Zetetic Scholar

An Independent Scientific Review of Claims of Anomalies and the Paranormal

No. 5

Professors on PSI

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Zetetic Scholar

AN INDEPENDENT SCIENTIFIC REVIEW OF CLAIMS OF ANOMALIES AND THE PARANORMAL

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Zetetic scholar

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LETTERS AND COMMUNICATIONS

I certainly enjoyed the bulk of Dr. Sebeok's article, "Close Encounters with Canid Communications of the Third Kind," but the section dealing with "Chris" (Page 9) I found deficient for three reasons. First, when an experimental paper in a scientific journal is available (Tests of Clairvoyance in a Man-Dog Relationship. Wood, George H., and Cadoret, R.J. Journal of Parapsychology, 22, 29-29), Sebeok's reliance on a popular book as an exclusive source for evaluating the work with Chris is improper. Secondly, Sebeok has misrepresented Pratt's wording in his quotation, "...the dog died in 1963, just when 'the trained psi research worker' was about to find the opportunity 'to investigate further along similar lines'." The paragraph in Pratt's book reads as follows:

> Speaking reservedly, I consider that these results present a strong challenge to the trained psi research worker to investigate further along similar lines when he finds the opportunity for further work with Chris is gone. I received a letter from Mr. Wood stating that Chris died in February 1963, at the ripe old canine age of fourteen years (Pratt, J.G. Parapsychology: An Insider's View of ESP. New York: E.P. Dutton & Co. 1966, Pp. 235).

Thirdly, given the clairvoyant tests done by Wood and Cadoret and reported in 1958 in which no one knew the order of the cards until after the dog's responses had been recorded, the "fourth possibility" Sebeok mentions of unconscious cues would seem to be eliminated.

--- James W. Davis
Research Associate
The Institute for Parapsychology
Durham, North Carolina

PROFESSOR SEBEOK RESPONDS:

I would like to thank Mr. Davis for his added reference and concur that, whenever feasible, the most authoritative citation available should be used.

However, I disagree with Mr. Davis' characterization of the Journal of Parapsychology as "scientific." Our views about what constitutes science and what superstition are, apparently, poles apart.

As for the tests done by Wood and Cadoret, they do not necessarily eliminate "unconscious cues." I discuss such matters of leakage in a long article, "Questioning Apes" (written in collaboration with Dr. Jean Umiker-Sebeok, shortly to be published by Plenum), in relation to experiments with chimpanzees and gorillas occasionally alleged to possess the rudiments of language.

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Professor Sebeok also asked for the publication of the following addendum and corrigenda re his article on canid communications (ZS,#3/4):

ADDENDUM

Ronald A. Schwartz's insightful article, "Slight of Tongue" (The Skeptical Inquirer 3, 1978, 47-55), appeared after I had completed my piece. His exposition of how the reputed psychic, Peter Hurkos, produced his illusions by ingeniously exploiting the uses of ambiguity precisely illustrates my poin, in a human context, about "presupposition" or semiotic keying. Accordingly, I wish to underline Schwartz's accurate and pertinent observation that "the gull is responding to his own internal construction, which naturally seems right or true to him. Ambiguity tends to permit an easy internal confirmation." The secret of the talking dog effect lies, as well, in this principle of inadvertent shaping of the source's message on the part of the destination.

CORRIGENDA

P. 3 title: for "Communications," read "Communication."

P. 4, l. 10: for 1933-39, read 1933:39.

P. 15, in the last paragraph of the text, lines 7 and 8 should properly read: "...in the message destination what should have been sought in the source..."

P. 16, note 4, line 20: for "Weltznschauungsfragen." read "Weltanschauungsfragen."

P. 18, add: Hahn, Emily, 1978. Look Who's Talking! New York: Thomas Y. Crowell. (See also the review of this book by Thomas A. Sebeok in the Times Literary Supplement, No. 3,990, p. 1041, September 22, 1978/)

P. 20: reverse the order of the Stapledon and Shepard entries.

****

I read with great interest the article by Dr. May and the attending comments. I thought that Mr. May's article was interesting, but at least, a few years behind the Velikovsky movement as evidenced in the magazines Kronos, SISR, and Catastrophism and Ancient History. Unhappily, the comments were even further behind as they seem to come out of the 1950's. Dr. Morrison's comments were really unbelievable. Mr. Jones also seems to be in the same vein, however, at least brought many substantive arguments. Concerning Mr. Jones' comments, I believe that the tiles from Ramses III were key artifacts in determining the correct time for the period. I do not believe that 1939 work by the Egyptian scholar, Hamza, is the definitive answer on the tiles. I believe that a blue ribbon panel of experts without preconceived notions should determine whether those marks on the tiles are truly Greek letters or unusual hieroglyphic marks.

I believe that all of us, the academic world, owe Dr. Velikovsky a great debt whether he is 100% right or 50% right, because he has forced many to do some original thinking.

--- Marvin Arnold Luckerman, Executive Editor
Catastrophism and Ancient History (Los Angeles)

****

I recently read the very interesting symposium on Velikovsky in Zetetic Scholar. One important question that seems still to be raised is, not the vehemence of the opposition to Velikovsky, which is understandable enough from several viewpoints, but why is it that Velikovsky has been so popular, both with the general public, and also attracting an organized movement of intellectual supporters? This is somewhat analogous to the great unanswered question in Kuhn, why should a revolutionary paradigm attract supporters? I think the answer has to be apart from whatever explanatory merits Velikovsky's theory may have, because there still remains the question of why any set of logic and evidence is taken seriously. (i.e. both Kuhnians and Velikovskians attacking their critics, take the position that facts and logic do not determine their acceptance; hence we should turn this implicitly sociological point upon the Velikovskians themselves.)

I do not propose to answer this question, but will offer a conjecture. Velikovsky seemed to be rather deliberately overturning a great many scientific and scholarly disciplines; the only one he has any faith in is psychoanalysis. Now certainly there was plenty of hostility between the psychoanalytic world and the world of academic science (mostly coming from the academic side; Popper's 1945 book went so far as to bracket psychoanalysis with Nazism as enemies of reason.) So Velikovsky - an Israeli psychoanalyst - might well have felt like launching a counterattack. Moreover, this is just when academic science was getting its full brunt of post-atomic-bomb publicity. An anti-science movement was just getting under way, from the humanitarian side, and this groundswell seems to have carried Velikovsky right on through today.

In this light, the vehemence of the early attack by scientists on Velikovsky takes on a new aspect. For academic science no doubt already felt itself threatened by the fascist movements, and the U.S. around 1950 was at the height of the McCarthyite hysteria. Velikovsky was felt to be not just an attack on some basic principles within science, but a symptom of a public mood that seemed very dangerous to academic scientists, all the more so because they must have felt they were threatened on several fronts simultaneously.

Today fascist anti-science has largely disappeared (except perhaps in some neo-fundamentalism), but the polarization of humanists against conventional academics goes on, probably deeper than ever. And in that light, I think we seriously have to worry about the long-term future of science. My comparative studies of the long-term dynamics of philosophy in various societies, for example, show many instances where much more rationally worked-out, coherent, sometimes even mathematically and empirically founded positions were displaced by successively cruder and more dogmatically asserted positions of the popularistic type. Perhaps the arrogance of the rationalistic intellectual communities contributed to provoking this. But it does make one wonder if we may not be seeing today the long slow preparation for another Alexandrian age.

---Randall Collins
Professor of Sociology
University of Virginia

EDITORIAL

The ZETETIC SCHOLAR represents an attempt to mediate strong viewpoints, both pro and con, on the paranormal. Such mediation is far from a simple matter. There is usually some truth on both sides, and there is a great danger of simply antagonizing both sides. It is generally far easier to "sell" a clear-cut position than it is to ask for tolerance and a spirit of agnosticism until full evidence can be examined. True Believer and True Disbelievers both are too often more interested in winning a debate than in engaging in mutually beneficial dialogue. Our viewpoint is that the ideal of science encourages us to seek truth, wherever we may find it, irrespective of our desires. In this sense, all the players in the game of science should be more interested in learning the objective nature of the world than in winning debates for personal justification. My experience has shown that experts on paranormal issues (on both sides) frequently have more in common with one another than with those scientists uninterested in such claims. The purpose of ZS, then, is to try to bring together the responsible proponents and critics who share allegiance to the rule of scientific method into serious discourse with mutual courtesy and respect for one another's positions. Recognizing that science is essentially a method and not a specific body of empirical claims, it should be possible for those who share this method to rationally compare arguments with a mind towards convincing one another and not just some outside audience that may be more interested in debate (with all the tricks and empty victories that can include) than in jointly sought truth by those who may have initially quite different expectations for where the facts may lead.

All serious scholars of the paranormal recognize that such claims are put forward by a wide spectrum of advocates and denounced by an equally wide range of critics. The expertize and rationality involved on both sides vary incredibly. But it is our view that the proper way to conduct scientific inquiry is to seek out the best advocates of any particular claim for examination. The mass media are full of irresponsible claims of the paranormal. This is unfortunate and deserves debunking. But a serious scientific journal such as ZS seeks to be will normally not waste its time on such matters. In our view, it would be like using a cannon to kill a fly. Our major concern at ZS is not the education and enlightenment of the general lay public; we are not on any moral crusade against the irrationality of the masses and those who feed them daily nonsense. That would be public relations for science and not the conduct of science itself. We leave that to others.

All of us have individual notions of just what deviant science claims should be labelled "crank" or "crackpot" ideas. There is little consensus about these matters and modern philosophy of science indicates there really are no proper a priori criteria we can use to make such judgements. From our perspective, terms like "crank," "crackpot," "pathological science," and "pseudoscience" too frequently prejudge the very claims needing examination. Scientists necessarily have subjective probability levels they will assign to new conjectures' being valid. Some ideas--such as Freud's classic example of someone's postulating the center of the earth is strawberry jam--seem patently absurd, and we would assign a probability of their

being true nearly zero. And it is perfectly proper for a scientist confronted with such a conjecture to choose to ignore it as not worth his time. But if a scientist chooses to examine such a claim and present his evaluation to other scientists, he must be willing to play by the normal rules of scientific evidence and present rational and proper arguments against the claim rather than simply ridicule it, misrepresent it, and use the authority of his position as a substitute for valid arguments. One can imagine even such a seemingly ridiculous claim as that the center of the earth is filled with strawberry jam put forward in a reasonable way ("You won't believe this, but I am a geologist and my careful measurements seem to reveal the center of the earth has a radical constitution of substances the closest chemical equivalent of which seems to be what might be called strawberry jam. I find it hard to believe myself, but I ask that you check my measurements and arguments and see if I am right or wrong."). The point is, we can and should assign low probability levels to strange claims being true, but we must recognize that long shots sometimes come up winners; so while we need not support such claims, we also should not seek to block inquiry into them by other scientists who may assign them higher probability levels. Highly speculative science can be bad or good science, but scientific ideas should not be judged poor science merely because they seem "far out." Judgements about good and bad science must refer to the character of the arguments presented, not the simple content of the claim itself.

