing, I would like to mention one valuable distinction that I have glossed over in the discussion here. Marcello Truzzi has divided the area that I have labeled here "crypto-science" into two varieties which he has called cryptosciences and parasciences.²⁰ The former include those forms of anomalies where the subject of research is a discrete object such as a UFO or a bigfoot, which potentially one could plunk down on the lab table of doubters, and thus end the dispute in a single stroke. Parasciences, by contrast, involve anomalies whose existence must be inferred from the connection between otherwise ordinary events, such as relationships between the position of planets and birth of champion athletes; or between dreams about future events and the events actually taking place. Thus the resolution of disputes in the parasciences always hinge on inferences, whereas cryptoscientific disputes potentially can be resolved simply by producing the thing involved. Here I have largely dealt with anomalous observations, without distinguishing between their cryptoscientific or parascientific qualities. Observations of both types are likely to be "hidden" both by those who experience them at first-hand and also by scientists who encounter them by chance. Nonetheless, there are some important differences in the sociology of science of these different types of anomalous events, and in an essay of much greater length, they could be delineated.

NOTES:

1. The proceedings of this conference have been edited by Rachel Laudan, The Demarcation Between Science and Pseudo-Science, Working Papers in Science and Technology, Vol. 2 #1 (April, 1983), Center for the Study of Science in Society, Virginia Polytechnic Institute and State University, Blacksburg, Virginia.
2. These studies are summarized in part in my "Social Intelligence About Hidden Events," Knowledge: Creation, Diffusion, Utilization, Vol. 3, #3 (March 1982), pp. 381-400. See also footnote 25 of my article in ZS #10.
3. Anomaly and Society: Social Intelligence About Hidden Events (in process).
4. W.N. Charman, "Ball Lightning," Physics Reports, Vol. 54, pp. 261-306.
5. Ron Westrum, "Social Intelligence About Anomalies: The Case of UFOs," Social Studies of Science, Vol. 7 (1977), pp. 271-302, at 289. I might mention that during my investigation of a poltergeist case, one older gentleman threatened to shoot me if I revealed the events to the local community. Considering what has happened in other such cases, he can hardly be blamed. See Ed Lowe, "The Amityville Horribles," The Detroit News Magazine, April 6, 1980, pp. 24-28.
6. See for instance, Michael A. Persinger, "Geophysical Variables and Behavior: VIII. Specific Prediction of UFO Reports with the New Madrid States by Solar-Geomagnetic and Seismic Measures," Perceptual and Motor Skills, Vol. 56 (1983), pp. 243-249. In 1969 the Air Force Project "Bluebook," to which UFO events could be reported, was closed.
7. See Westrum, "Social Intelligence About Hidden Events," at 394.
8. These interviews will be reported in a future article in Social Studies of Science.
9. See Ron Westrum, "The Human Factor in UFO Sightings," Proceedings of the 1981 MUFON Symposium (Cambridge, Mass. July 1981), pp. 59-74, at 65. The two studies which involved reporting rates for astronomers and engineers/scientists both were based on mail surveys. It seems plausible that those who respond to such surveys are more likely to report their sightings anyway. If so, then these rates (18% and 22% respectively, compared with 13% for the general public) are probably over-estimates. It is to be noted that "reporting rate" in this context is the probability that a sighter will attempt to report his or her sighting.
10. See Ron Westrum, "Knowledge About Sea-Serpents," in Roy Wallis, Editor, On the Margins of Science, special number of Sociological Review Monographs, Vol. 27 (1979), pp. 293-314.
11. The efforts of anomaly witnesses to verify or refute conclusively their experiences are often lengthy and painful. The catharsis when they get to talk about such things to a sympathetic listener is often impressive.
12. For those who can't resist a peek anyway, see Ron Westrum, "Science and Social Intelligence about Anomalies: The Case of Meteorites," Social Studies of Science, Vol. 8 #4, pp. 461-493.
13. Allan Hendry, The UFO Handbook (Garden City, New York: Doubleday and Company, 1979), p. xii.
14. Donald C. Pelz and Frank M. Andrews, Scientists in Organizations: Productive Climates for Research and Development (Ann Arbor: Institute for Social Research, 2nd Edition, 1976); Ralph Katz, "The Effects of Group Longevity on Project Communication and Performance," Administrative Science Quarterly, Vol. 27, #1 (March 1982), pp. 81-104.

15. It is worth noting that there are two potentially separable claims here. The first is that sociology can help in the management of science. As a student of complex organizations, it seems reasonable to believe that this is the case. The second is that sociological facts or theories can enter into scientific reasoning. This seems more contentious, but as should be apparent from the text, any branch of science which is dependent, even in part, on reports from the general public must use some sociological reasoning, whatever its quality.
16. Aime' Michel, "Project Dick," Flying Saucer Review, Vol. 18 #1 (January-February 1972), pp. 13-19, at 13. This article should be made required reading for all SETI researchers.
17. See Ron Westrum, "Scientists as Experts: Observations on 'Objections to Astrology'," The Zetetic, Vol. 1, #1, (Fall/Winter 1976) pp. 34-46; and "More on Astrology," The Zetetic, Vol. 1 #2 (Spring/Summer 1977), pp. 107-112. Both of these articles are followed by critiques jointly authored by Paul Kurtz and Lee Nisbet. It is disturbing that many of the big guns who signed the statement didn't do their homework. According to Paul Feyerabend, "When a representative of the BBC wanted to interview some of the Nobel Prize Winners they declined with the remark that they had never studied astrology and had no idea of its details. Which did not prevent them from cursing it in public." From his Science in a Free Society (New York: Schocken Books, 1978), p. 91.
18. See for instance, Ray Hyman, "Pathological Science: Towards a Proper Diagnosis and Remedy," Zetetic Scholar, #6 (1980), pp. 31-39.
19. Jeff Coulter, "Perceptual Accounts and Interpretive Assymetries," Sociology, Vol. 9 #3 (1975), pp. 385-396.
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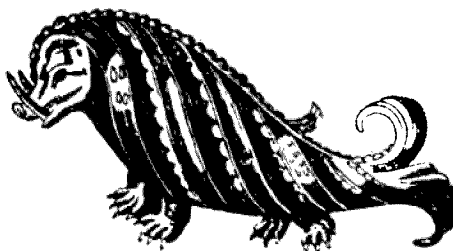
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SEVEN RECOMMENDATIONS FOR THE FUTURE PRACTICE OF PARAPSYCHOLOGY

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The purpose of this paper is to recommend a few strategies parapsychologists should take into consideration in their future attempts to obtain legitimacy and recognition by "normal" science. Some of my recommendations will, presumably, be no news to many parapsychologists; by offering some other, more radical and provocative ones, however, I will probably risk unpopularity. In the argumentation to follow I will take for granted that parapsychologists regard themselves as scientists and their endeavor to investigate psi phenomena as scientific. From my arguments it will become evident, however, that apparently it is not sufficiently clear to any parapsychologist what that claim actually means. Thus, it seems necessary to remind some people in the field from time to time of the standards and requirements they have to meet if they want to substantiate their claim to do scientific research.¹

In the following, I will first put forward the respective recommendation, and immediately afterwards I will make comments upon it. I will altogether make seven recommendations, and I start now, quite conventionally, with the first one.

First recommendation: Parapsychologists should instantly give up their revolutionary outlook upon their field and upon themselves.

Comment: Many parapsychologists have for a long time, at least since the publication of Th.S. Kuhn's book on the structure of scientific revolutions,² pleased themselves in calling their field "revolutionary" and themselves "revolutionaries." Especially some leading figures in the field of parapsychology, such as J.B. Rhine and J.G. Pratt, have adopted this view and claimed to be practising revolutionary science. Aside from the fact that here I cannot see any revolution at all, this self-assessment reveals a grave misconception: it is not enough to commit oneself to research in a "frontier science" or a field severely attacked by pugnacious advocates of the exclusive scientific truth to be called revolutionary. As sociologists Collins and Pinch have pointed out, "On a global scale parapsychology has many characteristics of orthodox scientific disciplines."³ One of these characteristics parapsychologists have adopted from the established sciences is the rigid application of orthodox scientific research methods in many, though not in all, of their investigations. This fact has been impressively documented, for instance, in Benjamin Wolman's voluminous Handbook of Parapsychology.⁴ Parapsychologists cannot at the same time loudly propagate revolutionary slogans.

It is evident that this does not exclude the possibility

* This is an expanded version of a paper presented by the author under a slightly different title at the Twenty-Fifth Annual Convention of the Parapsychological Association, Cambridge, U.K., August 16-21, 1982.

that some scientific endeavors may eventually lead to a fundamental change in a currently accepted basic scientific concept which -- in Kuhnian terms -- may be described as a "paradigm switch" or a "scientific revolution." What is untenable, however, is the claim of some parapsychologists that such a "paradigm switch" can be attained by way of taking the programmatic decision to revolutionize science.⁵

It may perhaps be added that parapsychologists' claim to practise revolutionary science will not just favorably dispose members of the "scientific community" toward the acceptance of parapsychology as a legitimate branch of science.

Second recommendation: Parapsychologists frequently seem to feel urged (or even entitled) to express themselves in more or less learned words on the problem of survival after bodily death. They should leave off this habit.

Comment: Although, as I have tried to show in detail elsewhere,⁶ strict repeatability of parapsychological experiments is impossible to obtain for theoretical reasons and cannot reasonably be postulated to be a condition sine qua non to establish parapsychology as a science, the results so far obtained in these experiments are still far too unreliable, ambiguous, and inconsistent to draw firm conclusions from them. Even the most cautious inferences parapsychologists draw from their experimental studies very often turn out to be essentially premature and invalid. On the other hand, conclusions drawn from thanatological investigations⁷ or from other examinations aiming at support of the survival hypothesis, such as those drawn from the famous "cross-correspondences,"⁸ those drawn from some of the spontaneous paranormal phenomena like apparitions⁹ and RSPK occurrences,¹⁰ or from phenomena produced in quasi-experimental settings like out-of-body experiences¹¹ or electronic voice phenomena,¹² to date are even more arbitrary and speculative than those drawn from experimental laboratory tests. Moreover, many of these phenomena are widely open to various kinds of alternative explanations, be they normal or paranormal. So, to give two further examples, the cases mentioned in the recent critical surveys by Ian Stevenson and Alan Gauld, respectively, are, in principle and without artifice, all explainable by means of a combination of capabilities of living persons.¹³

Applying scientific standards, survival cannot be regarded as proven, of course, as long as there are reasonable counter-explanations possible. As for me, in the foreseeable future I do not even see the slightest chance of getting conclusive evidence of survival in the sense of a definite scientific proof excluding any alternative explanation. No matter what our personal attitudes toward the survival problem may be, in a scientific approach to that problem we should realize that Occam's razor is still sharp. Therefore, we ought to responsibly avoid provoking treacherous hopes and expectations among the lay public by holding back our more or less poorly founded speculations for the time being, even though some laymen seem to be eager for "spontaneous cases, survival claims, and discursive material."¹⁴ How could we otherwise be able to justify our practice in view of possibly alarming outgrowths of irrational hopes and longings on the part of the lay public in consequence of our irresponsible rashness? If there are some parapsychologists who insist that they cannot give up expressing themselves on the question of survival, they, at

least, ought to be able to give compelling reasons for their opinion. Furthermore, they should unmistakably emphasize in their public statements that they are not able to provide any evidence in support of the survival hypothesis and that, therefore, the readers should take note of their explanations only very cautiously and with all the reservations necessary. In other words, these researchers should in any case clearly indicate that their statements are merely speculative in character.

Finally, we should afford to ask ourselves whether investigating the problem in question is important and desirable at all. Would we profit in one way or the other by finding out whether or not we will survive? Would this knowledge be useful to meet our vital interests and the requirements of our everyday-lives? Would it relieve our mortal dread? Answering to these questions, someone might argue: "Well, we have to do this kind of research since, being scientists, we are obliged to find out what the destination of man is." From my point of view this argument is anything but convincing since it merely advocates a fictitious reason or a feigned purpose. It is an inappropriate myth that scientific research is possible without concrete purposes. Unfortunately, this myth is a highly appreciated and popular one among scientists of any discipline since concrete purposes have always to be justified (or at least, justifiable upon request).¹⁵

In connection with this second recommendation, it must likewise be postulated that parapsychology should be kept free of any kind of ideological speculation on the nature of man, of the world, or of the universe, or on the meaning or purpose of life, and the like. Speculations of this kind should further on be reserved for aging Nobel laureates.¹⁶

Third recommendation: Parapsychologists should not too heavily rely on what some of them call "personal evidence" obtained through spontaneous paranormal occurrences or in quasi-experimental settings (e.g., instances in which so-called "psychic-detectives" are reported to have successfully assisted the police in criminal investigations; "chair tests"; etc.).

Comment: Obviously, stories reporting spontaneous paranormal occurrences do not form a reliable basis for a scientific study of the paranormal since their value as evidence depends on various imponderable factors, such as, for instance, the trustworthiness and reliability of the witnesses, the accuracy of perception and memory, the possibility of defective reporting, mere chance, etc. The many thousands of case reports which have been gathered in the parapsychological literature (especially those in the enormous collections compiled during the early years of the British Society for Psychical Research) may possibly all have happened as they are reported, although in most of the cases there are strong reasons to doubt that they have. To people already convinced of the reality of ESP and PK, it may seem likely that the case reports are really dealing with genuine instances of paranormal occurrences; as evidence of the reality of paranormal phenomena (and only such evidence counts in science), these stories are without any value, however. Even the results obtained in quasi-experimental settings as "chair tests," for instance, are -- as Piet Hein Hoebens has shown in many of his papers¹⁷ -- widely open to various kinds of flaws and over-interpretation.

Hoebens's studies, in particular, reinforce the case against the opinion that our knowledge about the paranormal can be much advanced by the mere compilation of spontaneous cases or by any kind of quasi-experimental investigation which today is still highly esteemed as proof of ESP or PK in parapsychology¹⁸. Now and in future, the controversy about the reality of paranormal phenomena will not be settled by disputes about alleged spontaneous paranormal occurrences. Parapsychologists should realize that case reports do not prove anything and that the only value such reports have is to stimulate invention of novel designs for rigid experimental testing.

Fourth recommendation: Parapsychologists should cease to pretend that they are able to explain anything by means of their present terminology which is merely descriptive and build up a standardized, methodically constructed terminology as soon as possible.

Comment: Occasionally, one can notice that parapsychologists use technical terms such as "psi performance," "psi information," "paranormal communication," and many others, in a way that seems to suggest that these terms have considerable explanatory properties. So, on several occasions I have come across statements in the parapsychological literature saying, for example, that certain phenomena can be explained as effects of a PK force or others as an information transfer independent of the recognized channels of sense. This manner of speaking is grossly negligent, however. In fact, nothing is explained by making reference to a "PK force" or to an "information transfer independent of the recognized channels of sense," respectively, as long as we do not know what a "PK force" is or how the "information transfer" operates. Parapsychologists may well label certain unusual (and as yet unexplained) phenomena as, let us say, "extrasensory" or "psychokinetic," but they should realize that these are only descriptive classifications lacking any explanatory value. Moreover, the extents of all these terms, and of many others as well, are so poorly specified that they can almost be used at pleasure. Keeping their present terminology, parapsychologists will hardly be able in the long run to sufficiently ensure understanding with fellow parapsychologists as well as with other scientists.

As a norm, scientific statements have to be intersubjectively understandable and verifiable. Parapsychologists cannot observe this norm using the vague terms which presently are at their disposal. It is a matter of great urgency, therefore, to methodically construct a standardized parapsychological terminology. By "methodical" I mean that each technical term has to be introduced explicitly, progressing from the most basic to the peripheral ones, thus standardizing the lingual means of the field¹⁹. Circular definitions must, of course, be avoided and it does not matter whether currently used parapsychological terms are redefined or new ones are introduced. By means of such a terminology, it will be possible to guarantee intersubjectivity of the statements made by parapsychologists.

Fifth recommendation: In view of the frequent inconsistencies of their experimental findings, parapsychologists should not resort to the fatalistical conception that these inconsistencies are necessarily constitutive of paranormal events.

Comment: Obviously, some parapsychologists are troubled or even discouraged by the fact that very many of their experimental results are notoriously inconsistent. One might even sarcastically remark that, in fact, this inconsistency so far seems to be the only reliable finding within parapsychology. Nevertheless, it is neither admissible nor logically self-consistent to conclude from this unpleasant fact that these inconsistencies must, so to speak, be necessarily constitutive of paranormal events²⁰. Such a conclusion is all the more questionable as it is by no means clear whether or not it is just the current methods and conceptualizations in parapsychology which may not yet be sophisticated enough to allow a water-tight explanation of psi functioning. Parapsychologists should, therefore, not overhastily abandon the concept of lawfulness in their field. Unfortunately, sometimes one cannot help suspecting that, as Bauer, Kornwachs and von Lucadou have recently stated²¹, some parapsychologists are even proud of the fact that they constantly obtain inconsistent results and, even worse, that these results evidently do not seem to fit into any of the currently available scientific conceptions of the world²².

Sixth recommendation: Parapsychologists should carefully consider the arguments of the critics of their field and collaborate with the scientifically-minded among them whenever possible.

Comment: In the history of their field, parapsychologists have always turned out to be among the keenest and most ingenious critics of their own research work. In addition, the field has received a lot of criticisms from outside. It is not necessary here to sketch the history of these criticisms to be able to state that they have been extremely different in character. In Germany, for instance, some critics have claimed that parapsychology is a novel form of "Wissenschaftskriminalität" (criminality in science)²³ or that "witch-madness" has entered the universities again²⁴. In the English speaking parts of the world, especially the critiques by Price²⁵, Hansel²⁶, and --- more recently --- Wheeler²⁷ have been most influential although a considerable part of their criticisms have been shown to be invalid. On the other hand, some critics, as for instance Truzzi, Hoebens, or Hyman, have really got down to the problems inherent in parapsychological research.

In my opinion, scientific criticism should be defined as "substantiation of the request to give up a particular orientation in the field of scientific activity"²⁸ Truzzi, Hoebens, and Hyman, for instance, the critics already named above, have met this criterion in their writings, some others, such as Prokop and Wimmer, in particular, have not.

Parapsychologists should welcome (and collaborate with) such critics who have shown that they are willing to discuss problems confronting parapsychology on a scientific level and who are ready to thoroughly examine the case parapsychologists believe they have made in favor of the factual occurrence of paranormal phenomena before they form definite judgement on the matter. Parapsychology has nothing to lose and much to potentially gain by collaborating with these critics. And -- who knows? -- some day we may even be able to give up the unpleasant distinction between the parapsychologist and the critic.

Seventh and final recommendation: Parapsychologists should strictly separate themselves from all those pseudoscientific claimants who frequently

put forward untestable ideas often full of supernaturalism and metaphysics and who refuse to adopt rigid scientific methods.

Comment: Unfortunately, despite of the rigid use of orthodox scientific research methods in many parapsychological investigations which I have praised in the comment upon my first recommendation, an alarming inclination seems to be omnipresent in many parapsychologists to flirt with occult or antiscientific ideas and to ogle with bizarre esoteric, mystic, or -- as Martin Johnson pertinently named it at an earlier P.A. Convention -- "parapornographic" groups or periodicals. These researchers occasionally show such an uncritical tendency to accept questionable pseudoscientific claims that sometimes one is under the impression that the critics of the field do not go entirely wrong when they reproach parapsychology for lacking critical judgement and intellectual self-discipline. That is not to say, however, that we should on principle keep away from these questionable organizations, or claimants, or periodicals. However, we should not meddle with them but rather state explicitly whether their arguments are credible, sound, and scientific, and what, therefore, we should consider their methods, concerns, and attitudes to be. And this does not even contradict -- as someone might suspect -- Charles S. Peirce's general demand that we should do nothing that might block inquiry.

Again, if we really want our field accepted as a science, we should act accordingly and, in methodological respects, be more papal than the Pope. Arbitrariness of our methods and statements, on the other hand, would open the door to all kinds of pseudoscientific speculation and lead to the field's vulgarization in the worst sense of that word. Thus, we would badly risk the still low degree of academic integration parapsychology has achieved to date. Therefore, it must be emphasized again that we should rigorously dissociate ourselves from obstinate occultists and credulous and thoughtless supernaturalists of whatever shading they may be.

Those among us who are worried about the financial support which these organizations and private persons currently give to our research should consider John Beloff's recent suggestion²⁹ that we propose the installment of a scientific commission which should be directed by a distinguished and respected scientist and receive sufficient financial advancement. The commission's term of office should last no less than three years, and its only assignment would be to put forward a report on the commission's opinion whether there are valid clues to the existence of extrasensory or psychokinetic phenomena (or of PSI-GAMMA and PSI-KAPPA, as Beloff -- adopting Robert Thouless's distinction³⁰ -- prefers to term it). It is essential to Beloff's proposal, however, that the activity of the commission should not be restricted to a check of the evidence already available. The commission should rather be authorized to financially support the pursuance of particular tracks of empirical data which, in their eyes, are promising or stimulating but not yet definitely conclusive. The commission, Beloff further suggests, should be installed in close cooperation with the Parapsychological Association and the American Association for the Advancement of Science. As I have briefly pointed out elsewhere,³¹ there will, of course, arise some problems as to the realizability of this proposal, but these problems should be solveable. Whatever the commission's conclusions may be, in any case they would be of considerable influence on the attitude of the "scientific community" toward parapsychology and, consequently, on the funding of parapsychological research. All people

interested in settling the unpleasant science versus pseudoscience controversy with regard to parapsychology, the critics of the field inclusive, should therefore lend their support to Beloff's proposal.

Finally, to turn back to the relations of parapsychologists to the occult, I must strongly emphasize that -- like K.R. Rao³² -- "I have little sympathy for those among us who are bothered by the methodological 'scientism' in our field. A return to hermetic contemplation may give one a more satisfying picture of psi, but such will not constitute a scientific endeavor." No matter whether we regard science as the most recommendable way to "approach the truth" (to speak like a Popperian, for once),³³ or whether we think of science as of just another ideology or tradition having no more rights than others (as Paul Feyerabend does),³⁴ in any case we will have to adhere to the methods and methodological standards which are held to be scientific in orthodox science, provided that we want to substantiate our claim to be scientists conducting scientific research. Note that I am not saying that the scientific methods currently available are of exceptional soundness and dignity per se. All I am saying is that, if we regard ourselves as scientists, we have to use them. We cannot have it both ways. Either we adopt the methods and methodological standards provided by science or we should cease to desire and to expect favorable recognition by the scientific profession. It's for us to decide!