ZETETIC SCHOLAR will generally seek to act as "amicus curiae" or friend of the court of science, not as a judge or jury. It may at times appear overly tolerant. That is a chance that must be taken if we are to have a free science in a free society. Parallels with civil liberties should be apparent. Science must avoid authoritarianism and the blocking of inquiry as its central feature. For many of us, the victory of science over religious oppression of ideas was a major battle and one which we do not want fought between forces of "orthodoxy" within institutionalized science and "heretical" scientists who put forward new hypotheses. As Anthony Standen long ago noted, current scientific claims can easily become "sacred cows," and there is little advantage (as Paul Feyerabend has argued) in replacing the old Priests of Theology with the new Priesthood of Establishment Science. Both run counter to the true spirit of scientific inquiry which is quite separate from the institutional vested interests of much of today's science-as-practiced.

Because of our concern with "scientific due process" and fairness in our proceedings, the editor will normally refrain from injecting his own reactions to some of the dialogues which he may feel were pretty clearly better argued on one side than another. Such comments may later come forward in ZS but, in general, an attempt will be made to let the discussants arguments speak for themselves on the assumption that the readers of ZS should be the judge and jury and not your editor. So, please do not construe editorial silence on arguments as indication of our agreement with those arguments. Your editor is only human and will surely make mistakes along the way, but it is essential that you readers see ZS as an objective and fair-minded forum if we are to successfully reach our goal of bringing about legitimate scientific discourse.

We appreciate your continued support and involvement with ZS. We recognize that ZS will always have a small audience, but it seems to be one of "those who count" and we continue our slow growth. We have some important articles and dialogues now in production, and we welcome your criticism, suggestions, and participation in our endeavor. --MT

ATTITUDES OF COLLEGE PROFESSORS TOWARD EXTRA-SENSORY PERCEPTION[1]

MAHLON W. WAGNER AND MARY MONNET

As we convey concepts of science and objectivity to our students and to our peers, we typically point to ourselves or to others like us (fellow scientists and professors) as living examples of how to use reason and objectivity in order to further the understanding of ourselves and the world around us.

However, Kuhn (5) and others have suggested that in reality progress in science is not of the orderly nature we conceive it to be, but rather that progress proceeds via revolutions of paradigms. It is argued that during these "revolutions" or crises the old paradigm is often defended on emotional rather than rational or objective bases. Border areas of a science are often the battlegrounds for these crises. In the case of Psychology the emergence of Parapsychology may serve as a case in point. Researchers in Parapsychology have often claimed that their research is unfairly judged by overly-harsh standards and subjected to undeserving criticism of fraud and faulty statistics which are not applied with equal rigor to research in other areas of Psychology. (Examples of this over-reaction by psychologists include: Hansel, C. E. M. ESP: a scientific evaluation. New York: Scribner, 1966 and Price, G. R. Science and the supernatural. Science, 1955, 122, 359-367, Bobbs-Merrill reprint # p-279.) With this in mind it is legitimate to inquire into the attitudes of scientists, scholars, and students regarding Parapsychology and related phenomena.

While testing the hypothesis that general university education leads to the decline in supernatural belief, and that scientific training in particular hastens this decline, Jahoda (4) at the University of Ghana (N=280 males), Pasachoff, et al. (7) at Harvard University (N=200 males), and Salter and Routledge (10) at the University of Pennsylvania (N=73 males and 22 females) found no overall support for either hypothesis, and with respect to extrasensory perception (ESP) found belief to average 12.0 (on a scale of 0=unqualified disbelief, 10=neither belief nor disbelief, and 20=unqualified belief).

Warner and Clark (12) and Warner (11) polled members of the American Psychological Association (352 replies of 603 and 349 replies from 515 sent out, respectively) to assess their attitudes toward research in Parapsychology. Favorable attitudes (ESP is an established fact or a likely possibility) were shown by 8% in 1938 and by 17% in 1952, and unfavorable attitudes (ESP is a remote possibility or an impossibility) were expressed by 50% and 49%, respectively. In both studies 89% felt investigation of ESP was a legitimate scientific undertaking, and 77% said it fell within the province of academic Psychology. Negative attitudes were reported as primarily due to "a priori" grounds and

[1]Appreciation is hereby expressed to Mr. Robert E. Schell for the endless hours of unrewarded assistance given the authors in computer programming time and data analysis.

experiments reported in journals, while those with positive attitudes reported these were due to having read books by Rhine and others.

Goodstein and Brazis (2) examined the biases of psychologists by asking them to evaluate a research study in astrology which reported either positive or negative findings. Those receiving the negative abstract rated the study as more valid and better designed than those receiving the positive abstract. (135 replies were from those sent the positive abstract and 147 from those sent the negative abstract, with 500 initially in each mailing.) In addition, the younger psychologists were more accepting of the positive results. The many emotional comments appended to the returns reinforced the suggestion that judgments in these border areas are based on other than rational and objective arguments.

In another survey of psychologists (8) it was found that of 235 Psychology departments responding (47% return rate) the majority (71.3%) felt that there should be no separate course of Parapsychology--although 62% felt that there should be at least minimal coverage of the topic in other courses. Negative comments centered around lack of relevance and importance to Psychology and lack of scientific evidence or credibility.

Recently Moss and Butler (6) surveyed their Psychology colleagues (N=37) as well as students (N=80) in California and found professors significantly more skeptical than students (2.31 vs. 3.81 averages on a 5-point scale with 1 indicating greatest disbelief and 5 greatest belief, respectively).

The most extensive recent survey of attitudes toward Parapsychology was conducted among the readers of a popular English journal, New Scientist (1). From a potential of 71,000 copies sold 1416 replies were received, mostly from persons possessing degrees and working as scientists and technologists. In contrast to the attitudes of psychologists 67% of those surveyed here were favorable toward ESP, and only 22% were negative. The majority (51%) of "sheep" (a classical Parapsychology term for believers; contrasted with "goats," the non-believers) reported their opinions were based on personal experience, and 40% reported newspaper accounts, books and scientific reports as their source. No data were presented on the source of the goats' attitudes. A briefer Gallup Poll (15 June 1978) noted that 51% of 1553 adults believe in ESP and that two-thirds of persons with college backgrounds do so.

Some of the problems with the previously cited studies include: (a) too few students sampled with vague questions; (b) only psychologists were surveyed more than 25 years ago; and (c) in the English survey an undetermined self-selection process resulted in 1416 replies from an unknown population, combined with a cursory quasi-statistical analysis

of selected aspects of the data leaving the reader in doubt about what else was, or was not, observed.

With the limitations of the previous studies in mind, and with the additional anecdotal suggestions that scientists, and particularly social scientists, are more unaccepting of research in the border areas of Parapsychology, this study was undertaken with the purpose of surveying the attitudes of college and university professors in various disciplines throughout the United States toward Parapsychology.

Method

Subjects

College catalogs or faculty directories were obtained from 120 colleges and universities selected at random from the 1968-1969 Cass and Birnbaum Comparative Guide to American Colleges. The only requirements to be selected were that the institution have at least 1,000 students and more than 100 faculty. From each directory 20 faculty were selected at random with the requirement that each of five general academic areas (natural science, social science, humanities, arts, and education) be equally represented. In this manner 2,100 questionnaires including prepaid addressed envelopes were mailed out, and 990 usable replies were received during 1973. It should be noted also that a preliminary sample of 300 "on campus" surveys were distributed at S.U.N.Y. Oswego--with 198 returns--to check for the possibility of ambiguous questions; none of which were found. Therefore, the total sample consists of 2,400 surveys mailed and 1,188 replies.

Materials and Procedure

The mimeographed one-page questionnaire was quite similar to those used by Warner and Clark (12), Warner (11), and New Scientist (2) with the addition of four biographical questions: sex, current academic field, birth order, birth month and year. Respondents could also request receiving the results or preserve their anonymity by removing the name and address portion of the sheet. (A copy of the questionnaire is appended.)

The data were analyzed primarily by use of cross-tabulation tables and related Chi Square tests and Pearson correlation coefficients.

Results

Biographical data of the respondents reflected trends found in the academic population generally. For example, 79% were males, 21% females; 37.5% were oldest, 14.0% only children, 25% middle children, and 24% youngest; with a median age of 41. However, the academic fields were not represented in the returns in proportion to the initial mailing (20%) with 25% of the total returns coming from the natural sciences, 20.7% from social sciences, 18% humanities, 15.2% arts, and 21%

education [$\chi^2(4) = 31.32$, $p < .001$]. In addition, there were significantly fewer returns than expected from universities with less than 1500 students as well as more than 10,000 students, and more returns than expected from universities with between 1500 and 3000 students [$\chi^2(4) = 164.1$, $p < .001$].

Attitudes toward ESP were quite similar to those found in England and the recent Gallup Poll with 66% favorably disposed and 23% negative. However, attitudes were considerably different when considering ESP in plants and animals where only 38% were positive while 49% were negative [$\chi^2(4) = 20.64$, $p < .01$].

Agreement was also found with previous surveys in that 84% consider the investigation of ESP to be a legitimate scientific undertaking, and 65% feel that it falls, at least in part, within the province of academic Psychology.

Table 1

A comparison of attitudes toward ESP in the literature

	Warner and Clark 1938	Warner 1952	New Scientist	this study
1. ESP is:				
an established fact	1%	3%	25%	16.3%
a likely possibility	7	14	42	49.3
merely an unknown	40	34	12	10.9
a remote possibility	36	39	19	19.4
an impossibility	14	10	3	4.1
2. Source of belief				
newspapers	29	24		53.5
books by Rhine	47	48		18.7
journal reports	41	35		26.1
hearsay	8	8		12.0
a priori	34	32		17.5
personal experience	2	8	(51% sheep)	10.9
3. A legitimate scientific undertaking				
yes	89	89	85	84
no	10	9	?	8
If yes, does belong within psychology				
yes	76	78	20	40.6
partly	2	3	42	23.7
no	7	11	11	2.8
4. Parapsychology is:				
Important and making steady progress			9	25.3
Important but making little progress			53	31.8
looked promising once, but very little in it			?	4.3
never promising			?	28.8
a pseudo-science			4	3.2
5. ESP in plants/animals is:				5.7
an established fact				33.7
a likely possibility				9.8
merely an unknown				22.6
a remote possibility				28.0
an impossibility				

When the various academic fields are considered separately, an interesting finding emerges which supports previously mentioned anecdotal reports. In Table 2 it can be seen that positive attitudes toward ESP are expressed by 73-79% of those in the humanities, arts, and education, while only 55% of those in the natural science and 56% of social scientists are positive; and of those declaring ESP to be an impossibility, 53% were in the social sciences [$\chi^2(16) = 110.2$, $p < .001$].

Table 2a

Relation between academic field and attitude toward ESP

	Academic Field					
ESP is	natural science	social science	humanities	arts	education	Total
an established fact	28	23	47	54	40	192
likely possibility	134	112	108	84	141	579
merely an unknown	43	31	17	14	18	123
a remote possibility	81	48	31	20	43	223
an impossibility	8	25	9	3	2	47
Total	294	239	212	175	244	1164

$\chi^2(16) = 110.2$, $p < .0001$

Table 2b

Comparison of the attitudes of social science professors, psychologists and Warner's 1952 survey toward ESP.

ESP is	all social scientists (this study)	psychologists (this study)	psychologists (Warner 1952)
an established fact	23	4	9
a likely possibility	112	21	49
merely an unknown	31	7	119
a remote possibility	48	16	136
an impossibility	25	25	36
Total	239	73	349

Social scientist vs. psychologists today, $\chi^2(4) = 137.69$, $p < .001$.

Psychologists today vs. psychologists, 1952, $\chi^2(4) = 50.39$, $p < .001$.

Other relationships that were consistently found indicated that those respondents positively disposed toward ESP were more likely to feel that it is a legitimate scientific undertaking, [$\chi^2(8) = 96.0$, $p < .0001$]; it is important to science and making progress, [$\chi^2(20) = 420.3$, $p < .0001$]; and to want to receive the results of this study, [$\chi^2(4) = 24.1$, $p < .0001$].

An important question to ask concerns the sources of information upon which the respondents based their attitudes toward Parapsychology. From a total of 14 categories of sources, the 6 most often cited were items in newspapers and magazines, 53.5%; books by Rhine and other parapsychologists, 18.7%; experiments reported in scientific journals, 26.1%; hearsay, 12.0%; a priori grounds, 17.5%; and personal experience, 10.9%.

When newspapers and magazines are considered, those citing this as a source were more positive toward ESP (70%) than those not citing this source (61%) [$\chi^2(4) = 26.4$, $p < .001$]. The same was true for books by Rhine; 77% of those reading such sources were positive while 63% who had not read these books were positive [$\chi^2(4) = 57.1$, $p < .001$]. Of the 307 people who cited scientific journals; significantly more said ESP was an established fact or an impossibility than those not citing this source [$\chi^2(4) = 18.9$, $p < .001$] strongly suggesting that respondents were reading dramatically divergent sources--perhaps the Journal of Parapsychology for the first group and Psychological Bulletin for the second. Fewer of those citing a priori grounds were positive toward ESP (56%) than those not citing it [$\chi^2(4) = 26.5$, $p < .0001$]. Finally, 91% of those referring to personal experience were positive toward ESP, while only 63% were positive who did not mention this experience [$\chi^2(4) = 75.1$, $p < .0001$].

There were marked differences in citing these sources depending on academic discipline: natural scientists had read fewer Rhine books and cited hearsay and a priori reasons more often; social scientists had read more Rhine books, and more scientific journals, and those in the arts and humanities more often cited personal experience [$\chi^2(20) = 50.75$, $p < .0001$].

Opinions concerning ESP in plants and/or animals showed the same relative trends mentioned above except that there was a much lower level of acceptance by respondents, and again differences appeared among the academic disciplines with natural and social scientists having a 2-to-1 negative opinion while the other areas were evenly divided [$\chi^2(16) = 64.18$, $p < .0001$]. Again those who wanted to be informed of the results were significantly more favorable than those who didn't [$\chi^2(4) = 28.5$, $p < .001$].

Table 3

Relation between academic field and attitude toward ESP in plants and/or animals

	Academic Field					
ESP in plants and/or animals is:	natural science	social science	humanities	arts	education	Total
an established fact	7	13	9	18	19	66
likely possibility	71	60	87	74	95	387
merely an unknown	35	20	16	18	20	109
a remote possibility	78	62	43	31	45	259
an impossibility	96	82	50	30	60	318
Total	287	237	205	171	239	1139

$\chi^2(16) = 64.18$, $p < .0001$

One final breakdown is of special interest in that it relates directly to the surveys by Warner (11, 12). Of the 239 social science respondents there were 73 psychologists. The attitude toward ESP of these psychologists differed significantly from the parent social science population [$\chi^2(4) = 137.69, p < .001$] primarily due to the fact that all 25 "ESP is an impossibility" responses, (53% of the total sample) were received from psychologists. In addition, the psychologists in this study differed from the Warner survey in their attitudes toward ESP [$\chi^2(4) = 50.39, p < .001$], primarily in being less neutral (10% here vs. 34% in 1952) and more negative (34% here vs. 10% in 1952).

Attempts to relate age, sex, or month of birth to attitudes toward ESP were unsuccessful, although older respondents in the arts were more negative toward ESP [$r(169) = .18, p < .015$] but this magnitude of correlation is hardly of practical importance. In examining the universities at which the respondents taught, there were no significant variations based upon geographic location or size of student body. However, those who taught at Lutheran/Catholic/Episcopalian schools were significantly more favorable and less negative toward ESP than their colleagues at either other religiously affiliated schools or other private or state-supported institutions [$\chi^2(4) = 15.68, p < .01$].

Discussion

The results presented here are in substantial agreement with many of the reported recent studies, except that the Gallup Poll found that only one-half of their total sample of 1553 adult Americans believe in ESP while two-thirds of those with college backgrounds do so. This coincides with the reports of Jahoda and others that a general university education does not lead to a decline in belief in the paranormal. In any case, it would certainly seem that college professors, as a group, have attitudes toward ESP that are much more positive than those of the American people as a whole [$\chi^2(1) = 45.8, p < .001$].

Perhaps the most important single finding to date was the greater negativity to ESP among respondents in the social sciences as well as the greater positive attitudes toward ESP by those teaching in the humanities, arts, and education. It is tempting to speculate that the latter three groups are more experientially oriented (and perhaps less sophisticated experimentally) and hence more disposed to accept evidence for a phenomenon which has been notoriously criticized for lack of scientific rigor, but which is readily amenable to personal experience and anecdote. Along this same line, perhaps the higher return rate of the questionnaire by natural and social scientists can be traced to their greater orientation, training, and motivation to answer questionnaires of a research nature (especially in a scientifically controversial field) than would be expected of non-research oriented professors in the arts and humanities.

It is also interesting to note that while there is a generally positive attitude toward ESP, psychologist respondents not only remained skeptical, but actually became less neutral and more hostile than was

reported 25 years ago by Warner (11). It is tempting to speculate that having read more journal articles and books by Rhine, and having a greater familiarity with research design and the potential pitfalls in experimental research, (e.g. Rosenthal, 9) may account for this greater degree of negativity toward ESP.

The low level of acceptance for ESP in plants and/or animals may reflect the poor wording of that particular question. At the time the questionnaire was mailed there was much adverse publicity in the media about failure to replicate the "Backster effect" of plant sensitivity to ESP. In retrospect, it would have been much better to ask one question relating to animals and another about plants.

One last comment is in order regarding the reliability of this study. Response rates for mail questionnaires typically vary from 20-40% with the lower figures being more predominant (3). Consequently, the return rate here of 49% coupled with a proportionality of sex ratio and birth order data found in the population of college professors combine to suggest that we have here, for the first time, an accurate reflection of the attitudes of American college professors toward ESP. These attitudes are surprisingly favorable indicating that perhaps Parapsychology has indeed found a much-sought-after place in legitimate academia. This would seem to be borne out by surveys indicating that courses relating to Parapsychology are taught for academic credit on many university campuses across the country [although 71% of Psychology departments surveyed in 1973 did not think such a course would be appropriate (8)] and by the additional observation that in 1969 the American Association for the Advancement of Science accepted the Parapsychological Association into affiliate member status.

In light of the high positive regard for ESP held by professors in the humanities, education, and the arts compared with the more modest approval shown by natural and social scientists or the strongly negative attitudes held by the psychologists in this survey, there are two possible questions which remain to be asked: (1) What must the field of ESP do to become accepted by the natural and social scientists--or as Kuhn suggests, must we wait and let the guardians of the "old paradigms" die out? or (2) What can psychologists do to alert their academic colleagues in particular, and the public in general, to the problems of uncritically accepting current research or performance claims of ESP as urged by Moss and Butler? Perhaps as a partial response to the second question we can take note of two new journals, The Skeptical Inquirer published by the Committee for the Scientific Investigation of Claims of the Paranormal and the more recent The Zetetic Scholar edited by Marcello Truzzi at Eastern Michigan University.

Summary

A survey of the attitudes of 1188 American college and university professors toward Parapsychology was conducted.

Overall professors were favorable toward the existence of extrasensory perception by a 2-to-1 ratio. This fell to a 38% positive attitude toward ESP in plants and animals, a more controversial topic.

There were marked differences in attitudes between the natural and social science professors (55% positive) and professors in the arts, humanities, and education (77% positive). The least positive toward ESP were psychologists (34%), and psychologists also accounted for 53% of all highly negative attitudes ("ESP is an impossibility").

When considering the basis of their beliefs toward ESP, there were again striking differences with professors in the arts and humanities more often citing personal experience while social scientists read more books and scientific articles and natural scientists relied more upon hearsay and a priori reasons and read far fewer books by Rhine and others. The age, sex, and birth order of professors were of no importance in determining attitudes toward ESP.

Appendix A

Survey of Attitudes towards Parapsychology

1. In your opinion is "extra-sensory perception"
   a. an established fact
   b. a likely possibility
   c. a remote possibility
   d. an impossibility
   e. merely an unknown

2. Is this opinion based on:
   a. reports in newspapers and magazines
   b. books by Rhine, Soal, and other parapsychologists
   c. experimentation as reported in scientific journals
   d. hearsay
   e. purely a priori grounds
   f. other (summarize briefly)
   _____

3. Do you consider the investigation of extrasensory perception a legitimate scientific undertaking?
   a. yes
   b. no
   c. not sure
   If so, does it fall within the province of academic psychology?
   a. yes
   b. partly
   c. no
   d. don't know
   e. other (briefly summarize) _____
   _____

4. Which of the following statements most closely matches your attitude to parapsychology:
   a. Parapsychology is a very important aspect of science and is making steady scientific progress.
   b. Parapsychology is a very important aspect of science but it is making little if any progress.
   c. Parapsychology looked promising at one time but it now looks as if there is very little in it.
   d. Parapsychology has never looked promising but one can see that some scientists might feel it to be worth some effort.
   e. Parapsychology is pseudoscience and broadly speaking for cranks.
   f. Extra comments _____
   _____

5. In your opinion, ESP in plants and/or animals is:
   a. an established fact
   b. a likely possibility
   c. a remote possibility
   d. an impossibility
   e. merely an unknown

6. Sex: _____M _____F

7. Current academic field _____
   _____

8. Birth Order:

   _____ oldest
   _____middle
   _____youngest
   _____only child

9. Birthdate:

   _____month _____year

If you would like to receive the results, please fill in the following:

Name:

Address:

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NOTE: After the article was submitted, the authors received a recent M.A. Thesis by Laura P. Otis, University of Toronto (1979) on a "Survey of Extraordinary Beliefs." In essential agreement with the present study, she found professors to be more skeptical than either college students or the general public in Canada, and that psychologists were the most skeptical.

A DIALOGUE ON "STATISTICAL PROBLEMS IN ESP RESEARCH"

PROLOGUE

The July 1973 issue of Science included the article "Statistical Problems in ESP Research" by Dr. Persi Diaconis, a statistician at Stanford University. In addition to his academic credentials, Dr. Diaconis is a highly respected member of the conjuring fraternity and is unusually skilled in and knowledgeable about the art of deception. Though a skeptic towards psi research claims, he is generally regarded by fellow critics as an open-minded and receptive analyst, one critical of but not hostile to psi research. Many proponents of parapsychology see him as a "friendly critic."