NOTES AND REFERENCES

1. After the presentation of the earlier version of this paper at the Twenty-Fifth Annual Convention of the Parapsychological Association, August 19, 1982, two commentators reproachfully remarked that they were somewhat bewildered by the fact that "such a relatively young man dared" (sic) to make recommendations as to which attitude much older and well-recognized members of the parapsychological community should adopt with regard to the future practice and the scientific status of their field. These remarks were most interesting to me since they indicate that in some science-sociological respects, at least, parapsychology has already fully adopted habits of 'normal' science! Of course, there is only one adequate answer to this reproach: if the strategies to be recommended here are really worth considering, then it is totally irrelevant whether the person who makes such recommendations is 26 or 76 years old.
2. Kuhn, Th.S.: The Structure of Scientific Revolutions. Chicago: University of Chicago Press 1962. Many scientists and philosophers who nowadays extol the farsightedness of what they call Kuhn's "philosophy of science", completely disregard the fact that Kuhn himself had originally conceived his model as a novel theory of historiography of science.
3. Collins, H.M. & T.J. Pinch: "The construction of the paranormal: Nothing unscientific is happening," in: Wallis, R. (ed.): On the Margins of Science: The Social Construction of Rejected Knowledge (Sociological Review Monograph 27). Keele, Staffordshire: University of Keele 1979, 237-270 (Quoted from p. 253). In my view, some of the critics of the field are too easily dismissed in parts of this paper, however.

4. Wolman, B.B (ed.): Handbook of Parapsychology. New York: Van Nostrand Reinhold 1977; also cf. Krippner, S. (ed.): Advances in Parapsychological Research. I. Psychokinesis. New York & London: Plenum Press 1977; Krippner, S. (ed.): Advances in Parapsychological Research. II. Extrasensory Perception. New York & London: Plenum Press 1978.
5. I am grateful to Prof. Marcello Truzzi for calling my attention to this question in the discussion after the presentation of the original version of this paper at Cambridge.
6. Cf. Hovelmann, G.H.: "Zum Problem der Wiederholbarkeit parapsychologischer Experimente. Teil I", in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 25 (1983): in press; Hovelmann, G. H.: "Zum Problem der Wiederholbarkeit parapsychologischer Experimente. Teil II", in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 25 (1983): in press. An abridged English version **is in press** under the title: "Are psi experiments repeatable?" in: Research Letter of the Parapsychology Laboratory of the University of Utrecht.
7. Cf. for instance Osis, K.: Deathbed Observations by Physicians and Nurses. (Parapsychological Monograph No. 3). New York: Parapsychology Foundation 1961; Osis, K. & E. Haraldsson: At the Hour of Death. New York: Avon Books 1978.
8. For a survey, cf. Saltmarsh, H.F.: Evidence for Survival from Cross Correspondences. London: Bell 1938.
9. Cf. Tyrrell, G.N.M.: Apparitions. New York: Macmillan 1962 (originally published in 1942).
10. Cf. Roll, W.G.: "Poltergeists", in: Wolman, B.B. (ed.): op. cit., pp. 382-413.
11. Cf. for instance Osis, K. & D. McCormick: "Kinetic effects at the ostensible location of an out-of-body projection during perceptual testing," in: Journal of the American Society for Psychical Research 74 (1980): 319-329; also cf., although there is no mention of possible implications of OBE research for survival, Blackmore, S.J.: Parapsychology and Out-of-the-Body Experiences. (Perspectives in Parapsychology). Hove, East Sussex: Transpersonal Books 1978.
12. Some critical comments are to be found in Hovelmann, G.H.: "Involuntary whispering, conversational analysis, and electronic voice phenomena," in: Theta 10 (1982): in press; also cf. Ellis, D.J.: The Mediumship of the Tape Recorder. Harlow, Essex: the author, 1978; Keil, H.H.J.: "The voice on tape phenomena: Limitations and possibilities," in: European Journal of Parapsychology 3 (1980): 287-296.
13. Stevenson, I.: "Research into the evidence of man's survival after death. A historical and critical survey with a summary of recent developments," in: Journal of Nervous and Mental Disease 165 (1977): 152-170; Gauld, A.: "Discarnate survival," in: Wolman, B.B. (ed.):

op. cit., pp. 577-630.

14. McClure, K.: "Correspondence," in: Journal of the Society for Psychical Research 51 (1982): 321-322 (quoted from p. 321); also cf. my critical remarks: Hovelmann, G.H.: "Scientific or entertaining? The language and content of the Journal -- reconsidered," submitted for publication in the Journal of the Society for Psychical Research.
15. Cf. Hövelmann, G.H.: "Reality, relevance, and responsibility," in: Zetetic Scholar 10 (1982): in press.
16. Cf. Hövelmann, G.H.: "Der Archetyp des Tricksters -- ein fatalistisches Alibi. Bemerkungen zu einem Aufsatz Lutz Müllers," in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 24 (1982): in press; Hövelmann, G.H.: "Natur und Mechanismus. Marginalien zu einigen Auffassungen W. Buchels," in: Zeitschrift für allgemeine Wissenschaftstheorie/Journal of General Philosophy of Science 14 (1983): in press.
17. Hoebens, P.H.: "Vom Lob der Genauigkeit in der Parapsychologie," in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 22 (1980): 225-234; Hoebens, P.H.: Comparison of Reports of the "Denver" Chair Test: A Critical Examination of the Methods of W.H.C. Tenhaeff. Unpublished manuscript, 1980; Hoebens, P.H.: "Gerard Croiset, le médium qui n'a jamais failli," in: Les Cahiers Rationalistes 369 (1981): 245-250; Hoebens, P.H.: "Gerard Croiset: Investigation of the Mozart of 'psychic sleuths,'" in: The Skeptical Inquirer 6 (1981): 1, 17-28; Hoebens, P.H.: "Croiset and Professor Tenhaeff: Discrepancies in claims of clairvoyance," in: The Skeptical Inquirer 6 (1981-82): 2; Hoebens, P.H.: "The mystery men from Holland. I. Peter Hurkos' Dutch cases," in: Zetetic Scholar #8 (1981): 11-17; Hoebens, P.H.: "Mystery men from Holland. II. The strange case of Gerard Croiset," in: Zetetic Scholar #9 (1982): 21-32; Hoebens, P.H.: "Die Legitimität des Unglaubens," in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 24 (1982): 61-73; Hoebens, P.H.: "Time machines, the Hume game, and a successful replication of a classic ESP experiment." Paper presented at the Twenty-Fifth Annual Convention of the Parapsychological Association, Cambridge, U.K., August 1982; Hoebens, P.H.: "Mystery men from Holland. III. The man whose passport says clairvoyant," in: Zetetic Scholar #10 (1982):
18. This argument does not touch, of course, the importance of the analyses of spontaneous cases recently conducted by Sybo A. Schouten, since they were not carried out with the intention to obtain evidence of the factual occurrence of paranormal phenomena. Cf. Schouten, S.A.: "Analysis of spontaneous cases," in: Research Letter of the Parapsychology Laboratory at the University of Utrecht 9 (1979): 53-63; Schouten, S.A.: "Analysis of spontaneous cases as reported in 'Phantasms of the Living,'" in: European Journal of Parapsychology 2 (1979): 408-454; Schouten, S.A.: "Analysing spontaneous cases: a replication based on the Sannwald Collection," in: European Journal of Parapsychology 4 (1981): 9-48; Schouten, S.A.: "Analysing spontaneous cases: a replication based on the Rhine collection," in: European Journal of Parapsychology 4 (1982): 113-158.
19. Cf. Kamlah, W. & P. Lorenzen: Logische Propädeutik. Vorschule des

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20. Here lies one of the (very few) problems which I still have with the system theoretic approach of von Lucadou and Kornwachs (cf. Kornwachs, K. & W. von Lucadou: "Psychokinesis and the concept of complexity," in: Psychoenergetic Systems 3 (1979): 327-342; Lucadou, W. von & K. Kornwachs: "Development of the system theoretic approach to psychokinesis," in: European Journal of Parapsychology 3 (1980): 297-314; Kornwachs, K. & W. von Lucadou: "Pragmatic information and non-classical systems." Paper presented at the Sixth European Meeting on Cybernetics and System Research, Vienna 1982; Lucadou, W. Von: "Der flüchtige Spuk," in: Zeitschrift für Parapsychologie und Grenzgebiete der Psychologie 24 (1982): 93-109; Lucadou, W. von & K. Kornwachs: "On the limitations of psi -- a systemtheoretic approach." Paper presented at the twenty-Fifth Annual Convention of the Parapsychological Association, Cambridge, U.K., August 1982. I must admit, however, that my objections do neither touch the justifiability nor the plausibility of this model itself.
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22. However, I know of some people (a few of which even call themselves "parapsychologists") who seem to dispose of the admirable ability to force any given phenomenon into any given conception.
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32. Rao, K.R.: "Science and the legitimacy of psi," in: Parapsychology Review 13 (1982): 1, 1-6 (quoted from p. 3).
33. Especially cf., Popper, K.R.: The Logic of Scientific Discovery. London: Hutchinson 1959 (originally published in German, 1934 [with the imprint "1935]).
34. Especially cf., Feyerabend, P.K.: Science in a Free Society. New York: NLB/Schocken Books, 1978.



CRITICAL COMMENTARIES:

COMMENTS BY JOHN BELOFF:

Like Gerd Hövelmann, I too, believe that parapsychologists ought never to underrate the importance of trying to persuade their fellow scientists to take them seriously. It is not a question of status that is here at stake; it is, rather, that until we win the backing of official science, we shall never have access to the funding and resources without which progress in this field will continue to be pitifully slow. I welcome, in particular, Hövelmann's support for my tentative suggestion for an official Commission of Enquiry. I could only wish that I had such an ally among those who occupy positions of power in the scientific hierarchy.

However, the question which is raised by his paper is whether the strategies which he is here recommending would have the desired effect. For, unless we can feel some confidence in this outcome, we may find that we have sacrificed a large slice of what has traditionally constituted the subject-matter of our science to no purpose. Doubts on this score at once begin to creep in with his second recommendation (R2) when the author remarks:

"results so far obtained in these experiments are still too unreliable, ambiguous and inconsistent to draw firm conclusions from them. Even the most cautious inferences parapsychologists draw from their experimental studies very often turn out to be premature and invalid" (author's underlinings). When we consider that the prime reason why the scientific community is still so reluctant to credit our phenomena is, precisely, because they are so fitful and uncertain, our prospects, it seems, are none too bright especially since the author has already proclaimed that: "strict repeatability of parapsychological experiments is impossible to obtain for theoretical reasons" (author's underlining). Yet for the sake of that elusive prize, official recognition, the author begs us forthwith to renounce (a) all thanatological concerns and (b) all investigation involving spontaneous real-life incidents (see R2 and R3).

Moreover, when we look closely at these two recommendations, we find that they embody serious misconceptions. Thus, commenting on R2, he writes: "Applying scientific standards, survival cannot be regarded as proven as long as there are reasonable counter-explanations possible" and, further: "in the foreseeable future I do not see even the slightest chance of getting conclusive evidence of survival, in the sense of a definite scientific proof excluding any alternative explanation." Now, what the author appears to ignore in such statements is that survival is no more than a theory or hypothesis put forward to account for certain anomalous findings. But, is there any theory or hypothesis in the entire corpus of science about which it could be said that it is proven so as to exclude any alternative explanations? It so happens that I, personally, remain unconvinced by the survival hypothesis, but I have nothing but the highest respect for scholars of the calibre of Ian Stevenson or Alan Gauld who regard survival as the most plausible interpretation of those findings to which they have drawn our attention; and I would certainly resist most strongly any attempt to suppress all speculation in this area in the interests of some supposed respectability.

Similarly, commenting on "the many thousands of case reports which have been gathered in the parapsychological literature (especially is the

enormous collection compiled during the early years of the British Society for Psychical Research)" (see R3), he asserts: "as evidence of the reality of paranormal phenomena these stories are without any value" (author's underlining). This means, in effect, that no anecdotal evidence, However carefully researched or corroborated, is worth anything as evidence for what actually transpired. One wonders whether the author has stopped to consider that if this assertion were generally conceded it would become virtually impossible to convict anyone of anything in a court of law. In fact lawyers take the view that direct testimony witnesses is considered superior to mere circumstantial evidence!

Further misconceptions are to be found with respect to R1, R4, and R5. I agree entirely with the point which the author makes in R1 to the effect that revolutions in science are not brought about by those whose credentials are sufficiently radical but rather by those who make discoveries which upset the equilibrium of the prevailing paradigm. Whether parapsychology can aspire to do this, however, I am very doubtful. I do not happen to share the faith of my friend J.G. Pratt, who thought of parapsychology as, essentially, marking time while it awaited the advent of a new Einstein. I, personally, do not believe that parapsychology will ever be incorporated into physics no matter how advanced or futuristic physics may yet become. I believe that we are in quite a different ballgame, our ball being the world of mind rather than the world of matter. Hence, if we are to consider ourselves revolutionaries, it is with respect to the prevailing metaphysics of materialism according to which everything must ultimately be explainable by the laws of physics. What the author leaves unsaid is where he stands on this issue. He can scarcely deny that parapsychology makes some very subversive claims. What we want to know is what kind of a revolution it portends.

His discussion of current terminology in parapsychology (see R4) is likewise obscure. What does he mean, for example, by a "methodically constructed" terminology? Admittedly, the terms we now use which have entered our vocabulary for diverse historical reasons could easily be improved if we were now starting afresh. But, given these historical constraints, our basic terms have at least the advantage of being theoretically uncommitted. They are not intended to be explanatory in the sense in which certain concepts in physics are explanatory because they are derived from a coherent body of theory; there is no such theory in parapsychology, and it is premature even to demand one. Nevertheless, to label some event as an instance of psi is not just purely descriptive; at the very least it implies that this event cannot be explained by any known physical theory. Let us not forget, moreover, that not every event may be explicable in theoretical terms. If you take seriously the idea of "free-will," then you are committed to the view that some events may originate in a simple act of volition and, beyond that, there is nothing further to be said of any relevance. Similarly psi may turn out to be another such manifestation of the mind in action. I would agree that we ought not to exaggerate the spontaneity of psi phenomena or boast about it or "overhastily abandon the concept of lawfulness" (see R5), but neither should we reject what may well be a cardinal feature of psi merely to placate our scientific neighbours.

Having, I hope, made it clear where I take issue with Gerd Hövelmann while all the time supporting his objectives, I am happy to conclude by

saying that I have no fault to find with R6 or R7. I agree that, wherever possible, we should collaborate with responsible critics such as are to be found in the pages of ZS and, equally, we should keep our distance from irresponsible pseudoscientists and mystagogues. I would merely want to add the proviso that there are no phenomena too absurd or bizarre to merit our attention.

COMMENTS BY SUSAN J. BLACKMORE:

I enjoyed reading Hövelmann's Recommendations, and they all set me thinking but two stood out; one because I so much agree and the other because I disagree. I shall comment on just these two and add a recommendation of my own.

I wholeheartedly endorse the suggestion that we give up calling ourselves revolutionaries. As Hövelmann points out, there is a world of difference between demanding scientific revolution and actually creating it. However, he seems to imply that there is nothing revolutionary in parapsychology at all. Yet there might be. For example the observational theories are potentially revolutionary in that they require totally new ways of looking at interactions and causation. I once spent an entire week arguing with Brian Millar about them. Every morning we each had new challenges at the ready, and by night time we had arrived at some sort of stalemate. After a week of this, we were both still convinced of our original positions, but (and this is why I mention it) we better appreciated just how much habitual thinking has to be given up. I now do not believe that the observational theories offer a way ahead for parapsychology, but they are an example of potentially revolutionary thinking of the sort which just might lead to a revolution.

I would also like to amplify a particular danger of parapsychologists taking up a revolutionary stance. I was at least partly drawn into the field because of the feeling that it was challenging the accepted concepts of psychology and other sciences, and I presume others are attracted for the same reason. I attributed the rejection of the subject by teachers and researchers to its revolutionary nature. It took me many years to learn that this might not be the only reason and that indeed parapsychology might not be so very innovative after all. Calls for revolutionary thinking can fire enthusiasm to try again, and one day that might actually lead somewhere, but in the meantime we should not let them obscure the very real poverty of much of parapsychology. We should not tempt others into our field under false pretenses by claiming revolutionary status and failing to stress the lack of progress made in one hundred years.

The recommendation with which I disagree most strongly is that parapsychologists should "leave off" commenting on the question of survival after death. After all, it was the fundamental question to many of the early psychical researchers and is, I suspect, still so for many parapsychologists today.

Of course inferences drawn from "survival evidence" are speculative, and of course the phenomena are "widely open to various kinds of alternative explanations," but that is no reason to abandon the whole enterprise or to stop talking about it. We should not make the mistake of assuming that just because there are alternative explanations they are necessarily

preferable. I am sure that Hövelmann would agree that "explanations" in terms of psi are "speculative," too, and those in terms of physiology and psychology are as yet extremely sketchy and primitive. Rather than dogmatically rejecting any of them, I think we should pursue all to see where they lead and which is more productive.

I wondered whether Hövelmann is a little too emotionally involved in arguing against survival because some of his further reasoning is rather strange. He says that "survival cannot be regarded as proven" and suggests that we need "definite scientific proof." But since when has proof been necessary for scientists to express themselves on any hypothesis? Indeed would anyone seriously support the contention that "definite scientific proof excluding any alternative explanations" is possible, let alone a desirable objective? I think not. What we need is research on all the alternatives so that we can assess which is preferable or, in Lakatos' (1978) terms, which research programme provides a more progressive problemshift.

We may indeed survive death. Personally I prefer to concentrate on trying to find psychological explanations for some of the phenomena. However, like many other parapsychologists over the past hundred years, I am interested in the question of survival and hope that we shall eventually be able to answer it. We shall not do that either by demanding proof or by ignoring it.

Finally I would like to add a recommendation of my own for the future practice of parapsychology, rather than ever chasing the negatively defined and elusive "paranormal," we should try to understand our whole range of allegedly paranormal phenomena regardless of which type of explanation turns out to be most useful.

I have previously made this suggestion in the context of OBE research (Blackmore 1982a.) Psychological theories of the OBE are now being developed. They are not yet sufficient to challenge any other theory very seriously, but in future they well may. I have outlined some ideas about the new areas these may take us into (Blackmore 1982a,b). In that case parapsychology has the choice between rejecting these new theories because they no longer consider the OBE to be paranormal, or of extending its research into the new direction and abandoning its strict adherence to the paranormal. In the first case we run the risk that others will forge ahead with new research programmes on OBEs, altered states of consciousness, hallucinations and so on, while the parapsychologists will stick to the outmoded and stagnant research programmes of a hundred years ago, so further cutting themselves off. I would far rather we shared our expertise and knowledge and followed very promising route wherever it leads, even if that means away from the paranormal.

This argument can be extended to many other phenomena such as NDEs, poltergeists, apparitions and so on. The big question is whether it can be applied to all of parapsychology's subject matter. Possibly it can. Zusne (1982) has applied the term "anomalous psychology" to the study of "human behaviour and experiences for which paranormal or occult causation is claimed and which appear to violate some of the basic principles on which nature is known to operate." If research in this area provides a progressive research programme, then it may seriously dent the phenomena remaining to parapsychology. If this happens then

parapsychologists will either have to join ranks with anomalistic psychologists or be left with almost nothing. Parapsychology's only hope lies in following the trail wherever it leads. Whether that will be towards "the paranormal" or away from it, only time will tell, but we'll get to the revolution in the end !

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COMMENTS BY H.J. EYSENCK:

It seems to me that on the whole these recommendations are quite reasonable, but some of them may require some comment as the recommendations themselves, and their implications, are not entirely clear. Let us look, for instance, at Hövelmann's fourth recommendation, namely that parapsychologists should cease to pretend that they are able to explain anything by means of their present terminology which is merely descriptive. This raises the philosophical problem of causality, and does little to help the parapsychologist overcome the problems originally raised by David Hume in this field.

To what extent, we might ask, does the terminology involving gravitation explain the phenomena of falling apples, circling planets etc.? There certainly is no agreed theory of gravitation; ever since Newton postulated "action at a distance," without really believing in it, there have been different theories of gravitation, none of which has been able to attract majority support. There is now, for instance, Einstein's theory of attraction between objects in terms of a warping of space time, and there is, on the other hand, the completely incompatible quantum mechanics theory in terms of gravity as a function of particle ("graviton") exchange. Thus when we ask for a definition of gravity, we get one of three different answers. We may simply be referred to the actual phenomena which the concept exists to deal with, explain and predict, i.e. the falling of bodies. Otherwise we may get a theoretical explanation in terms of concepts like gravitation (Newton), graviton (quantum physicists), or warped space-time lines or faults (Einstein). Last but not least, we may be given a formula which tells us how to measure the force involved, i.e. we are told that the concept can be defined in terms of its measurement. It is not clear how any of this explains the phenomena, or how we can postulate a causal chain which is not subject to Hume's criticisms.

Parapsychologists are simply following in the tradition of the physicists in their use of terms, with the exception of course that as much less is known or established in their field, and consequently terminology is used much more loosely. However, that is inevitable; Newton was criticised in much the same way for his use of the term "gravitation" by the French physicists who accused him of lack of rigour! It is not clear to me how parapsychologists can avoid the use of terms in a semidescriptive sense which some people no doubt will interpret as being explanations. The whole concert of explanation and cause is much more complex than Hovelmann seems to realise.

Hövelmann's fifth recommendation is that: "In view of the frequent inconsistencies of their experimental findings, parapsychologists should not resort to the fatalistical conception that these inconsistencies are necessarily constitutive of paranormal events." I think parapsychologists are probably justified in coming to this conclusion, simply because inconsistency of findings is a usual and may be a necessary consequence of not knowing what are the chief parameters that ought to be controlled in given experiments. Inconsistencies may result (and this has been amply documented) from the simple fact that different people react differently to the experimental situation. Thus extraverts seem to do better in parapsychological experiments than introverts; sheep than goats; etc.

Experiments which are boring to the individual may give results different from experiments which are interesting to the individual. Lengthy experiments lead to fatigue effects which, depending on the length of the experiment may lead to positive, negative, or indeterminate findings.