Unfortunately, the publication of Dr. Diaconis's critical article in Science has highlighted some problems that go well beyond the substantive content of his article. Many in the parapsychological community were distressed at the appearance of his article because it seemed to contribute to what they perceived as a continuing problem of "second class citizenship" for parapsychology within the American Association for the Advancement of Science even psychological Association is an affiliate of the AAAS. On the one hand, psi proponents complained that Science has refused to publish articles on research supporting their claims. Psi supporters have argued that articles submitted to Science in the past have sometimes met the standards for research published on other areas of science, but all such articles have been rejected by Science. Those supporting such rejections have argued that the psi studies submitted did not meet the standards for rigor demanded, especially for extraordinary claims which, they add, require extraordinary evidence.

The publication of Dr. Diaconis's article must be viewed against this background of what many psi researchers perceive to be a negative and unscientific bias against parapsychology at Science. Dr. Diaconis's article critique was perceived, then, as a

further demonstration of such alleged bias by Science, but on two rather distinct grounds. The first concerned the substantive quality of the article;it was argued that the article did not conform to the similar high standards used in evaluating pro-psi articles. This argument centers around the substantive content of Diaconis's article. The second grounds for complaint, however, concerned the manner in which the article was reviewed and refereed, an issue which does not really involve the article's contents directly. It is this second issue that needs clarification by Science before any real judgement is possible.

Though the article was informally reviewed by parapsychologists at the invitation of Dr. Diaconis, it has been charged that no parapsychologists were involved in the formal review process conducted by the journal itself. Since the article attacks parapsychology generally, and since parapsychologists are members of the AAAS which publishes Science, it would seem appropriate that at least one parapsychologist (and it should be remembered that not all members of the Parapsychological Association accept the reality of psi) be consulted about the article in any formal review process. This is not to argue that any parapsychologist should be given any sort of veto rights towards such articles, merely that "due process" should involve someone from the area criticized. Since peer review of articles is done confidentially, it is by no means certain that no member of the Parapsychological Association was formally consulted about Dr. Diaconis's article. But the parapsychological community is a small one, and the rumor that no parapsychologists were formally involved seems to have been generally accepted among parapsychologists.

Since the involvement of a parapsychologist in the formal refereeing process would eliminate this issue, I wrote to Science's Editor, Dr. Philip H. Abelson, asking him if he could inform me (and thereby ZETETIC SCHOLAR readers) if any parapsychologist had been thus involved (without naming anyone, of course). I have received no reply from Dr. Abelson to my letter sent him last June. The matter therefore remains clouded in mystery.

The norms of scientific etiquette for such matters are by no means clear. It would seem to me that if an "extremist" in psychology were to submit an article to Science denouncing the reality of the basic variables studied by sociologists (such articles by "methodological individualists" in psychology attacking sociology--e.g., the denial of the reality of groups by psychologist Floyd Allport--were once quite popular). it would surely be protested if no sociologists were consulted about the article. In similar fashion, an article submitted by a physiologist denying the validity of some mentalist construction of the psychologists would surely be submitted to psychological as well as physiological experts for refereeing. This is particularly true for an interdisciplinary journal like Science whose support comes from the different disciplines.

Whether or not parapsychologists were involved in the review process for the Diaconis article, we may never know. If a parapsychologist was formally involved, I hope he/she or Dr. Abelson will let us know and thereby clear the air of what may be a false issue. If one was not involved, many of us hope that such courtesies will be extended to the parapsychologists in the future. In any case, these criticisms are quite independent of the content of Dr. Diaconis's article, and Dr. Diaconis is in no way respon-

sible for the picking of referees for his article. In fact. since he obtained prior comments from parapsychologists on his own, it should be obvious that he welcomed constructive criticism from all quarters.

The matter has been further exacerbated by the disinterest (if not outright refusal) of Science to publish an extended, detailed criticism of the Diaconis article. Dr. Edward F. Kelly of the Duke University School of Engineering submitted such a critique of Dr. Diaconis's Science article. His submission included a cover-letter giving some of Dr. Kelly's reasons why he felt it important that Science publish his reply, as follows:

> The fact that this article [by Dr. Diaconis] has appeared in Science automatically makes it an unusually serious matter for everyone actively involved in parapsychology research. For me personally it is doubly serious because a substantial portion of the paper is devoted to an (indirect) attack on experimental work carried on by myself and several close colleagues. I am also directly involved in another way, inasmuch as I am co-author of a major survey of statistical methods in parapsychology published in B. Wolman's Handbook of Parapsychology (van Nostrand Reinhold, 1977).
>
> I therefore feel not only that some sort of sytematic reply to Dr. Diaconis should be encouraged by Science, but that I am at least one of the central persons from whom such a reply would be most appropriate. In that light I have prepared the enclosed critique.

Though the article was sent to Science in July 1978, Dr. Kelly's reply to Dr. Diaconis was not accepted for publication. Since I agree with Dr. Kelly that his detailed critique deserves placement in the public record, I am pleased to now publish it in ZETETIC SCHOLAR. In addition, I am gratified that Dr. Diaconis (who is a Consulting Editor for ZS) also encouraged me to publish Dr. Kelly's piece and agreed to write a rejoinder which here follows Dr. Kelly's reply. It also seems likely that further dialogue on these issues will be included in future issues of ZETETIC SCHOLAR as both these authors and the readers of ZS bring us further observations and analysis of the points raised.

Finally, after reading the original article by Dr. Diaconis in Science, 201 (July 14, 1978), 131-136, and the reply and rejoinder in this issue, ZS readers might further examine the following relevant additions to this Dialogue.

"ESP Research" (Letters on the Diaconis article), from Charles T.Tart and from Harold E. Puthoff and Russell Targ with a reply by Persi Dicaonis, in Science,202 (December 15, 1978), 1145-1146.

K. Ramakrishna Rao, "Psi: Its Place in Nature," Journal of Parapsychology, 42, #4 (December 1978), 276-303.

-- M. TRUZZI

REPLY TO PERSI DIACONIS

EDWARD F. KELLY

Persi Diaconis has recently published in Science an article entitled "Statistical Problems in ESP Research." (1) Given the author's qualifications as a professional statistician, a reader unacquainted with the current state of parapsychology research might innocently suppose this article to contain a comprehensive and scholarly appraisal of important methodological problems pervading this still controversial field.

However, it does not. The principal issues Diaconis raises are largely irrelevant to the main body of published experimental research. Furthermore, he makes a number of incorrect and/or highly misleading statements regarding certain clearcut matters of fact, as well as a number of other assertions of a rather personal, subjective and uncertain sort uncharacteristic of articles in Science.

In this reply I will document these remarks, but first I should identify myself. I have been actively involved in experimental parapsychology for the past six years. I am also the main author of the series of published studies involving the special subject BD, whom Diaconis discusses at length; and co-author of a comprehensive survey of statistical methods, published in the recent Handbook of Parapsychology (B. Wolman, ed., 1977), to which Diaconis refers with apparent approval.

The central failing of Diaconis' paper is that it repeatedly characterizes as typical of experiments in parapsychology situations and procedures which are in fact extremely atypical. I will discuss the two main examples at some length, using his headings.

"Feedback Experiments" (p. 134)

Roughly one-third of the article is devoted to this topic. Diaconis introduces some new methods of analysis for situations in which a subject is guessing successive cards from a "closed deck" (composition fixed, only order randomized) and receiving some specified degree of feedback after each guess. This is certainly an interesting and potentially useful contribution. However, it is extremely misleading to suggest, as Diaconis repeatedly does, that experiments of this type are common in parapsychology. Throughout this section, Diaconis systematically blurs for an unwary reader an essential methodological distinction which has been familiar to virtually every worker in the field since the 1930's, namely the distinction between closed-deck and open-deck procedures. In "open-deck" procedures with cards, for example, each successive target is generated independently, via sampling with replacement, random-number tables, mechanical or electronic randomization, etc. Under these circumstances, feedback of any level of completeness is permissible and does not affect the statistics. This is the way the great majority of feedback experiments in parapsychology have been done. These points, furthermore, are entirely clear from the survey by Tart to which Diaconis refers. After bemoaning in his

chapter one the scarcity of work utilizing feedback, Tart briefly reviews studies carried out during the past decade which involved any kind of feedback aspect. In the process he reaches far out into the periphery of the main research literature, surveying abstracts and unpublished studies as well as normal full-length reports. Even so, only four studies can be clearly identified as combining trial-by-trial feedback with a closed-deck testing regime. Three of these occurred in a single unpublished master's thesis, and all are clearly labeled as methodologically defective. To represent such procedures as "common" in parapsychology is therefore a gross misrepresentation. (2)

"No Information Case" (p. 134)

In the same section, still talking about the closed-deck situation, Diaconis mentions another long-familiar result, namely that in the case of no trial-by-trial feedback the variance of the distribution of the number of correct guesses can vary widely as a function of the response frequencies, and becomes maximal in the event all responses are equally frequent. Lest readers should wrongly infer that such response factors might account for the production of large numbers of excess hits artifactually, however, let me add that the principal direction of change in the variance is actually downward rather than up, relative to the simple binomial variance used in the "standard" calculations.(3) For the case of the standard ESP deck (5 each of 5 cards) the maximum (matching) variance is only 1.04 times the corresponding binomial variance. For the playing card deck, the correction is even smaller.

It is worth pointing out here that this closed-deck no-information situation is the one that dominated the early controversy over J.B. Rhine's methods of statistical evaluation. Rhine and his group had applied to this situation an analysis model which strictly assumes independent trials (3), and various critics quite appropriately questioned its appropriateness in the closed-deck case. In the ensuing dialogue several statisticians made important contributions to the evolving theory of the direct-hit distributions that arise from the matching of two decks of specified composition, one of which could be conceived as representing calls and the other, targets. The practical result was that even in the worst (matching frequencies) case the closed-deck no-information situation in fact differs only trivially from the true binomial, and both are well approximated even in quite short series of guesses by the normal. Thus Rhine's basic methods of statistical evaluation were vindicated. This was the precise meaning of the statement issued in 1937 by the Institute of Mathematical Statistics.(4) Without a word of explanation, Diaconis appears to disparage this entirely correct statement, construing it as an example of good statisticians led astray by the "special problems" of ESP research (p.133).

Misleading and incorrect statements also appear in the introduction to this section. Diaconis correctly points out that there is increasing interest in the use of so-called "free-response" targets such as drawings, art reproductions, or natural settings (although experiments of this sort had already been carried out in

the 30's and even before). But this is entirely separate from the matter of feedback, which I have already discussed. Technically correct methods of analysis for certain classes of free-response data have existed for decades, and significant advances have appeared from time to time as research workers gained familiarity with the properties of the methods. Our chapter in the Handbook provides an integrated treatment of this entire subject. Thus, Diaconis' statement that "the statistical tools for evaluating the outcome of more complex experiments are not available" (p. 134) must be sharply qualified; the only case to which it literally applies is the case of closed-deck procedures with trial-by-trial feedback, whether with forced-choice or free-response target material. As I have indicated, this case is rare in practice.(5)

"Informal Design and Evaluation" (p. 131)

Diaconis' most serious misrepresentations take shape primarily in this and the following two sections, although the process begins in this general introduction and continues on through the beginning of the section entitled "Statisticians and ESP." He offers the reader a series of sweeping suggestions about typical characteristics of experimental research in parapsychology: Thus it is said to be difficult to design a suitable experiment to test for ESP. Experimental designs are claimed often to be "non-standard," in ways that make statistical analysis difficult or impossible. Typically this may result from experimentation which is informal and careless, allowing abundant opportunities for re-definition of what is to count as success, and for subject cheating.

No effort is made to document these generalizations, and their extreme inaccuracy will quickly become apparent to anyone who takes the trouble to examine the literature.(6) Rather, Diaconis attempts merely to render them superficially plausible by providing a few non-experimental examples of the sort of thing he has in mind, and alleging that these are typical of what goes on in research. The weight of the discussion is in fact carried by three such examples, all involving "star psychics" performing under uncontrolled conditions. The three individuals involved are Bill Delmore (B.D.), Ted Serios, and Uri Geller. I will speak only to the B.D. case because it is the one with which I am most intimately familiar: I arranged the informal demonstration at Harvard which Diaconis witnessed, and which he discusses at length; and I participated in the design, conduct, and analysis of all of the published experiments, which Disconis did not witness and which he does not discuss at all.

Let me first point out that although "star" performers of the sort that provoke Diaconis' doubts have appeared occasionally in the historical development of the field, they are not the sole nor even the principal source of evidence for psi phenomena. Particularly in recent decades there has been increasing emphasis on process-oriented studies with unselected subjects, or subjects selected through performance on pre-experimental screening tasks of various kinds. These studies are typically much like "standard" psychological experiments, simple and routine in structure and relatively

immune to the sorts of error Diaconis discusses. Their main objective is to generate knowledge of the characteristics and conditions of occurrence of psi phenomena, rather than simply more evidence that the phenomena occur.

Secondly, the published reports of work with the "star" subjects are generally very careful about grading testing situations in terms of quality of controls. This is again particularly characteristic of reports published in the professional journals. Our work with B.D. is one example. We observed many of B.D.'s spontaneous performances, and witnessed a large number of seemingly quite spectacular events, some of which go considerably beyond the quality of the examples Diaconis cites. In our first article we briefly described a few such events, but we did so in a context which makes absolutely clear that these observations are strictly separate from the formal testing which followed, and of necessarily lesser value.(7) Similarly, in a later paper we described results obtained following relaxation of conditions in a fully controlled task at which B.D. had already succeeded. This was done deliberately in order to explore the outer limits of B.D.'s performance capability. The changes of conditions and their implications for evaluation of the corresponding results are discussed at length in the paper.(8)

For reasons which are nowhere made explicit, Diaconis is entirely confident that none of these distinctions is meaningful, and that the conditions of the formal experiments with B.D. did not differ significantly from the conditions of an entirely informal demonstration that he witnessed at Harvard. He gives no information at all about these experiments, however, which presumably makes it somewhat difficult for most readers to judge the force of the distinctions they are being urged to ignore. Therefore I shall supply some further information.

First let me point out that the central purpose of the 1972 Harvard demonstration was to attempt to secure funding for later formal experiments through the Hodgson Fund, a fund established there for psychical research during the time of William James and still administered by the Department of Psychology (and Social Relations). Absolutely no effort was made to control B.D.'s performance. The intent was to allow him to function in an entirely unrestricted way, in hopes of having enough things spontaneously happen to persuade open-minded observers that it would be appropriate to use the fund to support a series of formal experimental investigations. The demonstration was emphatically in no sense an experiment, nor a collection of experiments. The events which took place are certainly not referred to in any published report; indeed the occasion was so informal that I made no attempt even to take notes. Despite its being clearly labeled as an informal demonstration, Diaconis repeatedly refers even to the individual events as "experiments," without ever acknowledging to the reader that this terminological abuse is entirely his own invention.

Diaconis discusses at considerable length card stunts he witnessed B.D. perform, using them to illustrate problems of multiple

endpoints and subject cheating which he alleges are endemic in ESP research. I do not need or wish to be drawn into a defense of B.D.'s Harvard performance, but I would like to point out that in several respects Diaconis' conclusions even about that specific occasion go beyond his observations. In particular, he did not observe cheating per se, but only certain events which from his perspective as magician and skeptic he felt compelled to interpret as cheating. He even describes as matters of observational fact things which he cannot possibly know with complete certainty--for example, "...B.D. secretly counted the number of cards..." (p. 132), and so on.

Diaconis' apparently absolute confidence in his wholly negative appraisal of B.D.'s Harvard performance therefore seems to me unwarranted. But this is incidental to the main point: Ultimately what did or did not take place at Harvard is of extremely limited significance, and will probably always remain a matter of personal conjecture. A substantial part of the motivation for doing formal experiments with B.D. was in fact precisely to eliminate the kinds of uncertainties Diaconis describes, and thus to take these issues beyond the realm of subjective opinion. Let me therefore briefly describe some of the experimental work with B.D.

Prior to the Harvard demonstration we had already worked with B.D. for several weeks, and had accumulated substantial evidence of his ability to perform under controlled conditions.(7) Diaconis refers to this paper, but makes no mention of any of the reported results. The main series of tests involved 5,337 guesses on a 4-choice ESP test machine with paper tape recording and target selection by a quantum mechanical randomization device.(9) B.D.'s overall scoring rate for the series was 28.7%, $p < 10^{-9}$. He also produced significant results in every one of six different test situations, including not only a card-guessing test but a variety of electronically and mechanically controlled test devices.

The work subsequently supported by the Hodgson Fund (10) was much more extensive and focused primarily on two kinds of tasks using playing cards. In the first (11), B.D. attempted to identify individual cards randomly selected with replacement from a pool of ten decks. The experimenter (who had no normal knowledge of the target) enclosed the card in an opaque black folder and held the folder up for B.D. to observe across the top of an office desk. B.D. was not permitted to touch the folders, and the cards were inserted so that their backs faced him. Working at a rate of about one run per session, 46 runs of 52 calls were completed in this way. Under these conditions B.D. scored exact hits at a rate three times chance expectation, and there were several other highly significant results involving correct responses. However, the main purpose of the study was not simply to generate more evidence of ESP, but to attempt to gain some insight into the mechanisms underlying the performance. To this end, we made an intensive study of B.D.'s errors. These proved to be systematic, and systematically similar to the errors he made in a separate task requiring visual identification of briefly exposed color slides of playing cards. We tentatively interpreted this result as suggesting a mechanism in which the ESP

information is encoded in the form of fleeting and fragmentary visual imagery, the systematic errors arising at a secondary stage when B.D. attempted to identify his imagery.

The second series (12) involved variations on a basic task in which for each run B.D. shuffled a"call" deck to match a previously randomized and concealed target deck. In 55 runs carried out under these conditions he again produced an enormous excess of exact hits, but there was no evidence of systematic erroneous responses, suggesting that different mechanisms may have been operating.

For details, the original reports should be consulted. Enough has been said, however, to give some concrete indication of the nature of the experiments that were actually carried out. In his abrupt and sweeping dismissal of all of this experimental work involving B.D., Diaconis seems simultaneously to embrace each of two mutually inconsistent positions on the relationship between the conditions of the Harvard demonstration and those of the formal experiments: On the one hand, he acknowledges--as I believe virtually all readers of these papers would--that the conditions of the published experiments as described are satisfactory. Thus on p. 131 he states that "each of the studies referred to above describes experimental conditions beyond reproach." He goes on immediately to say, however, that "My own observation suggests that the conditions were not in control." That is, he appears to suggest that the actual conditions of the experiments differed from their published descriptions. No warrant is given for this astonishing suggestion, which is quite false and repeated with greatly expanded generality of reference in other places in the paper. Indeed, in his "Conclusions" (p. 135) and "Summary" (p. 131) he comes very close to intimating that he was present <u>during</u> the formal experiments themselves.

On the other hand he states on p. 133 that "the similarity of the descriptions of the controlled experiments with B.D. and Serios to the sessions I witnessed convinces me that all paranormal claims involving these two performers should be completely discounted." Surely this statement also requires further explanation. For B.D. the conditions were extremely <u>dissimilar</u>, in particular with respect to the two sources of error Diaconis stresses. The trial-by-trail outcomes that tell against the null hypothesis are unambiguously specified in advance, and the overall statistical tests for the occurrence of psi effects are appropriate and simple.(13) Opportunities for cheating or sensory leakage appear likewise to be reduced to the vanishing point, with the exception of the few deliberately weakened (and so labeled) series mentioned above.

If Diaconis in fact perceives serious weaknesses in the experimental condition, he has yet to explain what they are. The sweeping negative judgement he pronounces with such complete confidence seems to rest ultimately on personal grounds. At least what he has so far written depends <u>essentially</u> upon a systematic unwillingness to confront the details of the published experimental investigations, and upon a systematic effort to ignore, and to obliterate for the reader, the vital distinctions between these

investigations and the entirely informal demonstration he personally witnessed.

But B.D. is only one person, and our investigations only one small fragment of a literature now amounting to well over a thousand published experimental studies. Much more serious is Diaconis' generalization of his tactics to this literature as a whole. In his "Conclusions" and "Summary" all the errors and misrepresentations I have noted in this reply are collected and repeated in greatly generalized form virtually uninhibited by any indication of effort at fairness or objectivity. He makes what sounds like an impressive claim about the extent of his familiarity with ongoing research, but on the basis of his similar claims about the work with B.D. I think it is fair to wonder aloud whether he has in fact participated in anything that could appropriately be called a parapsychological experiment.(14) I think he is ethically obligated either to substantiate in detail his claim ("I have been able to have direct experience with more than a dozen experiments and detailed second-hand knowledge about perhaps 20 more"-p. 136), or to withdraw it. Similarly, the astonishing collection of inaccurate and misleading statements that comprise his "Conclusions" in particular (p. 135) approaches scholarly irresponsibility and at the least falls far short of what one would hope to find in the pages of Science.

Surely no methodologically sophisticated parapsychologist would claim that the literature is unblemished by examples of faulty design and analysis. Furthermore, and again just as in any other field, there is always a stock of unsolved problems at hand to which creative statisticians can turn constructive attention--our Handbook chapter for example identifies a number of these. But Diaconis nowhere comes directly and honestly to grips with the actual literature of the field. In his sweeping caricature of all of modern parapsychology research in the image of a few extremely atypical examples, he does grave disservice not only to parapsychology but to his fellow scientists and ultimately to the scientific process itself. It is frustrating and saddening to see such gross misinformation being distributed to the scientific community by a fellow scientist, using one of the world's most respected organs of scientific communication. I hope that at least some readers will recognize from this reply that there is quite another side to this story, and be moved to look into the subject themselves.

NOTES AND REFERENCES

1. Vol. 201, July 14, 1978, 131-136.

2. C. Tart, Learning to Use ESP (University of Chicago Press, 1976), chapters one and two (Diaconis' reference 31). A related and more pertinent topic which Diaconis might have discussed but does not is the question whether at least some apparent evidence of ESP success, particularly in extended open-deck series with trial-by-trial feedback, might reflect the existence of powerful pattern-recognition capabilities operating on low-level

non-randomness in finite sequences of "randomly" selected targets. Although this criticism also appears to me unlikely to erode the evidence for psi phenomena in more than trivial degree, it at least has the virtue of addressing the existing literature, unlike the criticism Diaconis chooses to make.