Hövelmann is certainly right in thinking that parapsychology "may not yet be sophisticated enough to allow a water-tight explanation of psi functioning." Of course it would be too much to ask for such a water-tight explanation of psi functioning; after all, we do not have any water-tight explanation of psychological functioning generally, or even the functioning of physical systems! I don't think it would be true to say, as Hövelmann seems to suggest, that parapsychologists "overhastily abandon the concept of lawfulness in their field." I think they are searching for lawfulness, but because of the complexity of the situation which constitutes the background of most experiments, and the lack of knowledge about the parameters to be controlled, inconsistency must be expected to be the rule, rather than the exception. To be actually pleased with, and proud of these inconsistencies would of course be foolish, but I don't think many parapsychologists would fall into this trap.

With regard to Hövelmann's first recommendation, I am not sure that this is realistic. Copernicus realised that his views were revolutionary, although of course the evidence in their favour was pitifully poor; equally, Galileo and Kepler were fully aware of the fact that their views were revolutionary. Of course the views of parapsychologists are revolutionary, in the sense that if their findings are true, they are incompatible with many of the teachings of orthodox science at the moment. Parapsychologists have not taken a programmatic decision to revolutionise science, as Hövelmann suggests. The revolutionary role is forced on them by the nature of their data.

Hövelmann says that parapsychologists cannot at the same loudly propagate revolutionary slogans, and also claim to be rigidly applying

orthodox scientific research methods. I fail to see any contradiction in these two aims. Copernicus, Kepler and Galileo claimed to be both revolutionary in their findings and theories, but orthodox in the application of scientific methods. After all, revolutions in science can only be produced by the use of universally agreed methods, and one must distinguish between methods and findings. The latter can be revolutionary, whereas the former need not be; there is no contradiction implied.

On the whole Hövelmann's recommendations are sensible and may serve the purpose of public realtions well; they do, however, to some extent raise philosophical problems which make some of them less acceptable to parapsychologists than might otherwise be the case.

COMMENTS BY PIET HEIN HOEBENS:

Readers unfamiliar with the contemporary German psi-scene may not fully appreciate the iconoclastic nature of Gerd Hövelmann's "Recommendations." For decades, psychical research in the Federal Republic has been dominated if not monopolized by the amazing Professor dr. phil. dr. med. Hans Bender. The Benderian Credo may be summarized as follows:

- * Psi exists;
- * Persons who doubt this should see a psychoanalyst to be cured of their prejudices;
- * The "qualitative" evidence as provided by miracle men such as Gerard Croiset is, in itself, conclusive;
- * The reality of psi has revolutionary implications for our views of Man, Nature, Science, the Universe, God etc.;
- *Parapsychology is the most effective antidote to the mechanistic-reductionist poison.

Now Mr. Hövelmann cheerfully urges his fellow parapsychologists that they give up their revolutionary pretentions, forget about "qualitative" evidence, shut up about Man, Nature etc., seek cooperation with the pig-headed skeptics and conform to the mores of materialist establishment science. From Bender's point of view, Mr. Hövelmann's paper could hardly have been more subversive. It is an unambiguous expression of support for the "new conservatism" in German parapsychology, as exemplified by Eberhard Bauer, Klaus Kornwachs and Walter von Lucadou.

I find myself in basic agreement with most of what Mr. Hövelmann says, so I will restrict myself to a few marginal comments.

Ad Recommendation 1: I am afraid that Mr. Hövelmann is liable to be misunderstood on this point. There is nothing wrong with emphasizing the possible revolutionary implications of "psi," and I will not blame parapsychologists for having a "revolutionary outlook upon their field and upon themselves" if they so wish. It is a very different thing, however, to excuse the shortcomings of modern parapsychology by appealing to the revolutionary nature of the psi paradigm. If that is what Mr. Hövelmann intended to convey, then I have no quarrel with him.

Ad Recommendation 2: Although my metaphysical predilections are definitely goatish, I cannot entirely share Mr. Hövelmann's strong feelings against claims of "survival." His Occamist arguments against the Life-after-Death hypothesis could equally well be applied to the psi

hypothesis. In both cases, the available evidence can in principle be explained without recourse to the hypothesized factor. "Survival" may just be ESP, and ESP may just be fraud and delusion. I am not sure that "super ESP" is necessarily a more parsimonious explanation than is "survival." As far as I am concerned, both seem somewhat implausible. I do not expect to "survive" in any meaningful sense of the term. Neither do I expect that, after my death, some urchin will superparanormally reconstruct my dissipated personality to amaze Professor Stevenson with yet another case suggestive of reincarnation.

The problem is not so much that survival is a supernatural and untestable idea, but rather that the available evidence is hopelessly weak.

However that may be: I expect that Mr. Hövelmann's second recommendation will be happily ignored by those psychical researchers whose interest in the field mainly stems from the need for scientific reinforcement of a basically religious belief.

Ad Recommendation 6: Of course, I applaud Mr. Hövelmann's call for closer cooperation between proponents and critics. However, I would deplore if parapsychologists were to restrict their tolerance to those critics who have managed to convince the psi community of their friendly intentions. It is most flattering to myself that Mr. Hövelmann regards me as a responsible skeptic (and even places me in the distinguished company of Professor Hyman and Professor Truzzi), but I am not sure that I would like to be flattered at the expense of some of my more radical fellow skeptics. There is a clear implication in Mr. Hövelmann's paper that Professor Hansel's critique has not "really come down to the problems inherent in parapsychological research." Now I am sure that Professor Hansel's writing can be challenged on several points, but I continue to think of ESP and Parapsychology as one of the most important, relevant and rational contributions to the psi debate. Parapsychologists cannot afford to ignore his criticisms. The same is true, if perhaps not to the same degree, for those other betes noires of parapsychology, Mr. Gardner and Mr. Randi. Mr. Hövelmann will appreciate that I pointedly exclude Dr. Wimmer.

Ad Recommendation 7: I suspect that this recommendation may have been specifically meant to infuriate the editorial staff of the neo-obscurantist magazine Esotera and Esotera's pet parapsychologist, Mr. Elmar Gruber. Actually, it is a tricky one. I quite understand why responsible parapsychologists wish to dissociate themselves from the crackpots, but they should beware lest their arguments backfire. Mr. Hövelmann seems to think that "pseudoscientists" are characterized by their predilection for "untestable ideas." How "testable" is the idea of psi? Granted, it is easier to think of a rigorous experiment for testing psi than, say, an experiment for testing Cosmic Awareness. However, psi is testable only in a very restricted sense. For example, if a researcher consistently fails to find a trace of the paranormal, he or she is not allowed to conclude the nonexistence of such phenomena.

On the other hand, some patently pseudoscientific claims are highly testable. Take Mr. Vandenberg's claim that several egyptologists, whom he names, died prematurely soon after having opened King Tut's tomb. Or take the claim that an iriscopist can see what's the matter with your feet by staring at your eyes. What's wrong with these claims is that they're wrong - not that they are "untestable" or "unfalsifiable."

So may I suggest an amended version of this recommendation? What I have in mind is something like this: "Parapsychologists should disassociate themselves from claimants who, appealing to a Higher Sort of Science, demand that their claims are accepted regardless of the scientific evidence." I think that this includes most if not all persons whom Mr. Hövelmann would rather not be seen with. Come to think of it: in their eagerness to appear "respectable" some parapsychologists seem to want to out-Gardner Gardner when it comes to summarily dismissing other people's pseudoscience. I have met psychical researchers who think it all right to believe in Ted Serios but scoff at the nonsense of ufology. I suggest that no parapsychologist has the right to snub Mr. Hendry.

I cannot share Mr. Hövelmann's enthusiasm for Dr. Beloff's recent proposal. I predict that the establishment of a scientific committee a la Beloff would simply result in yet another controversy.

Concluding remark: apart from these minor criticisms I find it difficult to disagree with Mr. Hövelmann. Which, I trust, will suffice to confirm Professor Bender's worst suspicions.

COMMENTS BY BRIAN INGLIS:

Taking Hövelmann's recommendations one by one:

1. It is possible to be revolutionary and scientific at the same time; Einstein was, and so were the quantum physicists. The problem arises when the findings of parapsychology subvert, rather than simply modify, scientific method. Thus ESP subverts experiments, if it exists, by bypassing controls; PK even more drastically.

2. Of course the evidence which psychical researchers have unearthed pointing to the reality of survival after bodily death should not be presented as proof; but it is surely absurd to argue that it should not be presented at all.

3. To argue that anecdotal evidence is inadmissible is a piece of behaviorist eccentricity which has done a great deal of damage in this area of research. You might as well investigate marriage but exclude the existence of love at first sight -- far less well-attested, incidentally, than ESP.

4. We do not know what magnetism is; and nothing is explained by referring to a "magnetic force." It is a convenience, however. So is "psi."

5. If consistent inconsistencies emerge, such as the decline effect, it is legitimate to regard them not, indeed, as proof of psi, but as its common accompaniments.

6. In theory, fine; but in practice this all too often simply does not work. The main problem is with those psychologists who pretend they have open minds, and often believe they have. They are fearful time-wasters, as they will always find some flaw in the protocol, retrospectively to account for positive results -- even in their own protocols.

7. Where does parapsychology end, and parapornography begin. Many, perhaps most, parapsychologists think of physical mediumship -- the medium exuding ectoplasm through her bodily orifices, and creating materialised forms out of it -- as beyond their Pale; yet the evidence for this type of phenomenon is strong. Poltergeists and UFOs are other examples of phenomena which are often repudiated at "vulgarisation." It is worth remembering that the reality of the mesmeric or hypnotic trance state was rejected on this score for over a century.

To sum up: parapsychologists should do their best to convince scientists of the reality of psi with the help of accepted scientific methodology; but they must not allow scientists, let alone sceptics, to apply procrustean research regulations of a kind which do not accord with the known, or presumed, facts about psi.

COMMENTS BY JURGEN KEIL:

"Some Doubts about the Hövelmann Recommendations"

Hövelmann's recommendations appear to be reasonable on first sight but need to be scrutinised more closely before they are endorsed too enthusiastically. In general terms there is the difficulty that Hövelmann does not clearly distinguish between (1) strategies which are supposed to make a favourable impression on some groups in the community (e.g. (a) scientists who read Science (b) scientists who read the Skeptical Inquirer and (c) scientists who read The Zetetic Scholar - there may be some overlap, but there may also be significant differences between the groups) and (2) strategies which are in agreement with scientific methodology and a particular philosophy of science point of view. To illustrate this it could be argued that some particular PK research involving metal bending could be rejected because of (1) even though it could be justified under (2).

I would agree as Hövelmann seems to imply that science is in some sense a social activity which to some extent depends on expectations, beliefs and assumptions which are based on more than scientific work carried out by scientists. Consequently I agree that (1) can be important. But it is also important to be fully aware when a particular decision is made primarily because of (1). Otherwise rationalisation as a form of self deception may distort the assessment of research possibilities and may discourage promising research even in places and institutions where there is no particular need for restraints because of (1). At any rate the hopes and expectations in connection with research which can be justified under (2) have to be evaluated against the possible disadvantages which might result from unfavourable comments, ridicule and other negative reactions under (1) before a decision is made whether a particular line of research is to go ahead or not. Such evaluations are largely subjective affairs and Hövelmann can hardly expect to find evaluation criteria which are widely accepted and which can be uniformly applied.

On the one hand Hövelmann is interested to create a favourable impression among scientists and this suggests an operational definition of science, i.e. science is what scientists do; on the other hand, he also rejects some widely held views (by scientists) when he argues that scientific research must have a concrete purpose.

Hövelmann mentions a range of philosophy of science frameworks which scientists can adopt (from Popper to Feyerabend) and this range could be extended further, but his comments are presented as if there was a unified view in some relationship to, or in agreement with, his arguments. Occam's razor is mentioned but not that in the life sciences it is to some extent a matter of subjective judgement whether two hypotheses with different complexities account equally well for some pattern of behaviour. Consequently it is more difficult than Hövelmann suggests to reject the more complex one because it could be argued that only the latter is giving an adequate account of the behaviour under investigation. It could also be argued that the relatively slow progress in orthodox psychology is partly due to Occam's razor and a resulting expectation that simple expressions will eventually be found to account for the behaviour of living organisms. It is possible that the previous success in the hard sciences has created

wrong expectations for some areas in the life sciences. This does not mean that we should look for little green men from Mars behind every PK phenomenon but we should not reject the possibility of relatively complex relationships between psi phenomena and other variables which might never be discovered if investigations are carried out within a framework of low level complexity.

In response to some of Hövelmann's seven recommendations (abbreviated here as H1; H2; --- H7) the following additional points may be raised:

(H1). Hövelmann agrees that parapsychological research might lead to a "scientific revolution" in the Kuhnian sense. Whether one talks about it or not is tied up with what above I called (1) strategies. I would not be unhappy if parapsychologists talked less about revolution and more about cooperation with orthodox science, but I cannot see any objective criteria in favour of this view which I would regard as particularly convincing. I agree, of course, that parapsychologists have adopted an orthodox scientific methodology but what is investigated is by definition unorthodox.

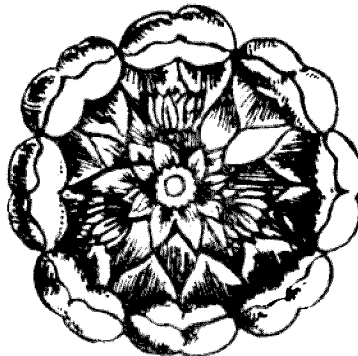
(H2). This question is related to a more general one, that is, how far specific and necessarily limited research findings should be interpreted (even if speculatively - there is little certainty even in the orthodox life sciences) in such a way that it can be understood by the general public. I believe scientists have a responsibility to be cautious in their interpretations but they also have a responsibility to be open about their research and that involves discussions about the possible wider implications of limited findings. This openness can lead to problems but secrecy or refusal to link limited experimental results to the complexities of life and death can lead to even more confusion and problems than might otherwise emerge. I do not believe that people necessarily hope for life after death as Hövelmann seems to suggest and although I have not seen any research which compels me to abandon my own belief that survival of bodily death is highly unlikely, I nevertheless recognise that what looks more or less reasonable depends to some extent on the community within which we live as well as on the definition of survival of bodily death.

(H3). I can happily agree with Hövelmann's expression "not too heavily" but this may mean different things to different people. Hövelmann seems to be in full agreement with criticism by Hoebens which I find difficult to evaluate since the events go back such a long time. Before Hoebens is too satisfied with finding a world free of psi events, he should probably carry out a similar investigation of an orthodox psychological claim which was made a similar number of years ago, and I would not be surprised if similar problems emerged. Hoebens only selected "chair" tasks which had methodological weaknesses but did not discuss improved tests carried out in Freiburg.

Given that parapsychologists are dealing with questions which had arisen out of life experiences, the investigation of spontaneous cases seems a promising strategy because many distortions which cannot be prevented in a laboratory setting are avoided. With modern monitoring equipment and statistical evaluations it should also be possible to reach high research standards when spontaneous cases are investigated. Indeed it would be a mistake to assume that research carried out in a laboratory is superior simply because of the laboratory setting.

(H4). The terms used by parapsychologists are not ideal as has been noted before. Different terms used in East-European countries have not found much favour in the West. I also see some need to communicate with the public, which becomes more difficult if entirely abstract terms are used. At any rate, new terms may have a better chance of being adopted when research has advanced to a point where detailed reliable findings reveal significant aspects of psi. In the meantime I do not see an urgent need for change.

(H6). I agree that a good deal of useful criticism is generated within the field. I also agree that parapsychologists should take some notice of the more reasonable critics from outside their own field. But often it becomes a public relations exercise rather than a useful discussion on how to improve a particular research procedure. Some response to reasonable criticism may be highly desirable and may in some circumstances provide the foundation for continued research. But parapsychologists must also ask themselves how much time and energy should be spent on such activities. There are only a small number of part or full time parapsychologists and the situation could arise where they spend all their time debating various issues instead of carrying out research.



COMMENTS BY STANLEY KRIPPNER:

"Three More Recommendations for Parapsychology's Future"

Gerd H. Hövelmann's recommendations contain so much of value that it may come as a surprise that he is one of the newest members of the Parapsychological Association. Although most of his points have been made by other parapsychologists over the years, Hovelmann has organized them beautifully and has argued for them eloquently. I can disagree with any of his suggestions, but I would like to react briefly to each of them.

1) One continuing problem in parapsychology is the proclivity of some zealous members of our enterprise to make claims and insinuations which exceed our data base. For example, we do not really know if psi phenomena will demand a revolutionary change in scientific outlook. Perhaps psi will turn out to be an example of hitherto undetected interpersonal expectancy effects (Rosenthal & Rubin, 1978). If so, this finding will represent a major advance in the social and behavioral sciences, but hardly one that could be considered "revolutionary."

2) The question of life after death is an important one; indeed, it is hard to imagine a topic of study with more serious consequences. However, Hovelmann is correct in his description of the data as unreliable, ambiguous, and inconsistent. An admirable review by William Roll (1982) supports this assessment. For all we know, these data may ultimately be explained by ordinary means or by a completely different approach, such as the "morphogenetic fields" proposal (Sheldrake, 1981). In any event, it is more appropriate to gather additional data than to make definitive claims about this provocative topic. On the other hand, I must take issue with Hovelmann that speculation on the nature of the universe should be reserved for aging Nobel laureates. I see nothing amiss with philosophical speculation as long as it is clearly labelled as such with cautionary caveats. Sometimes these speculations can lead to experiments which can move the field forward.

3) I feel that Hövelmann is basically correct in his position on spontaneous cases. However, these cases do serve one function that he alludes to. A scholarly assessment of the cases sometimes can produce patterns that illustrate directions for future research (e.g., Rhine, 1954).

4) I share Hövelmann's concern for a terminology which is descriptive. A label such as "extrasensory perception" will appeal to the media but, in the end, may be ill-advised. Perhaps we are dealing with extended sensory perception. Or perhaps clairvoyance, precognition, and telepathy will be found to have nothing to do with perception at all, making the sensory model inappropriate. Personally, I feel it would have been helpful if Thouless and Wiesner's (1974) terms "psi-gamma" (ESP) and "psi-kappa" (PK) had won acceptance.

5) Inconsistencies in experimental findings are the norm rather than the exception in parapsychology. But at least they demonstrate an attempt

on the part of parapsychologists to establish some sort of reliability. Replication studies are not undertaken as often as they should be in the social and behavioral sciences. When replications are attempted of complex behavioral or social phenomena, the data are frequently contradictory; "memory transfer" experiments (McConnell & Malin, 1973) and hypnotic "age regression" studies (Barber, 1969) are but two examples.

6) The issue of criticism is handled well by Hövelmann. I would agree with him that the new crop of critics is both more knowledgeable and more responsible than the abominations to which parapsychology has been subjected in the past. Indeed, I would urge more skeptics and more critics to write for parapsychological journals and even to join the Parapsychological Association. There is no reason why one has to be convinced of psi's reality to be a parapsychologist. As the advertisement so well puts it, "You don't have to be Jewish to like Levy's rye bread." Furthermore, there is no reason why criticism could not be looked upon as a legitimate area of study within parapsychology itself.

7) Hövelmann's final recommendation is both admirable and difficult. "Occultists" and "supernaturalists" frequently cite parapsychological research findings in an attempt to support their own world-views. Parapsychologists can not be held responsible for the use to which their data are put, but at the very least they should not conspire in these uses.

Hövelmann's list is excellent but I can not resist adding a few recommendations of my own:

8) Parapsychologists should provide complete data when publishing an experiment. This procedure will not only make replications easier but will prevent critics from making unjustified statements. For example, in writing up a dream ESP study some years ago, we (Ullman & Krippner, 1970) observed that the agent was encouraged to write down his associations to the target picture which he was attempting to telepathically transmit from a distant room to a sleeping subject. C.E.M. Hansel (1980) jumped at this statement and suggested that an experimenter had done the encouraging, stating, "an experimenter appears to have been with the agent when he opened his target envelope" (p. 246). This allegation was not true, and previous papers had stressed the pains taken to keep the experimenter and agent separated. However, our report did not clearly state that the agent was encouraged to write down the associations before going to the private room, or that he was further encouraged by written directions found in the envelope once it had been opened and the target picture revealed. It is true that some journals will not publish complete procedural descriptions, but a footnote could be added stating where the complete experimental protocol is available, upon request.

9) Parapsychologists should spend more time replicating each other's work. It is true that funds for psi-research are extremely limited and it comes as no surprise that experimenters are eager to break new ground with the little money they have. Nevertheless, the field needs a greater emphasis upon exact replications of important studies, but conducted in different laboratories by different investigators.

10) The field suffers from a lack of long-range planning. It is quite true that five-year plans are difficult to make when one's laboratory is only assured of funding for three years. However, the field will not move ahead very quickly if the research continues to be done in fits and starts, and by bits and pieces.

In conclusion, I repeat my congratulations to Hövelmann for his stimulating suggestions. I hope that they will be read, debated, and taken seriously by other parapsychologists as their potential benefit for parapsychology's future is clear. One does not have to be precognitive to appreciate the benefit of these recommendations for the future practice of parapsychology!

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COMMENTS BY MORTON LEEDS:

1. Abandon the revolutionary outlook.

There is a difference among revolutionary outlook, revolutionary means and revolutionary implications of one's work and endeavors. Hövelmann's recommendation is to abandon the revolutionary outlook. Some workers have this, many do not. None utilize revolutionary means; the usual worker utilizes scientific methodology as it is currently understood. Most are aware of the revolutionary implications of the field's endeavors. Ultimately for the scientific endeavor, revolutionary outlook is irrelevant so long as valid scientific methodology is employed.

2. Drop the problem of survival after death.

It is probably far too early in the history of psi research to be tackling this issue, so Hövelmann has a point. Of course, alternative explanations to survival must be considered first, and Occam's Razor continues to be valid. Still, all issues remain legitimate for scientific examination and survival is one of them.

To me, Hövelmann seems to be suggesting: "Well, maybe the earth is not the center of the universe, but let's keep the sun out of the discussion. It has to go around the earth in any final picture you may draw." One cannot predict what a more complete understanding may portray, but we should not create automatic exclusions based on current ignorance. Of course, it may be more politic to drop discussion of survival, but that's not the problem ultimately.

3. Do not rely too heavily on personal evidence.

Increasingly, Hövelmann is getting his way. The most interesting material, by far, comes from spontaneous, personal experiences, and it continues to provide the main drive, outside of ongoing scientific curiosity, for this kind of study. Time should take care of this, as the scientific approach gradually takes over.

4. Build up a standardized, methodically constructed terminology quickly.

Again, this is happening everywhere in the scientific study of psi. It is a field that is under-funded, spread very thinly, with only a few hundred researchers around the world working on it. They communicate very quickly, compared with some other areas of study and their terminology is becoming very uniform, at least among the English-speaking peoples.

5. Don't assume paranormal phenomena when inconsistent findings appear.

This charge may be valid, since our ignorance of what is really happening is still very large. We need to continue to refine and perfect both our theories and our practice.

6. Consider the arguments of their critics and collaborate with the scientifically minded.

Hövelmann has chosen to ignore the literature if he says this. Perhaps 80 percent of the average researcher's energy goes into answering the critics.

7. Parapsychologists should separate themselves from the quacks.

This is more easily said than done, especially in an area in which so few hard facts are known. The pseudoscientists cling like fleas to a dog. Ultimately, the parapsychologists are doing the right thing: they continue doggedly to work at their theories, testing and reporting, using the best of scientific technique that they know.

I'm not sure that a Commission would do much more than a whitewash of current dominant beliefs in this field. As such, it could be extremely damaging. Rather, the continued slow, steady growth of technique and knowledge of psi is the best process, by far.

COMMENTS BY WALTER V. LUCADOU:

When I read through Hövelmann's recommendations the first time, I thought that it is easy to agree with every point; and I was especially pleased with the provocative style of his presentation because I believed that it could awake some parapsychologists (especially in Germany). But later I became worried with the question whether the paper will serve its purpose. Those persons who try to investigate the matter of parapsychology on a scientific basis will of course recognize the intentions of the recommendations and will acknowledge them. But unfortunately those parapsychologists who feel themselves criticized will find several loopholes in Hövelmann's argumentation, and they might try to use them as justification for their own attitudes.

I generally doubt whether such recommendations will be useful for the evolution of science. Successful scientists have very often ignored requirements put forward by philosophers of science. Certainly this does not mean that "anything goes" because to be successful they must convince their colleagues, the so-called scientific community. Parapsychologists, however, were not very successfully in doing so until now. (Sometimes one gets the impression that the lack of success of some researchers is proportional to their interest in philosophy of science). Furthermore the rules of science do not only consist of a catalogue of requirements and standards which serve as a kind of meta-methodology, but they also contain some kind of unwritten social rules which cannot be required explicitly without violating them simultaneously. Unfortunately they are often disregarded, especially in this field (see footnote 1 in Gerd Hövelmann's manuscript). There is no other discipline where gossip or the so-called backstage information plays such an important role as in parapsychology. Since the discussion on parapsychology is very often connected with strong emotions of both protagonists and antagonists as well, such social factors often dominate the debate; and even the fulfillment of all the recommendations of Gerd Hövelmann would not alter the situation too much, I am afraid. Nevertheless I am in basic agreement with Hövelmann, and I will give only some comments which may improve his arguments.

Ad 1: It is true that parapsychology is not a revolutionary or alternative science, but it is investigating "anomalies" of a rather general type. The aim of parapsychology is to describe and to understand these anomalies. If a present theory, for instance physics, could explain them, nothing would be revolutionary (it is a task of parapsychologists to find such explanations). If we will find a new theory, this theory might be revolutionary but not the phenomena described not the field of science which has developed the theory. Thus quantum theory was a revolution in physics, but physics itself is no revolutionary science.

Ad 2: I think it is legitimate for both scientists and non-scientists and not only for aged Nobel laureates to pose the question of survival (this is an old question of mankind). But due to the lack of proper methods and very ambiguous data, it may be a fruitless work. I think it would be more economic first to solve the problems of psi. The question of survival per se is not unscientific even if it may be unsolvable.

Ad. 4: It is true that psi does not explain anything; nevertheless it could well be the case that one sort of an operationally well-defined anomaly (such as a card guessing experiment with a significant result) could be described in terms of another sort of anomaly (for instance a significant result with a Schmidt-PK-machine). Such descriptions are called phenomenological models, and there is no reason not to try to find such models. They can help to find out relationships between different sets of data.

Ad. 5: Experimental results cannot be inconsistent per se. They can be inconsistent in relation to a prediction or a model or a theory. Inconsistency is a property of models, not of phenomena. Thus inconsistency cannot be constitutive for paranormal events. Nevertheless such inconsistencies indicate that a model or a presupposition must be wrong. In parapsychology and other fields of science, very often there exist underlying models which are not formulated explicitly because they seemed to be obvious. In classical physics the notion of absolute space and time was taken as obvious, and it led to inconsistencies. From our point of view "information transfer" is such an underlying model of ESP-experiments. It may lead to inconsistencies. The purpose of our theoretical contribution (footnote 20 of Gerd Hövelmann's paper) was to introduce new concepts which avoid and hence explain inconsistencies such as the alleged elusiveness of psi phenomena. This however does not mean that the model is abandoning lawfulness. Quite to the contrary: it imposes, hypothetically, a law on hitherto inconsistent experimental findings (for instance by our proposed uncertainty relation).

Ad 7: I do not believe that John Beloff's recent suggestion to install a commission will solve the problems of parapsychology. We have already had such commissions. The English SPR was the first one, and the CSICOP will not be the last one. Similarly, there will never be one experimentum crucis which will lead to the conclusive evidence of psi. Science is a social and historical process, and any knowledge or evidence does not come from single experiments or single experimenters. Every experiment, however, should be done as well as possible, and every experimenter should work as carefully as possible. Nevertheless, there will be always questions remaining open. The history of science has shown that even the solution of rather tiny problems needs a lot of time. Thus, we should be more modest and more patient, especially in the field of parapsychology.

COMMENTS BY GERALD C. MERTENS:

"A Missing Recommendation, but Right On!"

Truzzi¹ proposed a "Hard Line Continuum" in reference to the various stands taken on the paranormal. I have attempted to illustrate this continuum below, as well as adding others in. Those in the (parentheses) I have added:

The Hard Line Continuum:

(Mertens) Skinner, (Randi) Hansel---Kurtz---Truzzi---(Hövelmann)---Softer
(Price) Hyman Line

I would place myself to the left of Randi and Skinner on the hard line suggested by Truzzi. I mention this at the outset of these² comments only to point out to readers my position. To me, Randi² still has too many minds and other such mentalistic cognitive psychology spooks running around in his debunking writing. I see mentalistic cognitive psychology and sociology and the pseudo science paranormal position as a continuum. Skinner has had too much "trust" in E.S.P. writing, per se, and too much aesthetic training in his own personal learning history influences his writing. All of this is on top of a real reservation about this "global approach" to the hard line continuum, as no person is "pure" in his or her total repertoire to warrant placement as a single point on such a line.

As a Skinnerian behaviorist of the "hard nose kind," I have come to the position that the shaping principle is one of the most neglected of the behavioral principles. The shaping principle simply stated contends you would take a person's repertoire where it is at and build by positive reinforcement from that point. Holding to this position on the shaping principle, I do want to heap praise (and all other kinds of positive reinforcers) on Hövelmann's well developed set of recommendations. The recommendations approximate the direction I believe the study of the paranormal area needs to follow.

I would go beyond this in my praise of Hövelmann's recommendations. The recommendations are generally good for all those who offer explanations for human behavior. That is, I believe all of psychology, sociology, psychiatry, etc. could benefit from attention to these recommendations. I believe when Hövelmann's recommendations are followed, we already have a lot of data to tell us what we are dealing with.

However, I do contend Hövelmann has made no recommendation in the area where a recommendation is most needed.³ I believe the recommendation needed most is one which helps to insure the study is free of fraud, dupes, getting excited over "chance", exaggerations, or the "real world principles" (laws of science, if you prefer) at work in the situation which go unnoticed and/or unreported. I still believe

E.S.P.'ers in general are laughing all the way to the bank with our money, time, and effort. I, at least, hope they are laughing. Why shouldn't one be willing to pay admission to see: a good con artist, a salesman doing the super sale, good skill at human exploitation of "the small kind," the excellent B.S. artist, etc.? These are real works of art. (Why not a better practice than paying admission to see someone blow wind and spit into a piece of metal per written script (notes) which usually the person has not memorized, but is only reading?) All one has to do to appreciate this novel art form is to divorce himself or herself from the fact that they personally are reinforcing a successive approximation of creeping "irrationality" toward the likes of a Holocaust and Jonestown. Kurtz put it nicely when he said, "There is always the danger that once irrationality grows, it will spill over into other areas. There is no guarantee that a society so infected by unreason will be resistant to even the most virulent programs of dangerous ideological sects."⁴ One continually confronts a possible compromising Catch 22-like problem on the attention variable. One may have to attend to the undesired behavior to observe and study it. On the other hand, the attention may serve as a reinforcer for the undesired behavior. How much of any problem is generated by one's own behavior is always a question. Autobiographically I remember the struggle I had to pay to see the supposed prime psychic of our time. On the one hand I wrote and spoke of him with total contempt, and I figured I should see, in person, what I had such a distaste for based on reading literature and hearing friends relate their personal experiences with him. On the other hand, giving \$50 to see him broke "my heart" and pocket book. After watching this supposed prime psychic of our day I found my own response to him most interesting. I was tempted to reduce my contempt for the guy. Anyone who can come into a "dress up affair" in a less than casual manner, and then proceed to do what he did in the name of paranormal deserves to laugh all the way to the bank with anybody's \$50 if they (including me) are dumb enough to pay it. (Can a person who dresses like that be all bad?)⁵

After the long history of hoaxes in the paranormal area, and the countless replication of the orderliness of the world in science, some of us need assurance that when we read in the paranormal area that attempts to control fraud have been made. In terms of time expenditures in one's life, I find one cannot be "open" (whatever that means) to any wild view of the universe. "Be open" has become a trite statement the way it is used. I believe it is more than a play on words to say, "It will take strong evidence for me to reopen my effort to assess in this area." Be it because of the "sins of their ancestors" (hoaxes of the past) or for other reasons, many individuals' studies in the paranormal area need to convince the reader what fraud precautions were taken. This may bring charges of excessive requirements of researchers in this area compared to other areas of inquiry. Personally I don't happen to believe this is so. If it were, I believe it is where the past has led us. I like the way Houdini put it, "I have said many times that I am willing to believe, want to believe, will believe, if the Spiritualists can show any substantiated proof, but until they do I shall have to live on believing from all the evidence shown me and from what I have experienced that

Spiritualism has not been proven satisfactorily to the world at large and that none of the evidence offered has been able to stand up under the fierce rays of investigation. It is not for us to prove that the mediums are dishonest, it is for them to prove that they are honest."6

I don't believe Hövelmann's position will probably make it with his rank and file E.S.P. colleague. I fear he will find it lonely out there. Such loneliness can be relatively tough at times. For example, autobiographically I recall such a period when I was the only unbeliever at Maharisi International University for a week, or in 1958 when I was voluntarily flying high alone, on my government sponsored L.S.D. trip as part of the now infamous C.I.A. and military L.S.D. experiment. Even though on the "trip," I was in touch enough to know I was the only person in the room on the "trip." To all the Hövelmanns (Those using scientific methodology in their search of the paranormal and E.S.P.) who are willing to put the response cost in the continued search, if it gets lonely out there searching, remember there are others with you out there if only skeptically watching. I am part of the group who believes the data is in, but I will keep watching good research methodology. Looking to see if for the first time something happens. There is no such thing as a mind and nonentities cannot be opened, so I am not open-minded, but I will look at good strong evidence.

Notes and References

1. Truzzi, M., "A Skeptical Look at Paul Kurtz's Analysis of the Scientific Status of Parapsychology" Journal of Parapsychology V91. 44, March, 1980.
2. James Randi is one of the best psychologists alive, and he is not even a psychologist!
3. Sometimes I fear I missed a hidden agenda in Hövelmann. By-products of Hövelmann may well be the terminal behavior I would like to see in this area. Hidden agendas have hit me in the past as an aftereffect. For examples, as an undergraduate student, I had Margaret Mead up there on a pedestal. In my undergraduate school days I went to two national conventions of the American Anthropological Association to hear her. I was dumb-founded when I heard she had a role in getting parapsychology the honor and recognition of being in AAAS. Not too hidden of an agenda is that they now have to produce studies which meet scientific critics. Perhaps?
4. Time Magazine, December 12, 1977, p. 100.
5. I pride myself on being the world's worst dresser.
6. Houdini, Harry., Magician Among the Spirits, Harper & Brothers Publication, 1924, P. 270.

COMMENTS BY ROBERT MORRIS:

Mr. Hövelmann's recommendations can be considered both individually and collectively, and I will do the former.

R. 1. In principle I agree. Researchers of whatever sort who describe their work as revolutionary generally sound a bit like TV hucksters talking about a revolutionary new detergent. I think it is obvious to all that psi research may lead us to some quite new concepts having fairly strong impact on a variety of scientific disciplines, such that the term "revolutionary" may eventually come to be appropriate. Such a statement about implications of findings is not necessarily incompatible with the notion that the methodology involved is fairly orthodox, however. One can use orthodox methods to generate knowledge with nonorthodox implications. Personally, I'm more than content to let historians of science discuss what produced revolutions and what did not.

R. 2. I have mixed feelings here. On the one hand I agree that psi research at present has little to say about the survival issue, and I agree that many within the research community speak as though we know more than (in my opinion) we actually do. On the other hand, I don't feel that discourse on the topic should be stopped. People have had many experiences that suggest survival to them, and the research community, both parapsychological and non-parapsychological, has made some progress in developing alternative explanations for such experiences, progress which can be publicly disseminated. Secondly, further discourse on the problem may lead to the kind of theoretical sharpening that could be empirically tested and at least falsified. I see the survival question as involving a set of very general constructs which at present cannot be effectively tied to the existing parapsychological data base.

With regard to whether investigating the question is important and desirable, I think that such issues in any area of science are up to the individual researcher. Mr. Hövelmann seems to be saying that inquiry into the survival question is not appropriate because it does not involve concrete purposes. Although I'm not sure what he means by concrete purposes, I can think of two fairly concrete reasons to pursue research aimed at evaluating the evidence for survival: (1) Much of the evidence is drawn from anomalous human experiences; a further understanding of the factors that contribute to these experiences should help us to learn how to handle such experiences such that we no longer fear them and can employ them in useful ways. (2) Such research may lead us in new directions, opening up new areas of knowledge through systematic research. In each case we could benefit considerably whether or not we ever learn if humans in some sense survive bodily death.

One minor point; in his notes, Mr. Hövelmann expresses annoyance (and I share his annoyance) with some who took him to task for being too young to give recommendations to his elders, and notes that age should not matter. Yet on p. 3 (of my copy) he states, "Speculations of this sort should further on be reserved for aging Nobel laureates." Apparently age matters after all.

R. 3. I agree.

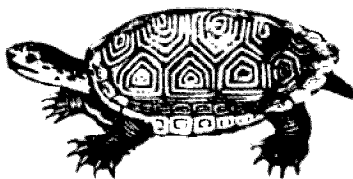
R. 4. I agree and applaud.

R. 5. I agree here as well.

R. 6. I agree in general, especially if the recommendation be broadened to read, "Researchers interested in the scientific study of anomalies should carefully consider the full range of arguments offered on a topic, and should seek active exchange and collaboration even with those with whom they appear to disagree the strongest." This advice is easy to give, and obvious, but not so easy to implement. Mr. Hövelmann suggests that eventually we may be able to give up "the unpleasant distinction between the parapsychologist and the critic." I suggest that we work hard on eliminating that dichotomy as soon as possible and as thoroughly as possible. As long as we categorize ourselves and let others categorize us, we will almost certainly be influenced by our perceived roles and will proceed competitively rather than cooperatively. When there are categories and "sides," there are winners and losers; those involved are likely to strive to win or avoid losing, rather than allowing a flexible exploration of issues by all concerned.

R. 7. I find this suggestion couched in ambiguous terms that need to be more sharply defined. It is certainly easy to agree with the notion that a responsible researcher should not lend public support to an organization or individual well known to be fraudulent. With less extreme cases, guidelines for conduct become more uncertain. Suppose I wish to learn about psychic development techniques so I can evaluate them experimentally. Am I being unscientific if I seek to interview trainers and trainees to gain a better feel for their procedures and claims? What if I enroll colleagues or students in a psychic development class, so they can see how it's done directly? Marcello Truzzi spent considerable time as a participant/observer in Anton LaVey's satanic church in San Francisco, doing sociological research. Was he being unscientific?

Although I find myself in general agreement with Mr. Hövelmann's recommendations, I also feel that we have much to learn about how to research anomalous claims. When we judge what is scientific, we do so within the context of current scientific methods and traditions. Yet those traditions and methods have always evolved and will continue to do so, often appearing a bit unorthodox at various stages in the process. Doing the things that allow one to be regarded as a good scientist by today's standards may or may not turn out to be the best practice in the long run.



COMMENTS BY CARROLL B. NASH:

As chairman of the session at the Parapsychological Association in which Gerd Hövelmann presented his paper, my comments to his seven recommendations are as follows.

(1) While parapsychology is not an attempt to revolutionize science, it may, nevertheless, have that effect by establishing in conjunction with further developments in quantum physics that mind underlies matter.

(2) Although parapsychology should be kept free of any kind of ideological speculation, study of the survival of bodily death is not, per se, beyond the scientific method. Furthermore, the principal of parsimony is applicable to the super-ESP hypothesis as well as to the spirit hypothesis.

(3) Because of claims of spontaneous psychic experiences, man was led to test for psi experimentally. As these tests have shown psi to occur in the laboratory, it would be ironic if it did not also take place in the field. Although it is not presently possible to determine whether or not any given ostensibly psychic spontaneous experience is paranormal, their study is of value as it suggests how psi is expressed in real life.

As regards quasi-experimental settings, the manner in which psi is manifested varies greatly with the individual. In order not to inhibit a paranormal phenomenon without giving it sufficient leeway to be expressed, the psychic should initially be permitted to demonstrate the ostensibly paranormal effect in the manner of his choice, following which the degree of control should be increased as the phenomenon becomes more manageable. If the controls do not reach a level which precludes alternative explanations, the paranormality of the effect should not be considered as having been more than suggested. The initial testing of ostensibly paranormal metal bending under quasi-experimental conditions has led to the development of sophisticated methods for its study such as strain-resistant gauges and piezo-electrical instrumentation.

(4) Hövelmann could make a contribution to parapsychology if he provided leadership in the construction of a standardized parapsychological terminology. A standardized terminology, however, should not be used to inhibit the development of new concepts with new terms.

(5) Parapsychologists should not abandon the concept of lawfulness in psychical research, unless after a much longer time than has already passed in its pursuit they should find no other choice.

(6) Agreed.

(7) Agreed, except for the installment of a scientific commission to put forward its opinion on the evidence of paranormal phenomena and to financially support research it considered promising. Such a commission would be in danger of becoming a self-perpetuating priesthood fostering the promulgation of outmoded ideas and discouraging research along previously unexplored trails.

COMMENTS BY IRMGARD OEPEN (as told to Piet Hein Hoebens):

[Prof. dr. med. Irmgard Oepen (Forensic Medicine, University of Marburg), with Prof. dr. med. Otto Prokop the best known German sceptic in matters relating to "occult medicine," regrets that her tight academic schedule does not permit her to accept Professor Truzzi's kind invitation to contribute a written comment on Herr Hövelmann's paper. However, she has authorized me to speak on her behalf and to summarize, for the benefit of ZS readers, her views on some of the issues Herr Hövelmann has discussed. Professor Oepen has seen and approved the manuscript.]

Professor Oepen applauds Herr Hövelmann's call for more scepticism within parapsychology. She recognizes that Herr Hövelmann, unlike so many of his senior colleagues in continental Europe, does not fit the stereotype of the credulous psychical researcher. She remarks, however, that self-criticism is merely a necessary, not a sufficient condition for scientific respectability. In a mature science, the adoption of critical, rigorous methods leads to a progressive accumulation of substantial findings. In spite of many claims to the contrary, such findings are notoriously absent from parapsychology.

On a philosophical level one is tempted to sympathize with rational proponents who merely demand a "fair chance" to prove the skeptics wrong. On a more practical level, however, one is compelled to think economically. A century of psychical research has failed to produce a single convincing breakthrough. Historical experience suggests that to give financial encouragement of parapsychological research projects is simply a bad investment.

In the meantime, the alleged discoveries of parapsychology are widely used to lend a semblance of legitimacy to a bewildering variety of noxious practices, especially in the field of "alternative medicine." Every year, hundreds of patients from Germany alone travel to the Philippines to be robbed by the local quack surgeons. This shocking business is defended by prominent academics who have become convinced that there is such a thing as "psychokinesis." Is it really surprising that many scientists, educators and criminologists in Germany are inclined to see "parapsychology" (at least the social phenomenon associated with this word) as a potential danger?

Of course, Professor Oepen does not wish to suggest that Herr Hövelmann and his friends should be held responsible for the persistence of harmful superstitions. To the contrary: she welcomes Herr Hövelmann's unambiguous denunciation of the charlatans. She is pleased that an informal conference in Marburg in November 1982 (attended by herself, Herr Hövelmann, disl. psych. Eberhard Bauer, dr. rer. nat. Walter von Lucadou and the present writer) resulted in an agreement to join forces in publicly exposing quackery.

COMMENTS BY JOHN PALMER:

I would like to commend Mr. Hövelmann for a thoughtful and useful paper. I find myself in agreement with most of his recommendations, sometimes strongly. As I have already consumed a huge amount of space in this issue, I will try to keep my comments relatively brief.

I agree that parapsychology's revolutionary pretensions have been a major obstacle to scientific acceptance. Too often in the past parapsychologists have seemed to point a gun at science's head, saying, "Here, accept our evidence and admit that your paradigm is wrong." This is hardly a good way to join the club. (I think this attitude is less prevalent among modern parapsychologists than is often supposed, but it is our responsibility to set the record straight.) Parapsychologists have uncovered a set of potentially important anomalies that deserve more serious attention among scientists than they presently receive, but this is a far cry from claiming a paradigm revolution. Paradigms are not overthrown by anomalies but by competing paradigms, and it is the latter which parapsychology lacks, at least at the necessary level of development. Even if we had such a paradigm, it would not necessarily follow that the existing paradigm(s) of science would be overthrown, because they deal with different classes of events. (See my reply to Dr. Alcock, pp. 91-103, for a further discussion of this point.) Our relationship with the rest of science should be one of cooperation rather than competition.