3. The "standard" method, first institutionalized in the work of J.B. Rhine's group at Duke in the 1930's, uses the normal approximation to exact cumulative binomial probabilities. This method and the adjustments arising in the closed-deck situation are discussed in detail in our Handbook chapter on statistical methods in parapsychology.

4. "... assuming that the experiments have been properly performed, the statistical analysis is essentially valid. If the Rhine investigation is to be fairly attacked it must be on other than mathematical grounds." (B.H. Camp, reprinted in JP, 1, 305 (1937).

5. The example Diaconis uses here is drawn from the SRI "remote viewing" work (H. Puthoff and R. Targ, Proc. IEEE, 64, 329 (1976)). This is in fact the only example discussed in the entire paper that would generally be recognized as constituting an experiment; its procedural and statistical weaknesses (although in my opinion less severe than Diaconis' description suggests) are well known, and not characteristic of the broader free-response literature. In fact many of these problems are deplored in the review by a parapsychologist to which Diaconis himself refers (D. Stokes, JASPR, 71, 437 (1977)).

   The other concrete suggestion Diaconis makes (p. 135) is that his new analysis methods may be used to salvage procedurally defective closed-deck experiments in which one card is guessed at a time and feedback is suspected to have occurred inadvertently, for example through sensory leakage from the experimenter. This is an entirely superfluous suggestion since very few (if indeed any) such experiments have been published in full-length form in a professional journal since the very earliest days of the research.

6. By "the literature" I mean in particular full-length reports of original research, such as appear in the professional journals of the field. These include the Journal of the American Society for Psychical Research (JASPR), Journal of Parapsychology (JP), Journal of the Society for Psychical Research (JSPR), and European Journal of Parapsychology (EJP). A useful annotated guide to the parapsychology literature is R. White and L. Dale, Parapsychology--Sources of Information, Metuchen, N.J.: Scarecrow Press, 1973. The recently published Handbook is among the best secondary sources, with excellent reviews of a number of central topical areas.

7. E.F. Kelly and B.K. Kanthamani, JP, 36, 185 (1972).

8. H. Kanthamani and E.F. Kelly, JP, 38, 355 (1974).

9. H. Schmidt, J. App. Physics, 41, 462 (1970).

10. According to Diaconis, the July 13, 1973 issue of Science reported that B.D. had been given a grant from Harvard "to explore the nature of his own psychic ability." Science notwithstanding, the grant was made to Drs. Irvin L. Child and Sidney Blatt of Yale University, and myself, for clinical and experimental studies with B.D. to be conducted at Yale and the Institute for Parapsychology in Durham, North Carolina.

11. E.F. Kelly, H. Kanthamani, I.L. Child, and F.W. Young, JASPR, 69, 1 (1975).

12. H. Kanthamani and E.F. Kelly, JP, 39, 206 (1975).

13. On p. 133 ("Statisticians and ESP") Diaconis implies that the simple chance models used to evaluate the occurrence of psi effects in these experiments were inappropriate. That suggestion is false. The justification of our basic methods, which are also common to the great majority of published experiments, is outlined in detail in the Handbook chapter and has been available since the late 1930's. He then goes on to give another untypical example of the misuse of statistics, namely the Zenith Radio experiments. It is true that the results of this very early series were at first incorrectly analyzed, along with some other results of group tests in which multiple respondents guessed cards from a single (open or closed) deck. However, several points of clarification should be made: The vast majority of published experiments use single respondents, in which case the patterning of responses has either no effect (open deck) or a trivial effect (closed deck). The core of the multiple-respondent problem lies in the spurious multiplication of the number of trials when the standard methods are applied as though each respondent has his own target order. (This analysis would still be correct, as Diaconis indicates, if the responses were independent, but of course this cannot safely be assumed.) The problem of multiple respondents was solved in the early 40's by a statistician openly supportive of parapsychology research, particularly in its statistical aspects (T. Greville, Ann. Math. Stat., 15, 432 (1944)), in direct response to the needs of research workers. Since that time the problems involved in group testing have been clearly understood by all knowledgeable researchers, and handled appropriately either by using Greville's method or a simplified method using majority votes of responses on each trial, or through generation of separate targets for each respondent.

14. In fact, it seems to me a compelling inference from the following statement that he has not: "The confusing and erratic conditions I have described are typical of every test of paranormal phenomena I have witnessed." (p. 133, italics his)

REJOINDER TO EDWARD F. KELLY

PERSI DIACONIS

I will discuss what I consider to be the three main points made by Kelly. These are:

Point 1. My results on feedback experiments are irrelevant to ESP research; and, anyway, the essence of my findings are well understood by parapsychologists.

Point 2. My description of what went on during an informal session with B.D. casts no shadow on Kelly's experiments with B.D.

Point 3. My article does not survey the statistical problems in ESP research. Rather, I draw conclusions based on a handful of demonstrations which are atypical, neglecting a huge volume of studies beyond reproach.

Response to point one--Feedback. In Feedback experiments the guessing subject is told if the last guess was right or wrong, often by being shown the card or target after each guess. My main point was that because of dependence (correlation) between targets, knowing the last target gives information about the next target. This complicates the analysis. Sometimes dependence is easy to understand--as with closed decks or sampling without replacement. In other cases--as when computer generated pseudo random numbers are used or when the subjects in an experiment like Targ and Puthoff's remote viewing set-up are given feedback--dependence is harder to explain in simple terms but none-the-less is a real problem.

Dependence abounds in complicated experiments; and, despite Kelly's claims, nobody knows how to adjust for the partial information made available by feedback. Here are some examples: there are many follow-up studies, underway or recently reported, using Targ and Puthoff's protocol. Not long ago, Goldman, Stein, and Weiner showed that Tart had been using a faulty random number generator in a feedback experiment. Feedback introduced the possibility that subjects could notice the patterns in the guessing sequence. This led Tart to redo the experiment with better random numbers, and the new results were non-significant.

My study of the effect of feedback in simple cases shows that one has to be very careful in handling feedback experiments--if the effect of feedback is not taken into consideration, this can make ordinary results look extraordinary. Although I dealt only with simple cases, there is no reason to think that feedback problems go away in more complex problems discussed above.

The current efforts of Gatlin, Targ and Puthoff, and Tart to measure the effect of feedback in their experiments involve a lot of thought and computer simulation. This ongoing search for a solution to the problem of feedback contradicts Kelly's claim that

there are available technically correct methods of analyzing modern complex experiments.

In summary on point 1: Kelly failed to understand that the cases I study are examples of problems that arise when feedback is combined with dependence. His reply shows that he doesn't understand the problems of feedback.

Point 2 -- B.D. In my article I wrote:

> "Rejecting the claims of a psychic who has been caught cheating raises thorny scientific problems. I am sure that B.D. used sleight of hand several times during the performance I witnessed." Yet, as one of the observers remarked, "the people who introduced B.D. never said he didn't do card tricks; they just claimed he had extraordinary powers on occasion."

In his response, Kelly makes essentially this same point (though he gives no hint of admitting that B.D. ever used sleight of hand or any of the other devices I reported). I think that ESP experiments done by known sleight of hand users must include, as part of the protocol, magicians skilled at detecting sleight of hand. None of Kelly's experiments with B.D. report any such explicit precautions. Indeed, as far as I know, Kelly and his coworkers were not aware that they were working with a sleight of hand performer, and may not admit that B.D. ever used such methods. This leads to the following dilemma: consider the informal demonstrations of card tricks that I reported. Logic demands one of two alternatives.

. Kelly was unaware that B.D. was using sleight of hand,

or

. Kelly was aware that B.D. was using sleight of hand but chose not to say anything (then or in any of his articles since then).

Neither possibility makes me put much faith in Kelly's research with B.D.

In summary on point 2: Kelly has reported experiments performed on a subject who knows and uses sleight of hand. Kelly's experiments do not incorporate an expert magician as part of their protocol and hence are under a cloud which is dark enough to make me regard them as another amusing curiosity, not scientific evidence of any kind.

Point 3--not a survey. One approach to evaluation of the ESP literature that allows straightforward inferences is this: take a random sample of published studies and review them. Such an undertaking is well worth doing. One complication, made explicit by many examples in my article, is that published studies may omit crucial details. I agree with Kelly; the experiments I have had contact with are not a random sample from the many hundreds of reported studies. I tend to get called in on more sensational studies such as trials done by experienced researchers who have had an impressive success.

I base my conclusions on the following inductive argument: I

have had contact with some of the most skillful researchers in the field. These include Ed Kelly--author of a lengthy chapter on use of statistics in ESP research; Julie Eisenbud--regarded as a kind of grand old man of the field by parapsychologists I know; Charles Tart--former national president of a large parapsychology group, author of many books and studies; Hal Puthoff and Russell Targ--authors of government funded studies; the most well-funded parapsychologists of all times. Surely these researchers should know how to avoid the pitfalls and use acceptable methodology. My contact with these researchers has not brought to light any evidence of paranormal phenomena. Instead, I find sincere, dedicated researchers doing sloppy experiments.

Perhaps the best summary of my conclusions can be found in Kelly's reply and my response: here it is 1979, and a serious ESP researcher is defending experiments done with a performer, observed to use sleight of hand, as evidence for the paranormal.

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QUOTEWORTHY

I inherited my mother's ability to send and receive communications. So did one of my sisters. In tests before representatives of the University of California, she was able, seven times out of ten, to receive messages sent to her telepathically. My mother, who lived to be more than ninety-six years of age, was in poor health the last years of her life. During these years I often wished to summon my sister. On such occasions, I never had to write or telegraph to her. Instead, I sent her messages telepathically, and each time she arrived in Santa Rosa, California, where I lived on the next train.

--Luther Burbank (1923)

If our personality survives, then it is strictly logical and scientific to assume that it retains memory, intellect, and other faculties and knowledge that we acquire on this earth. Therefore, if personality exists after what we call death, it's reasonable to conclude that those who leave this earth would like to communicate with those they have left here.

...I am inclined to believe that our personality hereafter will be able to affect matter. If this reasoning be correct, then if we can evolve an instrument so delicate as to be affected, or moved, or manipulated...by our personality as it survives in the next life, such an instrument, when made available, out to record something.

...If we ever do succeed in establishing communication with personalities which have left this present life, it certainly won't be through any of the childish contraptions which seem so silly to the **scientist.**

--Thomas Alva Edison

It is the customary fate of new truths to begin as heresies and to end as superstitions.

--T.H. Huxley

True science teaches, above all, to doubt and to be ignorant.

--Miguel de Unamuno

The many instances of forged miracles and prophecies and supernatural events, which, in all ages, have either been detected by contrary evidence, or which detect themselves by their absurdity, prove sufficiently the strong propensity of mankind to the extraordinary and marvelous, and ought reasonably to beget a suspicion against all relations of this kind.

-- David Hume (1748)

Never take anything for granted.

--Benjamin Disraeli

It's better to know nothing than to know what ain't so.