I particularly appreciated Hövelmann's fourth recommendation regarding terminology, as this subject has received too little attention among parapsychologists. The problem as I conceptualize it is that we use the same set of terms to label what we seek to explain (an anomalous relationship between a source and an effect) as we use to label the principle or process which (if it were to be sufficiently elaborated) might serve to explain it. I personally have found this terminological straight-jacket increasingly frustrating as I have become aware of it, and I sometimes find myself resorting to awkward locutions like "psi anomaly" when I want to talk about psi in the purely descriptive sense. What we need is one standardized set of descriptive, theoretically neutral terms and a separate set (or sets) of theoretical terms. The word "anomaly" would be a good model for the descriptive set, although its scope is too broad to be used as a synonym for psi.

The implications of this problem are particularly sinister because our current usage of terms like "psi" often creates the illusion that we are explaining an anomaly when in fact we are only identifying one. It also feeds back into the paradigm revolution issue: I think one is less likely to talk about a paradigm revolution if one fully appreciates this distinction. Our present terminology also serves to retard the development of genuine theory building in parapsychology by giving us this subtle illusion of understanding.

My major criticism of Hövelmann's paper concerns what I consider to be his overly narrow and rigid view of science, which comes through primarily in his discussion of his second and third recommendations. Such rigidity may be appropriate in purely physical science, but I think it is simply unrealistic when considering scientific attempts to understand

the human mind and behavior -- attempts which include but of course are by no means limited to, parapsychology.

I agree, that, in general, experimental designs result in less ambiguously interpretable data than do either quasi-experimental designs or field studies (which include "spontaneous case" investigations in parapsychology), but I would not draw the distinction as sharply as does Hövelmann. Many well designed experiments in psychology are intrinsically inconclusive because one can never be sure that one has not manipulated other variables in addition to the one intended. Conversely, quasi-experimental designs have become quite sophisticated and are being used increasingly to study problems that are not amenable to purely experimental investigation (Cook & Campbell, 1979). Conclusions can even be drawn from well conducted field studies, although here I agree that special caution is necessary.

To put this another way, I think that Hövelmann has perhaps fallen into the trap of treating pieces of research as either conclusive or worthless. In reality, I think evidentiality is a matter of degree. One should look at a given piece of research, regardless of the type of design, and ask oneself what are the alternate interpretations for the reported finding and what degree of probability should be assigned to each. The latter set of decisions, at least, I fear must of necessity be to some extent subjective.

The above has implications for Hövelmann's discussion of survival research. I agree (along with the great majority of parapsychologists) that the evidence for survival is far from conclusive, but I would not wish to go as far as Hövelmann in saying that there is no evidence for it at all. I also agree that I can see no way at the present time to establish survival CONCLUSIVELY by scientific research. However, I do not think it follows that one should refrain from undertaking such research or from proposing even speculative theories based on the survival notion, provided that such theories are capable of being developed to the point that they have testable implications.

I also do not think that the lack of practical consequences is a valid reason not to pursue survival research. Surely the value of science is not limited to its contribution to "CONCRETE purposes", by which I assume Hövelmann means technology. The worldview of science has had a major impact on Western culture apart from its technological fallout; indeed it is "skeptics" perception (in my opinion, misperception) of the potentially harmful impact of parapsychology on this cultural influence which to a large extent has fueled the psi controversy. Moreover, I think it is both legitimate and understandable that we would want to know something about our own nature and destiny. The fact that science may not be able to provide CONCLUSIVE answers should be no obstacle. We often are forced to make decisions and draw conclusions on incomplete or inconclusive data: some information is better than no information.

My only other disagreement with Hövelmann concerns reservations I have about Beloff's well intentioned proposal of a scientific commission to investigate psi. I think our experience with the Condon Commission on UFO's suggests that such bodies are unlikely to resolve anything when

the subject matter is controversial and the data ambiguous.

Although I have focussed disproportionately on my differences of opinion with Hövelmann, I would like to conclude by reiterating my strong agreement with most of his points, some of which I did not touch upon for reasons of space.

Reference

Cook, T.D., & Campbell, D.T., Quasi-Experimentation. Chicago: Rand McNally, 1979.

COMMENTS BY T.J. PINCH:

The object of Hövelmann's seven recommendations is to obtain "legitimacy and recognition" for parapsychology from "normal" science. The first comment that must be made is that by the very act of setting-up any institutional mechanisms for "Parapsychology Aid" (I include here the activities of consultant sociologists and philosophers) one is drawing attention to the very "non-normality" of parapsychology. We would find it odd, for instance, if physicists needed advice in order to get free quarks accepted as part of modern physics. Inevitably all self-consciously adopted strategies for the acceptance of parapsychology and other rejected fields will be double-edged swords, and I am deeply sceptical as to whether they can bring about their desired goal.

However, given a particular set of strategies, formulated with the intent of gaining recognition and legitimacy from orthodox¹ science, it can at least be asked whether the strategies embody a realistic picture of the practice of orthodox science and scientific change within orthodox science. I will look at each of Hövelmann's proposed strategies in these terms.

1. Give Up Revolutionary Outlook.

I do not think the situation is quite as straightforward as suggested by Hövelmann. Certainly, the espousal of revolutionary slogans and the call for programmes of revolution will not be effective--slogans seldom are. However, it remains the case that thus far parapsychology has not obtained a breakthrough and is meeting with "steady state rejection."² It is also the case that revolutions do occasionally occur in science.³ Given the right circumstances (and we do not know what these are),³ I see no reason why revolutionary change should not be possible. Just because Kuhn is in vogue does not mean that parapsychologists should neglect the revolutionary option altogether.

2. Leave off survival.

I think it is unwise for sociologists and philosophers to make recommendations as to the appropriate content of the field as Hövelmann suggests here.

3. Avoid Personal Evidence and Case Work.

In most natural sciences, case reports do not play a large part, but in some social sciences they can be important (e.g., areas of psychology). Also one wonders if the existence of meteorites could ever have been established if "rigid experimental testing" was the royal route to scientific legitimacy. I think the present use by parapsychologists of a whole range of different types of evidence is not a major barrier to them obtaining scientific acceptance.

4. Give up explanation for description.

What counts as explanation and what counts as description is a difficult problem in philosophy of science. It would also appear that most practising scientists use such terms loosely. I would suggest that focusing on this aspect of parapsychology makes no difference in terms of scientific acceptance.

5. Inconsistency should not be Made a Virtue.

Again we are dealing in part with the content of the discipline here as inconsistent results can be taken to be a property of psi. I think, however, there is less of a problem than there seems because those who postulate new effects to explain inconsistent results (e.g., experimenter effects) do so with the long-term aim of bringing about consistency (e.g. a consistent experimenter effect) As long as there has not been a total abandonment of logical consistency (and I see no evidence for this), then I see no severe barrier to scientific acceptance.

6. Critics arguments should be Considered Carefully and Collaboration Sought.

Scientific controversies are not usually resolved by the "let's all sit around the table and chat about it" spirit. Given the experience of most scientists working in a hostile environment, that contact with their critics is time-consuming and ultimately unproductive, I think parapsychologists would better be occupied with other activities. Certainly no one could quarrel with the weaker recommendation that they should be aware of the arguments of their critics.

7. Separate off from the Occult.

To get scientific ideas established, it does seem to be important to separate the highly professionalised interest of the researcher from outside interests and donors of funds. However, the existence of such outside interests need not be harmful. After all does the "Gee whiz! Isn't science wonderful" brigade, as presented in popular science magazines, harm physics? Also we must remember that orthodox scientists are very adroit at using funding from a variety of sources. I see no special problem for parapsychologists in receiving funding from occult interests as long as it can be maintained that these interests do not interfere with research in parapsychology.

Final Recommendation - Don't Abandon Scientific Method.

By definition scientific recognition must involve following the methods of science. This is sound advice!

Notes:

1. I prefer the term "orthodox science" to "normal" science as the latter implies Kuhnian normal science.

2. See, H.M. Collins and T.J. Pinch, "The Construction of the Paranormal," in Roy Wallis (ed.) On The Margins of Science: The Social Construction of Rejected Knowledge, Keele: Keele University Press, 1979, 237-270.

3. Though it seems two necessary preconditions are that there is a degree of conflict and that the phenomenon is fresh. See our discussion of paranormal metal bending as a possible scientific revolution in H.M. Collins and T.J. Pinch, Frames of Meaning: The Social Construction of Extraordinary Science, London: Routledge and Kegan Paul, 1982.

COMMENTS BY STEVEN M. ROSEN:

Gerd Hövelmann argues that the future success of parapsychology depends on our rigid adherence to the standards of scientific orthodoxy. We are admonished by Hövelmann to be "more papal than the Pope" in this regard, if we wish to win acceptance for our field and ourselves. In my opinion, the rather anachronistic, naive view of the status of scientific knowledge adopted in this paper detracts considerably from its value and prevents it from being convincing.

The crux of Hövelmann's difficulty is that the science of which he speaks is based on nineteenth century fiction, not twentieth century reality. Hövelmann implies a purely empirical, fact-gathering science, a science that can deal strictly in certainties, one so firmly anchored in what is "objectively out there" that it need not concern itself with values, purposes, subjective meanings, or with matters "metaphysical." In short, Hövelmann's science is the science of Objective Realism. Many modern philosophers refer to this nineteenth century doctrine as "naive Realism." Indeed, when viewed from our twentieth century vantage point, it can hardly be considered realistic.

In the past century, the authority of orthodox science has fallen open to challenge on several levels. Perhaps the most obvious question to be raised derives from the fact that by and large, the philosophy of Scientific Realism has undergirded the management and manipulation of resources on this planet for the last two hundred years. Looking around us at a fragmented world gripped by multiple crises and flirting ever more dangerously with catastrophe, we well may wonder how much longer we can continue to rely on the established epistemology. Even if we were to go so far as to exempt the credo of Scientific Realism from direct responsibility for our current dilemma, we could not deny it has done nothing to prevent it. Psychiatrist Aristede Esser is less equivocal: "Scientific knowledge is admired for being fast growing, but no one seems to notice that it may choke us all ... What kind of 'successful' knowledge is it that exacerbates the problem(s) it's principally asked to solve?" (Esser, 1982, p. 8).

Secondly, there are signs that the age of methodological reductionism is drawing to a close. One hundred years ago optimism about the universal validity of the scientific method had reached a high water mark. Thus it was believed that the strategy of classical science would soon bring complete order not only to the physical universe, but to the worlds of politics, economics, sociology, anthropology and others of the social "sciences." But today, the blush has gone out of the rose. Sigmund Koch's (1981)

sober reflections on the field of psychology well exemplify the growing disillusionment. In his ironic commentary on psychology's past strivings for legitimization as a science, Koch speaks of the prevalence of "meaningful thought" which:

"regards knowledge as an almost automatic result of a self-corrective rule structure, a fail-proof heuristic, a methodology -- rather than (a result) of discovery. In consequence, much of psychological history can be seen as a form of scientific role playing which, however sophisticated, entails the trivialization, and even evasion of significant problems (p. 257)."

Koch goes on to observe that after a hundred years as an organized discipline (interestingly, Koch's paper, like Hövelmann's, was delivered at a centennial celebration), psychology has managed neither to separate itself from philosophy nor establish itself as a science, and that "important sectors of psychological study require modes of inquiry rather more like those of the humanities than the sciences" (p. 269). Koch's concluding remarks may provide us with some helpful perspective:

"I have been inviting a psychology that might show the imprint of a capacity to accept the inevitable ambiguity and mystery of our situation. The false hubris that has been our way of containing our existential anguish in a terrifying age has led us to prefer easy yet grandiose pseudoknowledge to the hard and spare fruit that is knowledge. To admit intellectual finitude, and to accept with courage our antinomial condition, is to go a long way toward curing our characteristic epistemopathies. To attain such an attitude is to be free" (p. 269).

But the epistemological problem is deeper still, for beyond the doubts that have been raised about the exportability of the orthodox scientific method to the "hinterlands," its appropriateness has been questioned in the heart of its native territory, in the natural sciences proper, particularly in the field of physics. The paradoxes and uncertainties that have arisen with contemporary physics' attempts to probe scale extremes (the universe as a whole, the world within the atom) are now becoming widely known and have been popularly reported (see Zukav, 1979; Wolf, 1981; Capra, 1975). What are less clearly understood and in fact resisted, are their staggering epistemological implications. A hint of the problem has been given by physicist Henry Stapp (1979) and philosopher Milic Capek (1961); it has been spelled out more fully by physicist David Bohm (1980) and by myself (Rosen, 1982, 1983). In the present forum I must limit myself to a summary indication (while urging the reader to explore the matter further): The "anomalous" developments in the foundations of physical science, the thoroughgoing non-linearities, the "radical connectedness" observed among the phenomena, and perhaps more importantly, between the scientist/observers themselves and their observations (physics' "problem of measurement"), strongly suggest the bankruptcy of Scientific Realism and point to the need for a more human and humane, fully participatory, even aesthetic way of doing science.

So the twentieth century reality that confronts us is that the house of classical science is not in order. Its "wings" (i.e. the social sciences), erected during the period of expansion, are now being abandoned by a number of occupants for independent residences.

Many non-residents have begun to wonder whether the "neighborhood" is still benefiting from the presence of this old establishment and worse, cracks have been discovered in its foundations. Only in the "main building" -- the location of the pre-Einsteinian natural sciences -- is the normal program of activities apparently proceeding as before, but even here we should expect change, since sooner or later the foundational disturbances must be felt above.

For those who have waited so long at the door with bright hopes of gaining admission, conditions inside indeed may be difficult to face or even accept. But accept them and face them we must. Only by divesting ourselves of our nineteenth century illusions about science, by allowing the light of twentieth century developments to shine through, can we realistically evaluate the role of parapsychology, inquire on its proper relation to the time-honored establishment.

Once the circumstances within are recognized, our first inclination might be to follow the lead of those in the social sciences who are walking away from the building. But the phenomena we deal with do not permit us to do this. It may be possible to develop methodological alternatives in complete independence from orthodox science when studying, say, psychological processes such as emotion and cognition. Indeed, phenomenological and existential psychologists have already started down this path (see Valle and King, 1978). Yet parapsychological processes, by their presumed nature, are as intersubjective as they are subjective. We make the claim that the psi event is directly and veridically registerable in external reality; psi thus would have a physically manifesting aspect not found in the contents of a feeling, thought or dream. Consequently, we cannot afford indifference to the methods and concerns of physical science. We must still attempt to enter the house.

But in my opinion, we will not get a step past the threshold as long as we continue pretending to ourselves and to others that the strange hybrid we study is purely intersubjective. We might like to go in concealing the inherent "perversities" of our field, hiding the inconsistencies and paradoxes, the intrinsic irreducibility, experimenter effects, theoretical intractability, philosophical enigma. Our motive might be to secure a room on the "main floor." The point I have been making is that such an aspiration comes more from status fantasies we have been indulging than from a realistic appraisal of the actual role we can and should play.

Parapsychology's great irony is that while we have been stretching and straining ourselves to assume a posture that conforms with our idealized image of science, science itself is being revealed at its base in the natural image of us! To be sure, there are many in the house who would ignore these tremors, clinging to the belief that whatever may be happening down below, they will be able to stay the course of rigid orthodoxy. Such main floor occupants are not likely to allow us in regardless of the contortions we may perform to meet their expectations, for our very presence would make their efforts at denial more difficult. On the other hand, if we can overcome the fear of being who we are, a welcome may await us through the downstairs entrance from colleagues already at work in the foundations.

I agree with Hövelmann's comment on his first recommendation. Parapsychology has not been a genuinely revolutionary science because parapsychologists have attempted "the rigid application of orthodox scientific research methods in ... their investigations." But should we persist in such attempts if the subject-matter of parapsychology is revolutionary, as by all indications it is? Does it not behoove us to develop a methodology that would do justice to our subject-matter, great though the challenge may be? While Gerd Hövelmann, operating from a nineteenth century view of science and unrealistic pretensions about the role of parapsychology, charges us to drop all "revolutionary slogans" and be more scientifically "papal than the Pope," my position is that parapsychology must be radical, or it will be nothing at all. In truth, revolutionary slogans will not suffice. We need to begin a serious, systematic exploration of the deep-lying roots of scientific knowing vis-a-vis our field. In literal terms, to be radical is simply to return to the roots.

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COMMENTS BY GERTRUDE R. SCHMEIDLER:

Hövelmann is severe. He argues for conservative statements and scrupulous attention to facts (and surely this advice is sound). But he also appears to argue against exploring unmapped areas. It is true that such exploration may lead nowhere, but sometimes it can lead to important discovery. If he intended this latter advice, I think he is being overconservative.

The last four of his seven recommendations seem to me so clear that I hope it was unnecessary for him to state them. Of course (his fourth item) we should recognize that naming a phenomenon is not the same as explaining it. Of course (his fifth) our own failure to obtain clear, consistent results does not show that it is impossible for someone else to control the relevant conditions and thereby obtain repeatable data. Of course (his sixth) we should welcome informed criticism and make constructive use of it. Of course (his seventh) the pseudoscientific should be distinguished from the scientific.

Perhaps the first three recommendations could also have been phrased so that I would fully agree with them, but their wording and their defense trouble me. My basic problem with them is that Hövelmann wrote them as advice to parapsychologists -- to human beings -- and his advice seems to me to demand superhuman, unnatural, even unhealthy self-restraint.

Consider the first: that "Parapsychologists should instantly give up their revolutionary outlook upon their field and upon themselves." I heartily agree that we should give up the word "revolution"; it and "paradigm" have been so overused of late that they are stale and dull, bad for public relations. But give up the revolutionary outlook? No. This is quite a different matter. Research workers in every field, in my opinion, have a right to hope that their next experiments will be so insightful, deeply important, provocative, that the whole area will change once their not-yet obtained results are published. No matter that the hopes are seldom realized. It is this hope, this "revolutionary outlook" that sparks research in parapsychology as in other sciences.

The second recommendation is that we not express ourselves on the topic of survival after bodily death. To defend it, Hövelmann expresses himself; he writes that results of survival research have been ambiguous. I do not see why he should object if others also describe those ambiguities rather than treating survival as a taboo topic. Further, if an author begins by presenting a fair, objective description of the ambiguities, I would not consider it inappropriate for the article to continue by giving the author's personal opinion of whether the weight of the evidence is on one side or the other -- so long as personal opinions are clearly differentiated from description of the evidence.

The third recommendation is that we should not "too heavily rely" on personal experiences or spontaneous cases. The "too" of the "too heavily" makes the advice self-evidently sound. However the statement implies that we should not rely on such experiences or cases, and this is questionable. They can lead to the hunches from which brilliant new

research may emerge (though of course they can also lead to blind alleys). Rely too heavily? No. Rely heavily? Yes, if we are willing to take the risk.

Essentially, then, I am arguing in favor of freedom to explore a research direction or an essay topic that is personally exciting and that seems to have unrealized potential. I look forward to Hövelmann's rejoinder, to find if he is willing to modify his first three recommendations enough to show that he also approves such freedom.

COMMENTS BY DOUGLAS M. STOKES:

Gerd Hövelmann's comments are well thought out and reasonable in tone. I am pleased to see that, although he adopts a critical stance, he does not divorce himself completely from the field he is criticizing, calling parapsychology "our field" and using the pronoun "we" to include himself among the parapsychologists. This is in marked contrast to other critics, such as James Randi, who write profusely about parapsychological issues, while disclaiming any attribution of the title "parapsychologist" to themselves. However, such critics are indeed acting as parapsychologists when they reason from the data of parapsychological experiments to arrive at conclusions (albeit typically negative ones) about the nature or existence of psi phenomena. It is as if such critics wish to preserve the artificial and unrealistic distinction between the "good guys" (themselves) and the bad guys" (the parapsychologists). The effect is to condemn implicitly the mere pursuit of knowledge regarding ostensibly paranormal events. However, these critics are, of course, engaged in this very pursuit themselves! Furthermore, it is not realistic to sort people into polar categories with regard to parapsychological beliefs (and it reflects a dogmatic style of thought to insist on doing so); one is likely to encounter many more shades of gray with regard to opinions about even such a seemingly all-or-none issue as the existence of ESP than pure shades of black or white. Hövelmann's position is thus commendable, refreshing and constructive. Hövelmann's call for increased cooperation between parapsychologists and critics and increased attention on the part of parapsychologists to reasonable and accurate criticism is a fruitful suggestion. Hopefully, this increased cooperation will be reciprocal in nature.

I am in absolute agreement with Hövelmann that parapsychologists should do more to disassociate themselves from pseudoscientists and the occult lunatic fringe. There is an unfortunate tendency on the part of even some of the leaders of the parapsychological community to associate themselves, and in the process parapsychology, with pseudoscientific pursuits. In a recent presidential address to the Parapsychological Association, it was proposed that parapsychology and astrology would become increasingly less distinct disciplines in the future. Certainly, this sort of remark is not going to be helpful in either (a) getting "respectable" scientists to take parapsychology seriously or (b) furthering the development of the field (or at least preventing it from declining into a pseudoscientific discipline).

There are several points on which I disagree with Hövelmann, and I will enumerate these below:

(1) On the first page of his paper, Hövelmann asserts that it is not clear to "any parapsychologist" what the claim that parapsychologists and their discipline are "scientific" really means. To this I would reply that (a) it is not absolutely clear to anyone what this claim means, given the competing schools in the philosophy of science at the present time, (b) his statement is unprovable without some sort of exhaustive inspection of the mind of every parapsychologist, and (c) quite a few parapsychologists have just as good or better ideas of what science is than do many people practicing in other areas of science (if their ideas are not "better," they are at least firmer, more examined, and more differentiated).

(2) Hövelmann asserts that he "cannot see any revolution at all" in parapsychology. This position is difficult for me to understand. Research by investigators such as Helmut Schmidt suggests that human beings can predict events which are not yet determined under quantum mechanical theory (such as the emission of an electron from a sample of strontium 90). Such a finding is not explainable by any conceivable mechanism that can be proposed in the context of existing ("orthodox") theories of physics.