--Josh Billing

Every science has been an outcast.

--Robert Ingersoll

Every great advance in science has issued from a new audacity of imagination.

-- John Dewey

As nature preserves a fixed and immutable order, it must clearly follow that miracles are only intelligible as a relation to human opinions, and merely mean events of which the natural cause cannot be explained by a reference to any ordinary occurrence, either by us, or at any rate, by the writer and narrator of the miracle.

--Benedict Spinoza

Those who honestly mean to be true contradict themselves more rarely than those who try to be consistent.

-- O.W. Holmes

It was not that my employer objected to my experiments--only to experiments with gas engine. I can still hear him say: "Electricity yes, that's the coming thing. But gas--no."....The Edison Company offered me the general superintendency of the company only on condition that I would give up my gas engine and devote myself to something really useful.

-- Henry Ford.

Error of opinion may be tolerated where reason is left free to combat it.

--Thomas Jefferson

To know that we know what we know, and that we do not know what we do not know, that is true knowledge.

--H.D. Thoreau

We do not know one millionth of one percent about anything.

--Thomas Alva Edison

The demonstration that no possible combination of known substances, known forms of machinery and known forms of force, can be united in a practical machine by which man shall fly long distances through the air, seems to the writer as complete as it is possible for the demonstration of any physical fact to be.

-- Simon Newcomb

The abolishment of pain in surgery is a chimera. It is absurd to go on seeking it today. "Knife" and "pain" are two words in surgery that must forever be associated in the consciousness of the patient. To this compulsory combination we shall have to adjust ourselves.

-- Alfred Velpeau (1839)

There cannot always be fresh field of conquest by the knife; there must be portions of the human frame that will ever remain sacred from its intrusions, at least in the surgeon's hands. That we have alread, if not quite, reached these final limits, there can be little question. The abdomen, the chest, and the brain will be forever shut from the intrustion of the wise and humane surgeon.

-- Sir John Erichsen (1873)

The bow is a simple weapon, firearms are very complicated things which get out of order in many ways...a very heavy weapon and tires out soldiers on the march. Whereas also a bowman can let off six aimed shots a minute, a musketeer can discharge but one in two minutes.

--Colonel Sir John Smuth (1591)

As far as sinking a ship with a bomb is concerned, you just can't do it.

--Franklin D. Roosevelt,
Assistant Secretary of the Navy (1922)

Humility is not a state of mind conducive to the advancement of learning.

--Sir Peter Medawar

Science is always wrong. It never solves a problem without creating ten more.

--George Bernard Shaw

Truth is the object of philosophy, but not always of philosophers.

--Churton Collins

Truth uttered before its time is always dangerous.

--Mencius

Nothing is so firmly believed as what we least know.

--Montaigne

THIRTY YEARS AFTER KENNETH ARNOLD: THE SITUATION REGARDING UFOS

PIERRE GUÉRIN

Authorized Translation by Ron Westrum

This article makes no pretense of originality. It does not add any new element and it frankly admits owing everything to excellent works already published by others. The only merit claimed by its author is that it presents a synthesis of these works, classifying facts and ideas; presenting them in such a way, that at the end of the confrontation a certain number of unsatisfactory hypotheses eliminate themselves, leading to the single conclusion which, in the actual state of matters, takes account of all the known factors.

To be sure, the "UFO's" to be considered below are only those which remain after one has relentlessly eliminated confusions with known objects or phenomena, hoaxes, and the rare cases of pathological hallucination. This "irreducible remainder," representing 1% to 15% of all the cases reported (depending on which sources are used), comes to (at the very least) some tens of thousands in this century, not to mention those of other previous centuries.

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If one is to judge the state of UFOlogy by the writings of its modern practitioners, it would appear that it is now at an impasse; the best UFOlogical journals are a faithful reflection of this situation. For instance, in the UFOlogical literature written in French, it is more and more common to find, following one another:

1) an article by this or that physicist predicting---according to the usual interpretation of UFO's as extraterrestrial space probes come to study us---the explanation of the physical aspects of the phenomenon which are already reducible to our present state of knowledge in terms of magnetohydrodynamics or particle physics. This, in the expectation no doubt that new progress in theoretical physics will reveal to us the last secrets of the propulsion of UFO's.

2) a polemical piece by this or that UFOlogist convinced of the physical reality of the phenomenon, but emphasizing the insufficiencies and improbabilities of the classical extra-terrestrial explanation, and stressing the "paranormal" character of the UFO phenomenon and its relation to its sociocultural context. The UFO phenomenon, in turn, is blithely interpreted as parapsychological materializations of flying objects and humanoid entities which represent our own unconscious mental forces at each stage of our development.

3) a critical study of certain suspicious cases of UFO phenomena, presented by a UFOlogist who has not in the least entered the field to study serious cases, and whose conclusion, extended in an unwarranted manner to the entire subject, is that all UFO witnesses could well have "dreamed" their observations under the influence of some natural

stimulus, without any intervention being necessary from unidentifiable artifacts of non-human origin. Which leads in the end to a denial of the existence of UFO's, which unites more or less the theses of the late Donald Menzel to those of Philip Klass and the so-called "rationalist" members of the intelligentsia.

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None of the proponents of these contradictory interpretations seem ready to make the least compromise. The positions are fixed, the discussion becomes one between deaf persons, anathemas and excommunications start to fly, although each defends virtuously his own views....

Such a situation is not unique to UFOlogy; similar situations have existed throughout the history of human thought, and they produce themselves every time that the elaboration and acceptance of new concepts (which are expected by themselves to take account of the whole of the observed facts) have been unable to keep abreast of the accumulation of these facts. The global interpretation of the facts, being unable to rest upon old, insufficient, or erroneous postulates, thus becomes impossible. But the human mind resists just as much giving up the search for explanations for facts submitted to its wisdom, as it does putting received ideas to the question. It is thus driven to seek reductionist explanations at whatever cost, on the basis of what is already known. And of course this is only possible at the price of a rejection of a part of the observed facts, those which do not "accord" with established principles. The "explanation" produced will differ according to which group of observations is rejected. From which one might say that the closure of these antagonistic models makes them unworthy of the name, since the nature of a true model is to stick to the facts, or more exactly with all the available observational data, which verify it.

As I am going to show in examining in a critical manner the UFOlogical "models" in vogue, such a situation is exactly that which exists today in the world of UFOlogy. This stems from the fact that the huge panorama of UFO manifestations, complex and full of pitfalls, begins to become well known after 30 years of accumulation of data by the investigators (whose work is never sufficiently praised); yet nonetheless our philosophical and scientific mentality remains outdistanced by the phenomenon and its implications.

THE INADEQUACY OF THE CLASSICAL EXTRA-TERRESTRIAL "MODEL" (UFO'S AS SPACESHIPS)

This "model"---which was the first to be suggested by UFOlogists and which is still retained by many of them---considers UFO's to be sophisticated extraterrestrial vehicles,coming to earth from interstellar space, to land their humanoids, who have come in quest of specimens of minerals, vegetables, animals and even human beings, for purposes of study and analysis. This model not only sins by its anthropocentrism---founding too much on parallels with our own planetary explorations at the dawn of our space age---but it is also too much in disagreement with too many well established facts today to be sustainable

in this form.

To begin with, the immensity of interstellar distances is such[1] that at a velocity necessarily inferior to that of light (the lovers of tachyon-spacecraft to the contrary), it would require, for a vessel to come from another inhabited planetary system, tens, hundreds, perhaps thousands of years to cross the distance which separates the two systems. And this---no matter what the technology of our visitors is---to the degree that, to go from one point to another, they make as we do a displacement along a trajectory in space-time of four dimensions x, y, z, ict. For, in this space-time, the velocity of light is a limiting speed which cannot be reached (let alone exceeded) without disposing of an infinite amount of energy (a result which is well established by relativistic physics, and which has very little chance of being overthrown by discoveries which will transcend it perhaps, but will not disconfirm it). Thus, in making the hypothesis---admitted today by all astrophysicists---of a plurality of inhabited solar systems, one could reasonably expect (taking into account the number of such systems) extra-terrestrial visits on earth, but these visits would be necessarily well spaced in time on the average---a visit every 5000 years, for example---and could only be accomplished by gigantic cosmic vessels heavily equipped with supplies and fuel for such long voyages. UFO's, which are very small (some meters in diameter on the average) and whose frequency of appearance is counted in days---or hours during "flaps"---have evidently little in common with such spaceships. Let us suppose, to anticipate an objection, that they are launched from great mother-ships cruising throughout the solar system; it would still have to be explained why they are seen so much and so often. Whether one likes it or not, the trip (in the classical sense of continuous displacement) toward the stars is of a difficulty and duration of a totally different order from that of exploration of the solar system from the earth. It is not even certain that, in this manner, the enterprise can be realistically considered, for beyond a certain distance it implies a one-way trip.

A second argument exists, which differs from the interpretation of UFOs as classic extra-terrestrial probes, similar to our own. The analysis of the evolution of forms and behaviors of UFO's in the course of recent historical time reveals that these forms and behaviors model themselves, in part[2] on certain science fiction tales previous to the observations, but unknown to the witnesses, as if the UFO's exemplified the human technological dream; this thesis has been magistrally propounded by Bertrand Méheust.[3] This would imply---if the witnesses' visions of UFO's are not hallucinations---that UFO's are not spatial probes sent to seek us out and study us; for the Intelligence, whatever it is, which has introduced them into our environment knows us so well, and is so far master of the laws of matter, that it can fashion their appearance as a function of certain of our thoughts! To be sure, I do not deny---I was even the first to suggest[4]---a possible hallucinatory factor, artificially induced by the UFO phenomenon in the close-encounter witness, which would better take account of certain facts well known to investigators. Still, hallucination does not explain everything: the American "air-ship" was not only observed close-up by isolated and possibly hallucinatory witnesses but also at a respectable distance, high in the sky, by numerous witnesses more astonished than

fearful and apparently in possession of all their faculties who, having never read Jules Verne, nonetheless described it as a "retro" flying machine in the most pure style of the Vernian fantasy of the time.[5] The inhabitants of a far-off planet, interested in finding the earth and studying its actual inhabitants, would hardly have equipped themselves, for crossing interstellar spaces, with a design as little functional as that of the Albatross of Robur the Conqueror!

In the same line of thinking, the alleged behavior of modern extraterrestrial humanoids aping our astronauts in picking up plants and pebbles, without taking any precautions---or even worse that they are collectors of fertilizer for the planet Mars (sic) or yet prophets of Apocalypses (I will pass over this)---suggests a "movie" in which we are implicated, and has nothing to do with what would be the conduct of real scientific explorers discovering the earth. If the mental level of such explorers radically exceeds our own (a very probable hypothesis, as we shall indicate below), it is extremely likely that we would comprehend nothing of their scientific activities in studying the terrestrial environment. However, at the very least we would expect such activity to have none of the deceptive or grotesque aspects which are so frequently the case.

The UFO phenomenon is not therefore what it appears to be at first sight---even if an extraterrestrial intelligence is controlling it. Whether one likes it or not, it is infinitely more difficult to interpret than the first UFOlogists---with a few exceptions---thought it was.