Also, a scientific revolution need not entail abandoning traditional scientific research methods, as Hövelmann seems to claim, although it is true that certain findings may be revolutionary in the sense that they require the abandonment of existing theories (as opposed to methodologies). It is also not clear how Hövelmann's apparent advocacy of abandonment of existing methodologies at the beginning of his paper can be consistent with his call for adherence to "methodological 'scientism'" and to orthodox "methods and methodological standards" at the end his paper. I am in full agreement with this latter position, and am appalled by recent calls by some parapsychologists for abandonment of traditional scientific methods in parapsychology. Perhaps Hövelmann meant to assert that parapsychology can not be a revolutionary discipline without abandoning orthodox methodologies, although this position seems somewhat untenable for the reasons outlined above.

(3) I do not agree that repeatability is impossible for parapsychology for "theoretical reasons." If Hövelmann has a well-established theory of psi phenomena that enables him to derive this conclusion, I would like to see it, as he will be the first person to have such a theory. I also disagree with Hövelmann's minimization of the problem nonrepeatability poses for the establishment of parapsychology as a science. Some degree of repeatability and reliability of efforts is necessary (a) in order to establish theories of any power and (b) in order to convince skeptics who attribute the non-repeatability of parapsychological findings to fraud and methodological errors.

(4) I agree that most of the survival research is poorly conducted and generally subject to obvious counterexplanations in terms of normal processes and that Occam's Razor might indeed be profitably applied to most of it. However, unlike Hövelmann, I do see some value in survival research. The Western world is not areligious, but is presently largely subscribing to the covert religion of materialism, which denies that anything exists but material processes and events. There are, however, other, equally viable views of the universe, and survival research may in some instances serve to bring the possibility of these alternative views to the attention of people who may not have examined the basis of their implicit belief in materialism or may not even be aware of their unconscious subscription to materialistic philosophy. Thus, someone who is exposed to a discussion of the Raudive voice phenomena (to use a particularly ludicrous example)

may as a consequence reexamine his or her metaphysical beliefs (while hopefully also seeing the weakness of the research which prompted that reexamination). Also, I see as a legitimate enterprise argumentation for dualism on the basis of empirical evidence or philosophical considerations (for a review of such arguments, see Stokes, in press). To cite one such argument (of a mixed philosophical and empirical nature), a person typically considers himself to be a continuous entity which exists at least from birth to death and is associated in some inexplicable manner with a particular physical body. The person may identify himself with what Hornell Hart (1958) called the "I-thinker," that entity which thinks his thoughts, remembers his memories, senses his sensations, feels his feelings, and so forth. But, as the material substance of the human body is continually changing, to the extent that a person's body is a totally different collection of atoms and material particles from what it was several years ago, it is difficult to see how this "I-thinker" could be a material entity (i.e., identified with a particular collection of material particles). The question also arises as to whether the "I-thinker" might survive the ultimate death of the present body in the same way that it has survived the dissolution of the body of several years ago. Thus, philosophical arguments may profitably be raised that might be capable of, if not deciding, at least influencing one's beliefs about ultimate metaphysical issues. Hövelmann is, however, skeptical regarding whether we would profit from an answer to the survival question. "Would it relieve our mortal dread?" he asks. I believe that a positive answer might indeed reduce that dread, which has been one of the central concerns of human beings since they first appeared on this planet. I do not agree that we need to avoid "provoking treacherous hopes and expectations" as Hövelmann suggests. At any rate, what harm would there be in raising such hopes, which materialistic philosophy has so prematurely crushed into the ground?

(Having said all this, let me make it clear that I am wholeheartedly in agreement with Hövelmann that virtually all the existing research on the survival problem is at best absurd.)

(5) Regarding Hövelmann's contention that parapsychology should be "kept free of any kind of ideological speculation on the nature of man, of the world, or of the universe," I would ask, if such questions are not the central concern of science, what is?

(6) I agree that much parapsychological terminology is at best descriptive and is often only negatively defined (as the absence of known physical channels, etc.). But Hövelmann's recommendation that a new standard terminology be introduced will not improve the situation in the absence of the construction of new theories. Only terms which are coined in conjunction with specific testable theories (such as Schmidt's "strength of psi source") and which are intimately involved in the generation of testable predictions from a theory will rescue parapsychology from its current vague and descriptive terminology. Again, the construction of a theory on the present unreliable data base of parapsychological findings is virtually impossible, and so the adoption of a powerful terminology with strong empirical content may have to await more reliable research findings.

In this context, I concur with Hövelmann's observation that parapsychologists should not "overhastily abandon the concept of lawfulness in their field." Such an abandonment would almost certainly result in the field's stagnating in its current morass of unrepeatably results and its current atheoretical stance.

(7) Regarding Beloff's proposed "commission of inquiry," what have the S.P.R. and the Parapsychological Association been, if not commissions of inquiry? To expect a newly appointed commission to resolve in three years' time the open issues that have thus far withstood a century of attempts at resolution by various investigating bodies seems at best a futile hope.

Once again, I would like to commend Mr. Hövelmann for his tightly reasoned paper. Its tone is both reasonable and constructive.

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COMMENTS BY CHRISTOPHER SCOTT:

Parts of Hovelmann's article could be construed as a critique of parapsychological evidence. He is saying that the evidence for survival is negligible, that spontaneous cases are of little or no value as evidence for psi, that the evidence presented by stage performers and other professionals is highly suspect, and so on. I have no quarrel with any of this.

But clearly this is not his prime purpose, or he would not have couched his critique in terms of "recommendations for the future practice of parapsychology." What he is really doing is calling for a better window display. With the single exception of the 6th, all of his recommendations concern the way parapsychologists talk. But talk is not scientific practice. If he wants to improve the practice of parapsychology he should concentrate on the experiments that parapsychologists do. If he improves only the talk we will have a better shop window but the same goods in the shop. As one salesman's recommendations to other salesman, his paper may make sense, but he cannot possibly expect satisfaction to be expressed by the customer--with whom, as a skeptic waiting to be convinced, I align myself.

For my part, as long as parapsychologists go on doing bad experiments, I am quite happy that they should continue talking bad science: at least that way we all know the value of the goods in the shop. However I would willingly support an attempt to make recommendations really concerned with the practice of parapsychology.

COMMENTS BY ULRICH TIMM:

I am very pleased to recognize in Hövelmann's recommendations some of those principles which I have always regarded as self-evident for sound empirical scientific research. I further believe that all parapsychologists who have a proper scientific training and who are prepared to treat parapsychology as a science rather than as a substitute religion or metaphysics, are familiar with these principles. When they occasionally disregard them out of "forgetfulness," Hövelmann's recommendations may be a useful reminder. On the other hand, I do not regard it as either possible or desirable to influence those who call themselves parapsychologists but who do not wish to pursue scientific research. In that case a strict separation would be more appropriate.

With regard to details, I would like to add the following:

1) The methodology used by parapsychologists is indeed "orthodox" except that some rules are not always followed with sufficient care. But the content of parapsychology is by definition "unorthodox" because psi phenomena (unless they are found reducible to subjective or objective deception) cannot be explained within the framework of the established sciences. The existence of psi phenomena requires a substantial expansion and reformulation of the contemporary scientific view of the world. Undoubtedly this has the character of a "paradigm switch" or "scientific revolution." On the other hand, such changes are not unusual in the history of science and consequently it is superfluous to declare parapsychology in particular as a "revolutionary science."

2) I do not regard the survival problem as one which in principle is beyond any empirical investigations. Parapsychology could make an important contribution to the solution of this problem (as well as of the related mind-body problem). However, I agree that to date no conclusive results have been obtained and that, with regard to the present thanatology fad, this should be emphasized in statements to the general public.

3) The empirical evidence from "spontaneous cases" and from "quasi-experimental settings" must be judged according to similar criteria as for fully controlled experiments. (However, the possibility for error is much larger in the first case). "Personal evidence" is a separate phenomenon which can also occur in strictly controlled experimental situations. This is a rather intuitive experience of subjective certainty, which has an important motivational function in research without requiring a corresponding objective evidence. At any rate, this experience is useful if it inspires researchers to work out new hypotheses and investigations. The planning of psi experiments (particularly the selection of subjects) is frequently based on impressions of subjective evidence in non-experimental or semi-experimental situations, for instance the semi-experimental chair tests with the paragnost G. Croiset, which Hövelmann mentions and which Hoebens criticized, did not only give experiences of subjective evidence to the Freiburg parapsychologist Hand Bender, but also caused him to develop in his institute objective and controlled "chair experiments" which could be quantitatively evaluated.

4) In principle I would find it useful to develop a standardized descriptive terminology for parapsychological research. But I do not see it as an urgent task for a science which in theoretical and empirical terms is still in a trial and error phase.

5) We do not know as yet which attributes are constitutive of psi phenomena. The empirical "variability," "unreliability," "inconsistency," "elusiveness,"

etc. of psi data requires an explanation as well as any other empirical observation. I regard it as one of many possible hypotheses to interpret this fact as an expression of general stochastic laws in the psi domain. That has nothing to do with "fatalism" or "sarcasm." I do not believe, however, that a monocausal interpretation is sufficient in that case.

6) Scientific discussion consist largely of critical arguments and critical counter-arguments. Such discussions are indispensable for scientific progress. This is particularly the case for such an undeveloped science as parapsychology. In this connection, it is unimportant whether a parapsychologist participates in a discussion with someone who regards himself as a parapsychologist or as a "sceptic." Essential for the fruitfulness of a discussion is, however, that the arguments are competent, novel, important, free from non-scientific presuppositions, constructive and not obstructive.

7) This recommendation has been discussed by Hövelmann in such a convincing way that it requires no further comments.

COMMENTS BY JEROME TOBACYK:

Hövelmann proposes seven recommendations for facilitating the acceptance and recognition of parapsychology by "normal" science. Six of these recommendations appear to concern parapsychologists conforming to formal characteristics of science, while a seventh, "Parapsychologists should instantly give up their revolutionary outlook upon their field and upon themselves" appears to concern self-presentational tactics that are (should be) largely irrelevant to whether a topic is acceptable as science.

The origin of the revolutionary attitude among some parapsychologists may partly derive from their rejection by much of the more orthodox scientific community. As demonstrated by Cognitive Dissonance Theory (Festinger, 1957) and Reactance Theory (Brehm, 1966, 1972), such a rejection might result in an increase in belief/commitment, leading to a revolutionary attitude.

Such a revolutionary attitude might have adaptive consequences. Rather than conforming to the more orthodox attitudes of the scientific community and abandoning their interests in parapsychology, some parapsychologists, because of their revolutionary attitude, continue and further develop their research. This continued interest/research decreases substantially the possibility of making a Type II error.

The existence of such phenomena as studied in parapsychology could have such enormous consequences for man that it may be more critical not to make a Type II error than to make a Type I error. Certainly, when published evidence is provided for the existence of a paranormal phenomenon in a replicable manner, many attempted replications are likely, since such evidence is generally met with skepticism. Thus, the high likelihood of attempted replications of reported demonstrations of paranormal phenomena make the long term acceptance of such a finding due to Type I Error very unlikely.

Further a revolutionary attitude toward oneself and one's work might not only be the necessary motivation to carry out one's

research, but may be the mainspring of all human life. According to both Rank (1936, 1968) and Becker (1973) each man must possess a belief in their own heroic capacity, not only to achieve scientific breakthroughs, but most fundamentally, to solve the main, existential problem of life. This problem concerns the symbolic achievement of immortality--of personal triumph over one's inevitable death. According to Becker (1973) in The Denial of Death, "Man must justify himself as an object of primary value in the universe; he must stand out, be a hero, make the biggest possible contribution to world life, show that he counts more than anything or anyone else" (p 4). Thus, each man has the need to view himself as a hero - a revolutionary - as a basic feature of the human condition. I must question the value of a recommendation requesting that some men abandon a self-conception (belief system) that may be the fundamental meaning structure for their existence. Indeed, existential philosophers and psychologists, such as Kierkegaard, Nietzsche, and Frankl, also emphasize the importance of this heroic aspect of each person's life.

A person's revolutionary outlook (personal philosophy) should not strongly influence the critical evaluation of their research by other scientists, though it might influence the scientist's personal attitude toward the "revolutionary." Legitimacy and recognition as a science is not a popularity contest which is largely won or lost on the basis of self-presentational tactics. If research is properly conducted and reported, allowing public verifiability and intersubjective replicability, the self-correcting nature of science should result in a relatively objective evaluation of the scientific status of the phenomena being studied.

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COMMENTS BY RHEA WHITE:

In his comment on his first recommendation, Hövelmann says parapsychologists should not call themselves revolutionary because they have adopted from the established sciences the rigid application of orthodox scientific research methods. I do not think that parapsychologists consider their methodology to be revolutionary. It is what they have found with the application of those methods that some of them consider to be revolutionary. I would take this even a step further. I think it is the role of parapsychology to revolutionize scientific method itself, not only in the area of parapsychological investigations but in all fields. (But this is a vision, not a reality, and not the subject of this discussion.)

In his further comment on his first recommendation, Hövelmann says that parapsychologists cannot bring about a "paradigm switch" in science simply by "the pragmatic decision to do so." I can certainly agree that scientific revolutions do not come about simply by deciding to create them, programmatic or not. However, by aiming to understand the workings of one's own area of scientific investigation such revolutions do take place from time to time. Not by decree, certainly; not by wishing it to be so; but primarily because one conceives of the nature of reality in a way that resolves old problems and in a manner which can accommodate more facts than could the old view, as well as lead to the prediction of new findings which can be confirmed empirically. That is the aim of parapsychology, as I understand it, and the aim of any activity that calls itself scientific. The aim is to understand, not to revolutionize, but the nature of insight is revolution.

I am in agreement with Hövelmann's comments on the second recommendation, save for the last two paragraphs. This is not the place to argue on behalf of the importance and desirability of research on the survival problem, but I would at least like to say that those who are motivated to work in this area should not only be allowed to do so without being called "unscientific" (unless their methods earn that label: whether or not something is "scientific" cannot be judged on the basis of subject matter but rather of methodology). In fact, those who choose to work on the survival problem should be applauded. It has got to be one of the most difficult--if not the most difficult--research problem, but it should not be considered reprehensible for that reason! Moreover, I disagree with Hövelmann's final remark on this second recommendation, where he says "parapsychology should be kept free of any kind of ideological speculation on the nature of man; or of the world, or of the universe" etc. First, no science can advance without speculation. The very choice of a problem area to investigate requires speculation. Choice of methodology, subject populations, methods of analysis--all involve speculation in one form or another. But even more than this, the subject matter of parapsychology is the very nature of the mind and of the universe. In order to investigate the mind, in order to be objective in parapsychology, we must expose our subjectivity and describe with as much care as possible where we see ourselves in the sea of mind. Facts are spawned by speculation, and since the mind/body/spirit interface is what parapsychologists are investigating, until they can come into that more unitary conceptual view that Hövelmann says cannot be sought programmatically, they must do the best they can with what they have: ideological speculation on

the nature of humans, the world, the universe, the meaning and purpose of life, etc.

In his third recommendation Hövelmann cites Sybo Schouten as one who does not look to spontaneous psi as evidence, as if he were an exception. I seriously question whether many parapsychologists feel that spontaneous cases provide evidence for psi. If we take the reality of psi as a working hypothesis, cases may be studied as if they were psi-based in order to provide insights into understanding the psi process, but confirmation of these insights must always come from experiments. This is certainly not a new position. The Rhines, for example, have also advocated it since the 1940s.

I also am in agreement with the fourth recommendation, and I have high hopes that Hövelmann himself, who is involved with linguistics and with the philosophy of science as well as with parapsychology, will be able to do pioneer work in methodically constructing a standardized parapsychological terminology that would guarantee the intersubjectivity of statements made by parapsychologists.

As for the fifth recommendation, Hövelmann's remarks are fine as long as the inconsistencies in parapsychological data are not psi-determined. At this point we do not know. Being open to the bewildering possibility that they sometimes may be is a first step in designing experiments which might capture the elusive beast, perhaps after the manner of fencing in a larger area surrounding a smaller enclosure in which a valuable animal is kept. If unbeknownst to the owner the animal gets out of the smaller enclosure, it may still be found and recaptured within the larger enclosure.

Regarding the sixth recommendation: Yes, parapsychologists should carefully consider the arguments of the critics, with two provisos: (1) the critics, in turn, should listen in good faith to parapsychologists when exchanging opinions--it cannot be a one-way road; and (2), parapsychologists must also listen to themselves and to other parapsychologists. It is foolish for someone not familiar with the intricacies of a given field to criticize that field. There is a point at which master violinists can exchange views only with other master violinists--where an exchange is no longer productive even with average violinists--to say nothing of pianists or truck drivers or bakers or sociologists. There can be few fields where outside critics are attended to more carefully than in parapsychology. I think that at this time the balance should be righted by leaning in the opposite direction from that proposed by Hövelmann. Even the very best criticism, that which all would agree is constructive, can only be so in a negative sense. First there has to be something to criticize. The development of a viable experimental protocol sensitive to the nuances of the psi testing situation is also important, and critics and parapsychologists alike forget this. Parapsychologists cannot put critics first. They have to put parapsychology first. If they don't, certainly no one else is going to!

In regard to Hövelmann's final recommendation: I agree that parapsychologists should not adopt the methodology of pseudo-scientific and occult groups. However, I not only see nothing wrong with but actively support reading the literature of and listening to exponents of the fringe groups, who may be practitioners of genuine psi, at least part of the time. It is possible that suggestions and clues may be obtained from these people that can be tested experimentally, as R.L. Morris and his associates have done with the Airplane Project, for example.

Second, in writing that whatever our view of science, "in any case we will have to adhere to the methods and methodological standards which are held to be scientific in orthodox science, provided that we want to substantiate our claims to be scientists conducting scientific research," it is my understanding that Hövelmann is saying that the canon on scientific method is closed. He says the axioms of scientific method may not be of exceptional soundness but we have to follow them anyway. Balderdash! I say! If they aren't sound, then the canon is not closed and we can make them sounder, for the benefit not only of parapsychology but for all the sciences. As far as I am concerned, the canon of scientific method to which I think we should adhere to call ourselves scientists is that the final arbiter of any hypothesis or model is empirical, publically verifiable data predicted in advance of data collection or at least observation. As long as we adhere to this, we can stand any other dogma of science on its head if we like, and still be scientists behaving scientifically. Any way we can use to honestly get significant empirical, publically verifiable results is fine. If religious or other groups have any clues as to how to do that, I am not going to close my ears for fear of not being thought "scientific." A true scientist is open to the whole world as a source of ideas for his or her work. More established sciences may be able to forget that for a time, but certainly not forever if they wish to progress. Parapsychology, however, is in no position yet to do so. We have no choice, really, except to range as freely and widely as possible in our search for clues to viable research.

Gerd Hövelmann is a valued friend and collaborator. I admire his industry and his high standards and I thank him for this opportunity to find out for myself where I stand on some of these important issues. I am dismayed by how diametrically opposed we are, ideologically speaking. But there it is. Let us remember that it takes two points, widely separated and opposite each other, to build a bridge.

COMMENT BY LEONARD ZUSNE

It is true: parapsychologists have been guilty of all the things Hövelmann's recommendations are directed against. If they would only mend their public ways, improve their manner of self-presentation, then assuredly a more favorable recognition on the part of orthodox science would be forthcoming.

One should have nothing but admiration for Hövelmann's piece. It is permeated by a sense of fairness and fervor to set the parapsychological house in order. In fact, on superficial reading Hövelmann sounds like an outside critic, which is deceptive because he isn't. Hövelmann stands with both feet planted firmly in parapsychological soil. His seven recommendations are strictly in-house rules for how to behave like the compleat parapsychologist. They are almost Skinnerian in nature: behave like a scientist, and you will be (or feel like) a scientist. Or will you? This, to me, is the heart of the issue and not whether parapsychologists will accept and implement the seven recommendations.

Parapsychologists do use the scientific method in their work, but the use of the scientific method alone does not guarantee that the user is or will therefore become a scientist. To be a scientist

involves more than just methodology or even the possession of a systematic body of knowledge (which is hardly the case in parapsychology): it implies the acceptance of a certain view of the world. This view has many names, one of which is the "demonstrative" view. I have discussed it and its opposite, the "dialectic" world view, at length in this Journal (No. 8) and elsewhere. The point is that if one's conception of the world is of the latter kind, no amount of scientific methodology or terminological overburden will hide the underlying belief in a different kind of reality.

It is this world view that produces the phenomenon of a parapsychologist and a skeptic looking at the same experimental results but with both arriving at diametrically opposite interpretations of what was observed. It is the interpretation that counts, and one's interpretation of data is a function of one's philosophy. The work of even the most scientifically rigorous sounding individuals may be informed by the underlying predisposition to embrace, explicitly or implicitly, a dualistic world view in which the customary laws of causality may not always operate. In a world seen in this fashion one or more of the following obtain: (1) in the felicitous phrase of Rochas d'Aiglun, the "externalization of sensitivity" becomes possible, accounting for such phenomena as the out-of-the body experience, psychokinesis, telepathy, and clairvoyance; (2) non-physical beings (ghosts, spirits) or unknown and mysterious forces ("psi energy") intervene in natural phenomena; (3) the (basic) limiting principles that govern the ways in which nature works may be suspended - principles that govern the flow of time, spatial relations, or the nature of matter, making clairvoyance, teleportation, or psychosurgery possible; (4) causation is imputed not only to energy transfer among physical bodies but also to contiguity and similarity between objects and events, as in the current reasoning by analogy concerning the relationship between paranormal and quantum-mechanical events. An interpretation of the world in these terms is something that is simply not acceptable to one viewing it from the other side. Hövelmann's recommendations, eminently reasonable as they are in themselves, cannot have but a mere cosmetic effect on parapsychology because they address matters of method and public relations and not the root cause of parapsychology's problems.

I hasten to add that I have absolutely no objections to research on topics that are currently being investigated by parapsychologists or other researchers of anomalies. Quite the contrary, I have urged (Teaching of Psychology, April 1981; Perceptual & Motor Skills, 1982, 55, 683-694; Zusne & Jones, Anomalistic Psychology, 1982) that psychologists do not ignore, reject, or sweep under the rug extraordinary phenomena of behavior and experience but come to grips with them and teach their students what science has to say about such phenomena. In doing anomalistic research or in teaching about it the psychologist who has the demonstrative view of the world has quite an advantage over the parapsychologist because he (1) does not have a revolutionary outlook on the field or on himself that he must give up; (2) never has the urge to express himself in learned words on the problem of survival after bodily death; (3) rarely if ever feels called upon to rely heavily on personal evidence obtained through spontaneous paranormal occurrences; (4) already possesses the conceptual arsenal of intervening variables, hypothetical constructs, and operational definitions to help him through any definitional or

terminological confusions; (5) already tends to view inconsistencies as simply inconsistencies or as challenges to his investigatory talents and not as explanations of anything in themselves; (6) is and has been for some time, in the scientific mainstream and thus exposed to and responsive to criticisms from his peers, workers in other fields of psychology, workers in neighboring disciplines, and the society at large; and (7) does not have, as a matter of practicality, the problem of separating himself from pseudoscientific claimants who refuse to adopt rigid scientific methods and from their untestable hypotheses, full of supernaturalism and metaphysics. And, of course, he has also much less of a problem in meeting the criteria of public verifiability, intersubjective reliability, replicability, and falsifiability. In fact, there is so much going for the psychologist and so much against the parapsychologist that one may wonder: is parapsychology really necessary?

Gerd Hövelmann will reply to his commentators in ZETETIC SCHOLAR #12.



SAY, DON'T YOU THINK IT'S HIGH TIME YOU RENEWED YOUR SUBSCRIPTION TO ZETETIC SCHOLAR?

BOOK REVIEWS

Stargazers and Gravediggers. By Immanuel Velikovsky. William Morrow and Company, New York, 1983. 346 pp. \$14.95.

Reviewed by Henry H. Bauer

That the Velikovsky Affair continues to be discussed is proof of its significance; but there are different schools of thought about what that significance is. The root of the disagreements is no different than it was 30 years ago: is there or is there not any substantial and substantive merit in Velikovsky's claims about historical chronology and about planetary motions? Those who believe there is naturally see scientific significance in that; but also find significant the manner in which Science rejects revolutionary yet not impossible ideas that turn out to be correct. Those who still find no merit in Velikovsky's substantive claims can nevertheless find the controversy significant. Some find it so as an exemplar of public gullibility; or (not mutually exclusive) as an exemplar of public debates in which experts and laymen attempt to grapple with highly technical issues. In principle, there ought to be some common ground for debate among these differing views, the common ground being the importance of the controversy itself: how protagonists, media, and public comported themselves and interacted in judging the merits of purportedly scientific propositions. In practice, that potential common ground is unlikely to be much occupied because of human tendencies that were clearly displayed in the Affair during the 1950s, 1960s, and 1970s: Velikovsky's followers tended to take umbrage at anything that implied rejection of Velikovsky's claims; Velikovsky's critics were impervious to appeals to fair play since Velikovsky was so wrong, a pseudoscientist -- and Science owes no fair play to pseudoscience.

Those who find the controversy significant are provided important new material by the posthumous publication, in March 1983, of Velikovsky's memoirs of the affair, Stargazers and Gravediggers. There is convincing new detail about the unscrupulous behavior of such critics as Harlow Shapley and Cecilia Payne-Gaposchkin; there is fuller information about how Velikovsky came to his ideas; there are more clues for understanding Velikovsky's misconceptions about scientific practice.

As the last implies, I am one who finds no merit in Velikovsky's substantive claims on matters of science (and that view also influences what I find important in Stargazers and Gravediggers). Nevertheless, I was able to enjoy the skillful manner in which Velikovsky demolishes the feeble attempts at argument of some of his early critics. The philosopher Lafleur, in particular, receives wittily short shrift: Lafleur's own criteria (developed ad hoc, by the way) for identifying cranks, Velikovsky points out, entail that a valid revolutionary theory in science, in contrast to a crank theory, would be "in accord with currently held theories in the field of the hypothesis" as well as in other fields, indeed in all fields!

Harlow Shapley's concern to discredit Worlds in Collision is fully documented; and that concern was expressed most unpleasantly. He clearly threatened Macmillan, but by innuendo and in the passive voice, not willing to have openly known what he was doing -- labelling his communications as not for publication, and even having the gall to describe the right to publish as a basic freedom. Shapley was less than straightforward with the scholar Kallen, and downright untruthful with his friend Thackrey.

The unhindered publishing and selling of Worlds in Collision would have been far less damaging for Science than the unscrupulous, inept, and unsound tactics of Shapley, Payne-Gaposchkin, and the rest. Scientists owe it to their profession, as well as to the public, to exemplify in their public behavior and utterances the virtues they claim for the scientific enterprise: care with facts, for example, and argument based on facts and logic. Stargazers and Gravediggers ought to be required reading for scholars and scientists who want to engage in public controversies. It is sad to see some of the malfeasances by Velikovsky's critics repeated decades later by Carl Sagan, for instance, and by some members of CSICOP.

I felt real empathy with Velikovsky as he described his days in the library at Columbia, his gratitude at having available that wealth of intellectual riches. But his comments, how rarely he saw professors there, are a clue to the weakness in his own work. Velikovsky did not realize that successful scholarship,² particularly in science, results from communal and ultimately consensual activity. He developed and elaborated his ideas in isolation from those contemporaneously concerned with research in the fields that engaged his interest. He published infrequently, and after having lived with his ideas so long that he was no longer able to benefit from detailed criticism. In contrast, professional scholars and scientists expose their continuing research in the form of papers read at meetings and short articles in disciplinary journals, and they are able to adapt to the existing expert consensus -- or, at least, to be very clear about their disagreements with it before they diverge too far from what has been consensually established. Moreover, practicing members of disciplinary communities come to understand professional interactions in a way that Velikovsky evidently did not. His first meeting with Shapley shows that Velikovsky was very naive about how science is done: as I read Velikovsky's own account, it seemed obvious to me that Shapley would think he had been accosted by a crank; equally obviously, Velikovsky never understood that. Velikovsky reveals great naivety also in his admonitions to Brett of Macmillan not to be frightened (because the book was good, inter alia); and in his inference that Conant found nothing unscientific in his book, because if he had, he would have pointed it out.

Stargazers and Gravediggers provides some support for criticisms of Velikovsky that the Velikovskians have strenuously resisted. The pamphlet Cosmos Without Gravitation, published privately by Velikovsky in 1946, is demonstrably unsatisfactory³ as the technical discussion that it purports to be. Some have suggested that Velikovsky no longer subscribed to the views expressed there, pointing out that Worlds in Collision did not cite that pamphlet. But in Stargazers, the footnote on page 165 shows no retreat by Velikovsky from that thesis. Again, one of the more ad-hominem criticisms of Velikovsky was of his self-importance, reflected for instance by his comparisons of himself with the greatest names in science. In Stargazers there is some support for that allegation: see pages 61-63 re Darwin, pages 102 ff. re Galileo, page 276 re Faraday, and page 297 -- that Einstein would plausibly think of Velikovsky when talking about Benjamin Franklin and Isaac Newton.

So Stargazers will provide more fuel for the controversy,⁴ for critics of Velikovsky and for critics of the critics. It is indispensable for anyone who wants to understand the controversy, and I can also recommend it to all who are interested in public attitudes toward science and in public debates about science and pseudo-science. Moreover, the book is very good reading -- by far the best written of Velikovsky's works, enlivened occasionally by a delightful dry wit. Stargazers covers the years up to 1956, and the Epilogue alludes to two more such books as forthcoming: The Test of Time, and the story of the AAAS symposium of 1974 and its aftermath. If they are as well written and authentic as Stargazers, they will have been worth the wait.

Notes:

1. Details are given in my book-length analysis of the controversy, forthcoming from the University of Illinois Press.
2. See, for example, John Ziman, Reliable Knowledge, Cambridge University Press, 1978.
3. See the relevant chapter of the forthcoming book, note 1.
4. And perhaps indications of new battles, too. All other books by Velikovsky -- including the posthumous Mankind in Amnesia -- were published by Doubleday; but Stargazers is put out by William Morrow. I am curious to know why the change was made.

The Fakers: Exploring the Myths of the Supernatural. By Danny Korem and Paul Meier (revised edition). Grand Rapids, MI: Baker Book House, 1980. 181 pages, \$8.95.

Reviewed by Douglas H. Ruben and Marilyn J. Ruben

Preternatural phenomena exist, in part, because of their large box-office attraction. Sensationalistic reports of the purported occult existing through an observable or unobservable medium are commercially popular for many reasons, but mostly because of their believability. Korem and Meier's book is provocative in this respect. It attempts to superimpose onto these popular beliefs in spiritism and transcendentalism a qualification of natural science. The authors expressly aim to distinguish "pseudo-occult" from "occult" phenomena for readers of the lay Christian market who, they feel, indiscriminably consume replete amounts of deceptive information. To this extent, renown specialist in legerdemain Danny Korem and Christian psychiatrist Paul Meier criticize the misdocumentation of events in Christian psychical literature for obscuring what may amount to either trickery or skillful ideomotor action. Their refutations strike particularly at Dr. Koch's "clinical" reports of supernaturally possessed clients (e.g., in The Devil's Alphabet, Between Christ and Satan, Christian Counseling and Occultism, etc.) pursued here with the same determination as Randi's exposure of Uri Geller's "powers." Autography, tarot cards, readings, dowsing, psychic surgery, and even fire walkers are among the selected anomalies that Korem eloquently explains by comparing them to his own magical replications. His veracity and research investigations reported in the book further led him to recently produce a television documentary exposing a leading purported psychic, James Hydrick, who was trying to establish a cult (aired April 16, 1983). Thus, readers are at once prepared for a reassessment of the Christian depiction of paranormality.

However, this was the first disappointment. A book so poignantly titled "the Fakers" certainly promises well beyond the occasional promotion of psychical fraudulence. Anticipated from this title, instead, is a revelatory account of how scientific principles

underlying mystical experience are obscured magically by the artistic elegance of claimed psychics. Korem, himself, admits that "given proper circumstances, anyone can be made to believe that he has witnessed something which never took place" (p. 19) and that if "magicians can be fooled, how much easier is it to fool his audience [?]" (p. 19). So, expectedly, readers eagerly await that moment of realization when the prestidigitator discloses how a trick is done. But rarely is this expectancy satisfied. Korem's few explanations are merely concessional to passify his readers (thus maintaining the magician's oath of secrecy.)

Beyond this, the scientific reader anxiously awaits clarification on insightful points of observation. For instance, "the pendulum in and of itself possesses no powers" (p. 50) and that "one must review the physical objects, check written testimony and screen oral testimony" (p. 50). These statements are immediately enjoined by scientific intrigue. When falsifications of mystical phenomena are achieved this way, by appealing to realistic or "naturalistic"

events, the book's scientific orientation is greatly magnified (especially for a religious market). But then, is this, too, a deception? To what degree does this devotion to science actually prevail? First, realize that every chapter is followed by a brief "psychiatric commentary" provided by Dr. Paul Meier. Meier's devout Christianity sends a strong religious message through his interpretations of Korem's research. Unfortunately, this interpretation is frequently not only inaccurate, but it largely distorts and contradicts the integrity of Korem's naturalistic skepticism. Blatant adulation appears, for instance, when Korem will stress assumptions about falsely accepted cause and effect relationships and, in the same chapter, Meier carelessly comments that "if one parent is schizophrenic...., about 50 percent of the offspring will also eventually become schizophrenic" (p. 65). (Whither causality?)

This embarrassing perversion of Korem's insights is epitomized in Chapter 12. Here his magical wizardry yields to an emotional ontological argument for the truth of biblical scriptures. Why, one might ask, is this chapter included in the book? Does its obsession with "prophecy," "relevance," and "fulfillment" (the implicit syllogism) add sufficiently to the purpose of the book, to separate the pseudo-occult from the occult? Biblicism taken to this extreme seems incompatible with the radical "atheistic" attitudes underlying Korem's assault on fakery. Do Korem and Meier reasonably expect to inspire scientific explanations of phenomena by citing passages from a highly disputed resource, itself evolving for mystical or psychical reasons? To wit: does one prove the existence or fakery of unicorns by citing passages from mythology? Certainly not. In fact, even Christian readers who are interested in anomalies may also seriously question the value of Meier's commentary (and Chapter 12) in the book. Why adulterate a perfectly persuasive disputation of mystical phenomena with statements about Christian rehabilitation?

Perhaps Korem's need to include religious fervor in an otherwise scientific treatise of psychical events is because he felt the treatise was unpublishable without it. However, in guaranteeing his publication, did Korem sacrifice the scruples of scientific reasoning in order to conform to Christian expectations? Our belief that he did is a discouragement largely felt by the behavioral science community.

BOOKS BRIEFLY NOTED

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

- Bauer, Eberhard, and Walter von Lucadou, eds., *SPEKTRUM DER PARAPSYCHOLOGIE*. Freiburg im Briesgau: Aurum Verlag, 1983. 253pp. No price indicated, paperback. A festschrift for leading German parapsychologist Hans Bender on his 75th birthday. An excellent, though uneven as are most festschrifts, compendium of current analyses by both admirers and critics of the past work of Professor Bender. Hopefully, the book will become translated into English for the wider audience it deserves.
- Berger, Charles R., and James J. Bradac. *LANGUAGE AND SOCIAL KNOWLEDGE: UNCERTAINTY IN INTERPERSONAL RELATIONS*. London: Edward Arnold, 1982. 151+viii pp. \$14.95 paperback. A technical work of special relevance to understanding cold reading processes where the individual seeks meaning and uses linguistic strategies to avoid uncertainties.
- Billig, Otto, *FLYING SAUCERS: MAGIC IN THE SKIES: A PSYCHOHISTORY*. Cambridge, Mass.: Schenkman, 1982. 265+vii pp. \$ paperback. A very interesting study comparing apparition reports with UFO contact reports and a consideration of the magical thinking often involved. *Recommended*.
- Brannigan, Augustin, *THE SOCIAL BASIS OF SCIENTIFIC DISCOVERIES*. New York: Cambridge University Press, 1981. 212+xi pp. \$9.50 paperback.
- Bylinsky, Gene, *LIFE IN DARWIN'S UNIVERSE: EVOLUTION AND THE COSMOS*. Garden City, N.Y.: Doubleday, 1982. 238+xiv pp. \$17.95. A beautifully illustrated look at evolution large and small including a section on extra-terrestrial possibilities.
- Chubin, Daryl E., *SOCIOLOGY OF SCIENCES: AN ANNOTATED BIBLIOGRAPHY ON INVISIBLE COLLEGES, 1972-1981*. New York: Garland 1983. 202+xiii pp. \$30.00. Over 300 studies by historians, philosophers, psychologists, and sociologists of science, 80% of which are annotated, plus an introductory essay giving perspective. Not exhaustive but a selected bibliography including even presented papers as well as normal publications. *Highly recommended*.
- Cohen, Daniel, *THE ENCYCLOPEDIA OF MONSTERS*. New York: Dodd, Mead, and Co., 1983. 287+xi pp. \$14.95. Everything from the abominable snowman to the zeuglodon, in entertaining and careful but not scholarly fashion. Popular cryptozoology with something for everyone including the ufologists.
- Collins, H.M., ed., *SOCIOLOGY OF SCIENTIFIC KNOWLEDGE: A SOURCEBOOK*. Bath, England: Bath University Press, 1982. 238+iv pp. 5.9 pounds, paperback. An excellent collection of articles edited by a leading expositor of the "strong programme" for the sociology of science. *Recommended*.
- Corliss, William R., compiler, *TORNADOS, DARK DAYS, ANOMALOUS PRECIPITATION, AND RELATED WEATHER PHENOMENA: A CATALOG OF GEOPHYSICAL ANOMALIES*. Glen Arm, Md.: Sourcebook Project (P.O. Box 107; Glen Arm, MD 21057), 1983. 196pp. \$11.95. Another amazing volume by our leading anomalist, indispensable for anyone serious about these topics. *Highly recommended*.
- Davidson, Mark, *UNCOMMON SENSE: THE LIFE AND THOUGHT OF LUDWIG VON BERTALANFFY, FATHER OF GENERAL SYSTEMS THEORY*. Los Angeles: J.P. Tarcher, 1983. 249pp. \$15.95. A nicely done biography of a major scientist whose work has important implications for many areas of science including contemporary holistic approaches.
- de Camp, L. Sprague, *THE FRINGE OF THE UNKNOWN*. Buffalo, N.Y.: Prometheus, 1983. 208pp. \$16.95 clothbound, \$8.95 paperback. An excellent collection of de Camp's entertaining and knowledgeable essays on various aspects of science, reprinted mostly from science fiction magazines over the last 30 years. Particularly good in the areas of archaeology and ancient technologies, and full of fascinating items in the history of science and pseudoscience. Though certainly opinionated, de Camp is seldom dogmatic (as compared to Asimov and other such pop-scholars). I found this collection generally superior to his earlier collection of similar writings in *The Ragged Edge of Science* (1980).
- Dorson, Richard M., *MAN AND BEAST IN AMERICAN COMIC LEGEND*. Bloomington: Indiana University Press, 1982. 184+xix pp. \$20.00. A wonderful volume on the folklore of exotic animals from bigfoot to the sidehill dodger by a leading scholar and collector. Fascinating and amusing lore mixing tall tales, strange reports, and downright hoaxes. *Recommended*.
- Eberhart, George M., *MONSTERS: INCLUDING BIGFOOT, MANY WATER MONSTERS, AND OTHER IRREGULAR ANIMALS*. New York: Garland, 1983. 344+xiv pp. \$25.00. A remarkable bibliography, this volume is a must for anyone interested in cryptozoology, folklore of exotic animals, etc. 4,450 items located by Eberhart with excellent short introductory essays for the various categories. A most welcome volume. *Highly Recommended*.
- Ferrucci, Piero, *WHAT WE MAY BE: TECHNIQUES FOR PSYCHOLOGICAL AND SPIRITUAL GROWTH THROUGH PSYCHOSYNTHESIS*. Los Angeles: J.P. Tarcher, 1983. 252pp. \$6.95 paperback. A program based on the teachings of psychologist Roberto Assagioli which seeks to integrate "subpersonalities into holistic growth. Not a set of spiritual answers so much as a system for higher self-actualization.
- Fowler, Raymond E., *THE ANDREASSON AFFAIR: PHASE TWO*. Englewood Cliffs, N.J.: Prentice-Hall, 1982. 278pp. \$5.95 paperback. A followup to the earlier study of the controversial UFO contactee case. Though intriguing, all contactee cases based on hypnotic "recall" are deeply suspect in terms of any direct evidential value (which is not to say that they might not lead to testable hypotheses where hypnosis is not involved). Certainly, if one is to take such case reports seriously at all, the Betty Andreasson case is among the very best.
- Gardner, Martin, *THE WHYS OF A PHILOSOPHICAL SCRIVENER*. New York: Quill, 1983. 454pp. \$12.95 paperback. A remarkable and often surprising series of essays outlining Mr. Gardner's personal philosophy and his reasons. The essay "Why I Am Not a Paranormalist" should be of special interest to ZS readers, but Gardner's surprising views on immortality, prayer and God are also relevant. One can question Gardner's claims to wisdom, but his learning is broad, his thoughts provocative, and his writing style is remarkably clear given the opacity of some of the subjects discussed. Gardner may be a third rate philosopher, but he is a first rate scrivener.
- Gauld, Alan, *MEDIUMSHIP AND SURVIVAL: A CENTURY OF INVESTIGATIONS*. North Pomfret, Vt.: David & Charles, 1983. 287-xiv pp. \$18.95. A very important book by a leading proponent of the authenticity of survival evidence, presented in a careful and reasonable fashion despite the highly controversial character of the alleged phenomena. One can disagree with Gauld's conclusions (which are not dogmatically stated) but respect his tone and his arguments. Certainly, it is such work as Gauld's that responsible critics need to address. *Recommended*.
- Gilling, Dick, and Robin Brightwell, *THE HUMAN BRAIN*. New York: Facts-on-File, 1983. 192pp. \$15.95. A beautifully illustrated introductory and popularly written book. I found it entertaining and informative as well as a balanced presentation.
- Gross, Loren, E., *UFOS: A HISTORY, VOLUME ONE: JULY 1947-DECEMBER 1948*. New York: Arcturus Book Service (263 N. Ballston Avenue; Scotia, NY 12302), 1982. 169pp. \$12.95 spiralbound. A very important study, highly welcome and likely to result in an excellent series. *Recommended*.
- Grossinger, Richard, *PLANT MEDICINE, FROM STONE AGE SHAMANISM TO POST-INDUSTRIAL HEALING*. Boulder, Colorado: Shambala, 1982. 432pp. \$9.95 paperback. A revised edition of the 1980 work. A personal but fascinating anthropological-psychological integration viewing the whole realm of alternative medicines. Largely a philosophical rather than a scientific effort but covers much ground in sympathetic fashion that has startling freshness and insights of great power.
- Hicks, David, *TETUM GHOSTS AND KIN: FIELDWORK IN AN INDONESIAN COMMUNITY*. Palo Alto, Cal.: Mayfield, 1976. 143+x pp. \$6.95 paperback. An excellent student-oriented volume in the Explorations in World Ethnology series. Particular concern with the supernaturalism in this preliterate culture.

- Hoffman, Albert, *LSO: MY PROBLEM CHILD: REFLECTIONS ON SACRED DRUGS, MYSTICISM, AND SCIENCE*. Los Angeles: J.P. Tarcher, 1983. 210+xiii pp. \$7.95 paperback. Reflections and history by the discoverer of LSD, including his views on different realities, LSD and meditation, etc. An important work for those interested in the psychedelic movement and its history.
- Jenkins, Elizabeth, *THE SHADOW AND THE LIGHT: A DEFENCE OF DANIEL DUNGLAS HOME, THE MEDIUM*. North Pomfret, Vt.: Hamish Hamilton/David & Charles, 1983. 275+vi pp. \$32.50. An important new biography of Home with some new materials but essentially dependent upon his own and his wife's books. Certainly worth reading but a disappointment for anyone hoping for the definitive and balanced new study of this remarkable figure.
- Johnsgard, Paul and Karin, *DRAGONS AND UNICORNS: A NATURAL HISTORY*. New York: St. Martin's Press, 1982. 163+xi pp. \$9.95. A fanciful guide to unicorns and dragons including a checklist and field guide for the watcher. Whimsical and fun with many cute twists in reinterpreting the lore about these "endangered species."
- Kakar, Sudhir, *SHAMANS, MYSTICS & DOCTORS: A PSYCHOLOGICAL INQUIRY INTO INDIA AND ITS HEALING TRADITIONS*. New York: Alfred A. Knopf, 1982. 306+x pp. \$15.00. A western-educated Indian psychoanalyst examines the Indian approach to the treatment of emotional disorders, based on fascinating field work and case studies. An important cross-cultural analysis.
- Kamler, Howard, *COMMUNICATION: SHARING OUR STORIES OF EXPERIENCE*. Seattle, Wash.: Psychological Press, 1983. 274+vi pp. \$24.95. Philosopher Kamler presents a new Theory of Stories which replaces a sharing model of communication over the more usual model of a source message over a channel to a receiver which is less interactional. Since he deals with how stories are resisted and the problems of eyewitness testimony, the book is relevant to the study of anomalies and their communication. The book is a model of clarity, so much so that its message seems more simple than it really is. The book is perhaps most valuable for its explanation of miscommunication takes place so often.
- Klass, Philip J., *UFOS: THE PUBLIC DECEIVED*. Buffalo, N.Y.: Prometheus Books, 1983. 310+viii pp. \$17.95. This third volume of critical analysis of UFO evidence has the same problems and virtues of the authors other UFO books. Still, this book needs to be read by every serious ufologist. Despite Klass's excesses and errors of omission of contrary details and his general style which so offends many readers, he has unearthed important findings and made significant arguments that need reply from critics who too often prefer to ignore him. This volume seems to have less personal vitriol and ad hominem attack in comparison to his second book (*UFOS Explained*) and neglects his previous explanation of UFOs as anomalous plasmas (in *UFOS Identified*). Klass presents an excellent brief for "the prosecution," but--as with all such manifestly "reasoned" presentations-- we need to hear from the defense. Bruce Maccabee has already responded to Klass on the New Zealand case, with very damaging effects upon Klass's arguments. And the critiques of Klass's past books have shown him to earlier misrepresent a great deal. So, it is too early to pass judgement on this latest attack on ufology. As with all such books, pro or con UFOS, we can not simply take the presentations of evidence and argument at face value and assume that internal consistency in the book establishes congruence with the factual state of affairs. But Klass is the leading "lawyer" for the anti-UFO forces, and it is high time that his critics give his arguments and evidence the careful examination --and perhaps rebuttal-- that they deserve. I hope the UFO community will respond more quickly to this new book than they did to his earlier volumes. Though I think the UFO proponents severely damaged the arguments in Klass's earlier books, those of us non-specializing in this area must stand impressed by this new book's arguments until we can read public replies from Klass's opponents.
- Krippner, Stanley, ed., *ADVANCES IN PARAPSYCHOLOGICAL RESEARCH, VOLUME 3*. New York: Plenum, 1982. 338+xiv pp. \$32.50. This collection of six outstanding review essays represent the state of the art in contemporary parapsychology. No serious student of parapsychology should be without this series, and in my own view the volumes seem to be getting better each issue. Highly recommended.
- Laudan, Rachel, ed., *WORKING PAPERS IN SCIENCE & TECHNOLOGY, VOL. 2, NO. 1: THE DEMARCATION BETWEEN SCIENCE AND PSEUDO-SCIENCE*. Blacksburg, Va.: Virginia Tech Center for the Study of Science and Technology, April 1983. 200pp. \$4.00 (Available from: Center for the Study of Science in Society; Price House, Va. Polytechnic Institute and State U., Blacksburg, VA 24061). Papers presented at the Virginia Tech 1982 workshop. Eight important papers covering all sorts of anomaly topics. A real bargain.
- Leahey, Thomas Hardy, and Grace Evans Leahey, *PSYCHOLOGY'S OCCULT DOUBLES: PSYCHOLOGY AND THE PROBLEM OF PSEUDOSCIENCE*. Chicago: Nelson-Hall, 1983. 277+iii pp. \$25.95. An important book with some excellent analyses and a good historical perspective. The authors dismiss the possibility of methodological demarcation between real science and pseudoscience (a point on which I much disagree) and accept the socially negotiated distinctions (which I reject), but then they go on to argue that pseudosciences may become sciences (and vice-versa) and that there is rationality to be found in the pseudosciences and that useful knowledge (nonscientific) exists to be found in the pseudosciences. So they accept the label pseudoscience for what I prefer to call protosciences while not condemning them as irrational or anti-scientific. So, I end up agreeing with them given their definition of the scene. Recommended.
- Leary, Timothy, *FLASHBACKS: AN AUTOBIOGRAPHY*. Los Angeles: J.P. Tarcher, 1983. 395pp. \$15.95. Though I expected to dislike this book, I found myself quite fascinated by it. I suspect Leary owes much to his editor (based on what I have seen of his live performances), but the book is readable and informative.
- Lester, David, *THE PSYCHOLOGICAL BASIS OF HANDWRITING ANALYSIS: THE RELATIONSHIP OF HANDWRITING TO PERSONALITY AND PSYCHOPATHOLOGY*. Chicago: Nelson-Hall, 1981. 181pp. \$18.95. A good review of the experimental and general literature on graphology and both conservative and well balanced in its presentation of the evidence. A welcome book.
- Lethbridge, T.C., *GHOST AND GHOUL*. London: Routledge and Kegan Paul, 1961. 156+xi pp. 2.75 pounds. Lethbridge's account and analysis of his personal experiences while conducting research into the archaeology and history of the pagan gods in Britain. Entertaining and thought provoking.
- Levis, Ken, ed., *VIOLENCE AND RELIGIOUS COMMITMENTS: IMPLICATIONS OF JIM JONES'S PEOPLE'S TEMPLE MOVEMENT*. University Park: Pennsylvania State University Press, 1982. 207+xv pp. \$17.50. An important work which should be read by all the people who seem to constantly invoke the terrible example of Jim Jones when railing against cults and the alleged influence of irrationality slopping over from the study of the paranormal. It is not that simple, and this collection might open some eyes. Recommended.
- MacKenzie, Andrew, *HAUNTINGS AND APPARITIONS*. North Pomfret, Vt.: David & Charles, 1983. 240+xv pp. \$18.00. A fascinating survey of the major apparition cases based on the records of the British SPR, including some new evidence related to old cases. Must reading for anyone concerned with alleged hauntings, and a very important addition to the literature. This is perhaps the best first book one might read about this subject. Recommended.
- McConnell, R.A., ed., *ENCOUNTERS WITH PARAPSYCHOLOGY*. Pittsburgh, PA: Privately published by the author, 1982. 235 pp. \$9.00 paperback. A rather interesting selection of papers which should make an excellent anthology for use in courses dealing with parapsychology. I found this an excellent cross-section of papers from 1876 to 1980.
- McConnell, R.A., ed., *PARAPSYCHOLOGY AND SELF-DECEPTION IN SCIENCE*. Pittsburgh, PA: Privately printed by the author, 1983. 150+vii pp. \$7.00 paperback. A remarkable document whatever one might conclude about McConnell's own evaluations presented in this rather personal book which expresses McConnell's views on many matters, especially the self-deception he thinks is present among scientists including his own colleagues in parapsychology. The book is very uneven and includes several papers that were rejected by the psi journals. McConnell goes into some detail giving his analysis of why. The first paper in the volume deals with "extraocular image" in China. It is good to have this document now published and available, but Dr. C.K. Jen's paper giving his own observations was disappointing to me as I think it will be to others who have been trying to learn the details of Chinese parapsychological efforts. I found myself sympathetic to much argued by Prof. McConnell (though his final paper which deals with the future of our world betrays a superficiality in sociology as well as some cultural values I do not share), it seems likely that this book will have more lasting value for sociologists and historians of parapsychology than for the parapsychologists themselves.

- Mendonsa, Eugene L., *THE POLITICS OF DIVINATION: A PROCESSUAL VIEW OF REACTIONS TO ILLNESS AND DEVIANCE AMONG THE SISALA OF NORTHERN GHANA*. Berkeley: University of California Press, 1982. 270+xi pp. \$28.50. An excellent ethnographic study of the social control functions of divination in both religious and political spheres.
- Mertens, Gerald C., ed., *BEHAVIORAL SCIENCE BEHAVIORALLY TAUGHT*. Lexington, Mass.: Ginn Custom Publishing, 1980. 64pp. No price indicated. This is a compilation of papers dealing with conjuring and psychology, including seven new short papers by Mertens intended to inoculate students against "irrationality" in materials about the paranormal. A useful compilation even if presented in a rather one-sided way. Of particular interest is Merten's essay "Are Mark Wilson and Doug Henning Behavioral Psychologists but They Just Don't Know It?"
- Michell, John, and Robert J.M. Rickard, *LIVING WONDERS: MYSTERIES AND WONDERS OF THE ANIMAL WORLD*. New York: Thames and Hudson, 1983. 176pp. \$9.95 paperback. A wonderful illustrated collection of Fortean. Basically a book for the mystery monger rather than the scientifically oriented reader, the book is well done and generally careful with its facts (though contrary arguments are sometimes unmentioned). I particularly loved the section on cats with wings.
- Mishlove, Jeffrey, *PSI DEVELOPMENT SYSTEMS*. Jefferson, N.C.: McFarland, 1983. 299+xi pp. \$24.95. A revision of Mishlove's doctoral dissertation, it is valuable for the wide range it surveys, but Mishlove shows frequent lapses of critical judgement one should not expect of a scientist. For example, to say that critics who claim Uri Geller is a trickster offer evidence "less substantiated than the evidence for the genuineness of the phenomena" (p. 115) shows skewed and scientifically improper balance Mishlove brings to his subject. He apparently does not accept that the burden of proof in science is on the claimant and that this burden is a heavier than usual one for the psi proponent. Still, the survey has its virtues in that one will likely learn about systems previously unheard of by the reader. As a general book in the psi field, it is welcome, but it is unfortunate that too many people will probably think that that this book represents the state of the art in parapsychology when it seems likely many parapsychologists who are more conservative than Mishlove will be unhappy with this book.
- Moulton, H.J., *HOUDINI'S HISTORY OF MAGIC IN BOSTON, 1792-1915*. Glenwood, Ill.: Meyerbooks (P.O. Box 427; 235 West Main St.; Glenwood, IL 60428), 1983. 159+xxv pp. \$35.00. A facsimile of the original manuscript prepared for Houdini by Moulton compiling information on conjurers which Houdini planned to use for his own history of magic never completed. With playbill illustrations from the Christopher Collection and an introduction by Milbourne Christopher. Full of early curiosities including "human salamanders" or fire-resistance displays, telepathy "demonstrations," etc. A remarkable primary source and most welcome.
- Nelli, Raymond A., *INTRODUCTION AND INFORMATION COMPENDIUM*. Springfield, Va.: High Energy Electrostatics Research (P.O. Box 5286; Springfield, VA 22150), 1982. 188pp. \$20.00 spiralbound. This is the introductory and over-view volume in a 3-volume series on (1) Antigravity and UFOs, (2) Paranormal Phenomena, and (3) Parenergy, being prepared for complete publication in 1984. Electronics engineer Nelli has compiled an extraordinary series of patents (12 published in this first volume) which he and his colleagues have managed to unearth. An appendix includes information and price lists on high voltage equipment available.
- Nelli, Raymond A., *ANTIGRAVITY AND UFOs*. Springfield, Va.: High Energy Electrostatics Research, 1982. 448pp. \$65.00 spiral-bound. The first volume in this series of three, this book deals with electro-gravity propulsion systems, UFOs and VTOLs (Vertical-Take-Off-and-Landing Craft), and psychotronics and energy. On the one hand, this volume includes much of a speculative character, much of it badly informed as in the discussion of the "Philadelphia Experiment" literature which neglects its critics, but the strength of the volume is in the remarkable patents published and compiled here for the first time. A series of experiments for demonstration/replication are outlined along with the commentary. But the bulk of the volume consists of over 40 patents (including French, Austrian and Italian ones) including 5 for VTOLs. Fascinating and perhaps promising stuff, but technical evaluation is beyond the competency of this reviewer. One needs to remember that a patent on something does not necessarily mean it works as described (contrary to a lot of popular opinion).
- Nickel, Joe, *INQUEST ON THE SHROUD OF TURIN*. Buffalo, N.Y.: Prometheus Books, 1983. 178pp. \$14.95. I found this a very impressive and seemingly well done job of debunking claims made for the shroud of Turin. It is certainly far better than some of the misguided attacks in the literature. Recommended.
- Quebedeaux, Richard, ed., *LIFESTYLE: CONVERSATIONS WITH MEMBERS OF THE UNIFICATION CHURCH*. Barrytown, N.Y.: Rose of Sharon Press, 1982. 218+x pp. \$9.95 paperback, \$12.95 hardbound. An interesting propaganda volume since its publisher is an organ of the Unification church. The volume does eliminate some of the stereotypes held by many critics of the Moonies.
- Randi, James, *TEST YOUR ESP POTENTIAL*. New York: Dover, 1982. 51pp. \$3.50. The book's back cover describes the author as "generally considered the foremost authority on ESP phenomena, research and fraud" and he is identified as associated with the Committee for the Scientific Investigation of Claims of the Paranormal which the author writes "has looked into hundreds of cases where persons have claimed psychic powers" (a demonstrably false statement). The book is really an example of what Martin Johnson has called "pornoparapsychology" but this time done by a critic. The book includes a set of cards, "similar" to the Zener cards, which are on such thin boards that one can easily see through the backs to the symbols printed on the other side if there is a modicum of light coming through them. If Randi does not receive thousands of letters from people claiming to have demonstrated ESP by using such cards, that will be paranormal.
- Riccardo, Martin V., *VAMPIRES UNEARTHED: THE VAMPIRE AND DRACULA BIBLIOGRAPHY OF BOOKS, ARTICLES, MOVIES, RECORDS, AND OTHER MATERIAL*. New York: Garland, 1-83. 135+viii pp. \$18.00. The second in this series of Topical Bibliographical Guides to Anomalies under the general editorship of J. Gordon Melton. This remarkable compendium is indispensable for anyone interested in the subject of vampirism. Riccardo's thoroughness is commendable and amazing. A very welcome volume. Highly recommended.
- Roberts, David, *GREAT EXPLORATION HOAXES*. San Francisco: Sierra Club Books, 1982. 182+x pp. \$12.95. An important volume for anyone interested in the problem of fraud in science. Deserves far more attention than this book received.
- Rogo, D. Scott, *LEAVING THE BODY: A PRACTICAL GUIDE TO ASTRAL PROJECTION*. Englewood Cliffs, N.J.: Prentice-Hall, 1983. 190+iv pp. \$5.95 paperback. A very nice survey of the systems used by different groups to obtain OBEs. Not basically an attempt to scientifically evaluate the experience so much as a how-to book for the would-be experimenter.
- Ronan, Colin A., *SCIENCE: ITS HISTORY AND DEVELOPMENT AMONG THE WORLD'S CULTURE*. New York: Facts-on-File, 1983. 543 pp. \$29.95. A nicely illustrated text-book-like survey of the history of science which I thought was very well done for a popular-level volume, and the emphasis on cross-cultural history is most valuable and informative.
- Russell, Peter, *THE GLOBAL SPECULATIONS ON THE EVOLUTIONARY LEAP TO PLANETARY CONSCIOUSNESS*. Los Angeles: J.P. Tarcher, 1983. 251pp. \$8.95 A mixture of humanistic psychology and futurism, arguing that a worldwide transformation of consciousness is probable and consistent with our past evolution.
- Scott, Gini, *THE MAGICIANS: A STUDY OF THE USE OF POWER IN A BLACK MAGIC GROUP*. New York: Irvington, 1983. 219pp. \$10.95 paperback. A highly readable ethnography of a Satanic cult (here called "Hutians") which was a schismatic outgrowth from the Church of Satan when it split off in 1975. Scott's analysis of the psychodynamics involved is controversial, but the ethnographic content is excellent, and the book should be read by anyone interested in contemporary Satanism.
- Smith, Jody Brant, *THE IMAGE OF GUADALUPE: MYTH OR MIRACLE?* Garden City, N.Y.: Doubleday, 1983. 175+xiv pp. \$14.95. An interesting story about the "miraculous" cloth in Mexico which should give Shroud of Turin advocates a good competitive run for capturing public interest. The image is allegedly supernatural in origin and scientific tests including computer enhancements are cited as evidential of the supernatural origin of this well-known portrait of the Virgin Mary.
- Stafford, Peter, *PSYCHEDELIC ENCYCLOPEDIA* (revised edition). Los Angeles: J.P. Tarcher, 1983. 420pp. \$12.95 paperback. A remarkably thorough compendium on narcotics' history, botany, pharmacology, effects, purity tests, etc.

- Thalbourne, Michael A., compiler, *A GLOSSARY OF TERMS USED IN PARAPSYCHOLOGY*, North Pomfret, Vt.: William Heinemann, 1983. 91+vi pp. \$8.50. A useful compendium of both terms and their origins. Some terms, especially from the skeptics' side (e.g., "cold reading") are missing, but hopefully there will be a later edition and even "zetic" may get it in.
- Walker, Stephen, *ANIMAL THOUGHT*. Boston: Routledge & Kegan Paul, 1977. 427+xiv pp. \$35.00. A very impressive survey of the literature highly recommended to those interested in animal consciousness, learning, memory, etc.
- Wallis, Roy, ed., *MILLENIALISM AND CHARISMA*. Belfast, Northern Ireland: Social Sciences Department, The Queens University (Belfast BT7 1NN), 1982. 318+viii pp. \$20 hardcover (postpaid). This remarkable symposium volume has been specially produced by this university department because of its high quality but lack of commercial marketability. ZS readers should find this volume of special interest and should call it to the attention of others since distribution and publicity for the volume is minimal. Papers include excellent social science papers dealing with: charisma in new religions; the UFO cult of Bo and Peep; Werner Erhard's est; recruitment into the Unification Church; the Babi and Baha'i religions; and Melanesian Cargoism and medieval European chillasm. A very significant work for sociologists and anthropologists of religious cult movements. Highly recommended.
- Max, Benedicta, *MIRACLES AND THE MEDIEVAL MIND: THEORY, RECORD AND EVENT, 1000-1215*. Philadelphia: University of Pennsylvania Press, 1982. 323+xx pp. \$25.00. A fine scholarly study of early Christian miracles of all sorts showing their interrelationship with social life in the Middle Ages and with special attention to their propaganda value in the record of historical events.
- Weinstein, Donald, and Rudolph M. Bell, *SAINTS AND SOCIETY: THE TWO WORLDS OF WESTERN CHRISTENDOM, 1000-1700*. Chicago: University of Chicago Press, 1983. 314+xii pp. \$25.00. A study of 864 saints who lived between 1000 and 1700, using multi-variate analysis on this data. Fascinating collective biography.
- Whelan, Elizabeth M., and Frederick J. Stare, *THE 100% NATURAL, PURELY ORGANIC, CHOLESTEROL-FREE, MEGAVITAMIN, LOW-CARBOHYDRATE NUTRITION HOAX*. New York: Atheneum, 1983. 304+xiv pp. \$14.95. A debunking book which, whatever questions might be raised about the objectivity of its authors who are hardly disinterested parties, is well worth reading and is surely an antidote to much of the faddism and nonsense around.
- Yeterian, Dixie, *CASEBOOK OF A PSYCHIC DETECTIVE*. Briarcliff Manor, N.Y.: Stein and Day, 1982. 197pp. \$14.95. An autobiographical account of a psychic sleuth which is noteworthy, when compared to similar other books, for its absence of documentation to validate the author's claimed successes.



THE

CENTER FOR

SCIENTIFIC ANOMALIES RESEARCH

The Center for Scientific Anomalies Research (CSAR) is a private center which brings together scholars and researchers concerned with furthering responsible scientific inquiry into and evaluation of claims of anomalies and the paranormal. The Center will:

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

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

- Prof. George Abell (Dept. of Astronomy; University of California, Los Angeles),
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 Prof. Richard Kammann (Dept. of Psychology; University of Otago),
 Dr. John Palmer (Parapsychologisch Laboratorium; Rijksuniversiteit, Utrecht).

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Mr. Robert J.M. Rickard (The Fortean Times),
Mr. Leslie Shepard (author-editor), and
Ms. Rhea A. White (Parapsychology Sources of Information Center).

The primary focus of the Center will be on the study and evaluation of bodies of anomalous observations rather than upon esoteric theories seeking to explain already known phenomena. The orientation of the Center is exclusively scientific, places the burden of proof on the claimant, and recognizes the need for a degree of proof commensurate with the extraordinary character of the phenomenon claimed. But the Center also wishes to promote open and fair-minded inquiry that will be constructively skeptical. We recognize that scientific anomalies, where valid, may be instruments and driving forces for reconceptualization and growth in scientific theory. Critically and constructively approached, legitimate anomalies should be welcomed by science rather than perceived as ill-fitting nuisances. History clearly demonstrates that tomorrow's science is likely to contain surprises, and tomorrow's theories are likely to explain some of what are today viewed as controversial anomalies. Also, tomorrow's explanatory theories may be in areas of science not now perceived as relevant to the anomalies being considered. Thus, "anomalistics" must necessarily be an interdisciplinary endeavor.

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CSAR is a private Center whose policies and governance are under the control of its governing board. Members and Consultants thus do not control CSAR, but their suggestions and criticisms are always welcome by the governing board. There are a variety of associations individuals may have with CSAR. These include the

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For further information or applications, please write to:

Dr. Marcello Truzzi, Director
Center for Scientific Anomalies Research
P.O. Box 1052
Ann Arbor, Michigan 48103

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The CSAR DIRECTORY OF CONSULTANTS invites qualified applicants. Our goal is to put together the most complete list of experts on anomalies and claims of the paranormal, and being listed in the DIRECTORY does not imply any association with CSAR. We want both critics and proponents for inclusion. If you think you would qualify to join this international network, please write CSAR for an application form. There is no obligation and there could prove important advantages. Or if you know others who would be qualified and should be included, please tell them.

Following is a list of just some of the persons who will appear in the forthcoming CSAR DIRECTORY OF CONSULTANTS. Help us expand our network of anomalists.

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Richard de Mille	Patrick Curry
J. Richard Greenwell	K.R. Rao
Gini Graham Scott	Gary Alan Fine
Gerd H. Hövelmann	Bruce Maccabee
Roger W. Wescott	Geoffrey Dean
Susan J. Blackmore	Irvin L. Child
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Adrian Parker	Paul T. Mountjoy
James Randi	Barry J. Greenwood
Willis Harman	Jenny Randles
Frank B. Dille	Donald J. Mueller

... and many more.

So, write for an application if you are a qualified anomalist. You will find yourself in excellent company.