REFUTATION OF THE PARAPSYCHOLOGICAL MODEL

To deal with these various objections, a school of thought comprising several amateurs both noisy and sure of themselves, but whose intellectual rigor is not always their strong point, has recently published and defended[6] the so-called "paranormal" model, which assimilates UFO's to materializations of human parapsychological origin, brought about by collective or individual distress, and modelling itself precisely on the forms which translate, in each historical period, the fears and aspirations of humanity. My feeling is that if the "model" effectively explains certain facts which the hypothesis of extra-terrestrial space probes does not, on the other hand it is in total contradiction with other facts, to the point where one can hardly enumerate the improbabilities it involves. Let us only cite two, which are substantial enough:

To begin with, are there examples in the parapsychological literature of materializations caused by the mind's effect upon matter, which resemble UFO's to any degree? The answer is no. An amateur parapsychologist who hardly deserves the title has recently claimed to see strong analogies between "ectoplasm" and UFO's and their alleged occupants. I will not pronounce, here, any opinion of the problem of the physical reality of ectoplasms, about which the least that can be said is that it is much more controversial than the existence of UFO's. I will allow myself only to remark that, on many points, the ectoplasms, according to the assurances of those who have put them in

evidence and submitted them to experimentation, differ radically from UFO's in their physical effects. In particular, the ectoplasmic substance, according to the repeated and consistent experiments of Dr. Osty working with the medium Rudy Schneider,[7] absorb infra-red rays---causing the innumerable cases of a sensation of coldness felt during mediumistic experiments---whereas UFO's are known for their intense heating effects, even dessicating a damp soil and sterilizing it of humus to the point where nothing can be grown in it for weeks, just as if it had been subjected to Foucault currents like those caused in microwave ovens. To identify, even remotely, UFO's with ectoplasms is to pass off too easily the evidence for the former which, once materialized in our environment, show themselves almost always to be structured machines when they are seen in good enough conditions so as not to be confounded with the fluctuating luminous "halo" which often surrounds them, or with the beams of light, truncated or not, that they emit, or even with the "balls of fire" or other small exploratory satellites which can come out of them. One has to have an incredible degree of blind faith to find an analogy between the ectoplasmic forms which issued from mediums during the last century and structured flying objects which return radar echoes, emit an intense light or a buzzing, produce electromagnetic effects (upon radios, motors, etc.) and traces on the ground, and finally move around as if they were piloted by an Intelligence.

If UFO's were really parapsychical manifestations, we would find ourselves in the presence of a totally new and original phenomenon which has no equivalent in the entire dossier of mediumistic effects. UFO's have, to be sure, a paranormal side, and there exist, as I and others have indicated before,[8] profound analogies between all extraordinary paranormal phenomena.

But these analogies are not such as to make UFO's simply a variant in the panoply of effects of the human mind on matter. They are, in fact, much more than that.

Let us nonetheless suppose that we are wrong, and try to see if the "model" defended by the parapsychologically-inclined UFOlogists is verified by its consequences, which is the basic requirement of good old scientific method.[9] One would have to show, to be specific, a temporal and geographical correlation without exception between the frequency of UFO's and the psycho-social factors which bring about apparitions, as well as a correlation between the form of UFO's and the technical and cultural assumptions of the people who see them. However, there is no such correlation. As we know, UFO's occur in waves localized in time and space (usually these waves affect a limited geographical zone, two or three times as large as France, during some weeks or some months at the most); but these waves do not correlate, as Pierre Vieroudy[10] claims they do, with the worries of populations or the distress of individual witnesses. A recent critical analysis by Nicolas Gresloul[11] has established this in a very convincing fashion.[12] Furthermore, the types of UFO's which characterize a given wave, or a series of waves, can reproduce certain forms imagined by novelists in countries very distance from those which are affected by the wave. Thus, it was not in France, country of Jules Verne, nor even in Europe, that the UFOlogical version of the Albatross was seen in 1896-1897, but

in the United States, and this during a period limited to several months. It is obvious that the good citizens of the United States who were visited by the "air-ship" did not live and breathe in a more Vernian ambiance than the French did! No more do the Papuans who now receive visits from modern UFO's live in an atmosphere created by Western science-fiction and the first successes of space exploration toward the end of the 20th century.

To sum up, UFO's, which differ radically from the alleged mediumistic manifestations, can very well be inspired, in each historical period, by forms drawn from human imagination. This does not imply, however, that there is any relation between their frequency of appearance or their geographical localization at the time of the wave and the mental currents which activate the individuals or populations which observe them, and which are wrongly supposed to play the role of creator or initiator of the phenomenon. Everything takes place as if the psyche which creates and directs the UFO's---although partly inspired by our own---is nonetheless distinct from ours.

REFUTATION OF THE SPONTANEOUS WAKING HALLUCINATION "MODEL"

Let us examine finally the spontaneous waking hallucination "model" (wrongly called the "waking dream" by certain authors[13]) which it is fashionable today to invoke in "rationalist" circles whenever the richness of detail and strangeness of the testimony rules out a banal confusion with a badly interpreted natural object or phenomenon, and the witness is recognized to be acting in good faith. One could hardly wish greater success in rebutting this "model," than have the by now well-known and generally accepted results of the statistical studies of Claude Poher.[14] Let us recall the most important of these results: the number of UFO's seen in the sky increases with the angular height of vision above the horizon, following the so-called "Bouger's law" (which expresses, in astronomical observation, the progressive growth of the atmospheric transparency as the thickness of the atmosphere declines, so that one can see progressively higher in the sky); further, the overall annual number of UFO's seen in the sky is proportional to the amount of sun received by the region considered (that is to say its atmospheric transparency). These results indicate that at the origin of the UFO observations, there is indeed a real physical object or phenomenon, the observation of this phenomenon or this object obeying the optical law of the absorption by the layers of air. This excludes all hallucinations of the psychotic type (for which physical stimuli are unnecessary), but not a momentary hallucination of a new type, which would be spontaneously produced in the witness of sound mind by the sight of real physical objects or phenomena which the witness could not identify. Such visual experiences would also obey the laws of optics, the hallucination produced aligning itself nicely with the statistics of Poher.

Here again, we have to ask ourselves if this type of hallucination is plausible, and if the hypothesis implied is verified by its consequences.

Certain mistakes of perception are extremely common and well known by psychologists, but their primary characteristic is to last

only a short time: Who has not been able to see, while driving at night, an imaginary obstacle appear in front of one (a person, a barrier, etc.) for a few seconds, which is only an illusion produced by fatigue? For fatigue creates a state of semi-sleep (characterized by the alpha rhythm on an electro-encephalogram), in the course of which images surge up which recall those of dreams. An external stimulus (which is here the "obstacle" itself, felt to be a danger by the driver) always suffices to destroy this state.

However, this type of misinterpretation differs radically from the always strange and sometimes terrifying scenes, rich in incomprehensible details, seen while fully awake, often in broad daylight, lasting several minutes, by UFO witnesses of whom it is often affirmed that they are perfectly sane and have not been drinking. One can put this even more strongly: psychiatric literature, which is totally devoid of UFO symptomatology, does not describe profound and lengthy hallucinations except in persons suffering from pathological states (psychoses, brain damage, etc.), and it never reports persons who are not suffering from these states being subject to them,[15] even where there is a lowering of the level of awareness, due to seeing an unusual object or a natural phenomenon which the witness cannot interpret.[16] To invoke such a process to explain UFO sightings, is to utilize a deus ex machina more gratuitous than the classic reference to extraterrestrials; for the existence of extraterrestrials is in accord with the data of modern science, whereas the existence of "waking" deep hallucinations which spontaneously affect subjects making them see "saucers" is not.

Let us nonetheless accept this new hallucinatory phenomenon and seek to find out if it is verified by its consequences. To this question, numerous facts lead unambiguously to a negative response. Our refutation utilizes arguments similar to those which we have given above in relation to the parapsychological "model."

First of all there do not exist correlations between waves of UFOs and geographical and temporal concentrations of objects (planets, satellites, weather balloons, helicopters on maneuvers, auto headlights lighting up the countryside at night) or phenomena (atmospheric or otherwise) which are supposed to furnish the stimulus for the supposed spontaneous hallucinations: the lack of correlation exists because this type of stimulus is omnipresent, statistically, in all countries and all time periods, which goes against the very notion of "wave." This argument is particularly important for landings and "close encounters of the 3rd kind," which are observed particularly in rural places of low population density, where the probability of observing natural celestial phenomena is greater than it is in the city, and stays high outside of UFO waves.

Then again, to say "hallucination" is to say "psychological phenomenon"; but descriptions of UFO's, as J.J. Walter has remarked,[17] do not bend to fit the rules which psychoanalysis has put forward for products of the unconscious, which always have a symbolic import. More precisely, the UFO phenomenon presents a very great number of purely physical properties without great symbolic meaning. For example: erratic "dead leaf" movements, pulsing lights, monochromatic light beams (truncated or not), buzzings, etc.; to which must be added the

discoidal form, about whose signification it seems that Jung was totally mistaken (J.J. Walter) in invoking the mandala. And if certain themes associated with some close sightings of UFO's and most of the contacts of "the 3rd kind" are apparently charged with unconscious human contents, on the other hand other themes are almost systematically absent from the general tableau of observations of UFO's (distant or close encounters), such as war, violence, etc., which have nonetheless solid roots in our psyche! Here again one finds the profound originality of the phenomenon, which clearly sets it off from that which the unconscious would produce.

Let us finally add that the thesis of the spontaneous waking hallucination does not take account of the physical effects alleged, such as marks on the ground, the stoppage of automobile motors, etc.

THE SOLUTION OF THE PROBLEM

It is now possible to decide what UFO's are not, and also what they are, and more generally to draw up a balance sheet from the reflexions which the phenomenon inspires.

1) UFO's are not space-ships come to explore our planet by travelling through interstellar space; nonetheless they present themselves to us in the form of flying machines apparently constructed and guided by an Intelligence;

2) UFO's are not human parapsychic materializations, nor spontaneous hallucinations, even though they can sometimes create paranormal interferences in the witnesses in close encounter cases;

3) to move, UFO's---according to the best testimonies---can just as well travel through the atmosphere as disappear on the spot to rematerialize at a distance, an effect which seems to belong more to magic than to current physics;

4) the true solution to the problem of interstellar voyages does not reside in moving along a straight trajectory between stars, as such a trajectory would take infinitely too much time (due to the limiting factor of the speed of light). If no better solution than travel along a trajectory exists at all, the slow and progressive exploration of the galaxy is nonetheless at the disposal of an evolved civilization which is willing to cut itself off from its roots; but this exploration would not be carried out with craft responding to the description of UFO's. If on the other hand a solution exists, which avoids the difficulty through the mastery of a "hyper-physics" permitting the use of space-time warps or other processes of which we do not have for the moment any idea, then the machines used for these space-time warps would more likely appear to us, when they showed themselves, to be the products of magic rather than physics, even if in fact they were nothing of the kind. All solutions of this last type naturally

invalidate conclusions usually drawn vis a vis the extreme rarity of extra-terrestrials to earth---conclusions based on the postulate of interstellar trajectories through our own space-time.

5) Modern astrophysics lets us anticipate the existence of innumerable planetary systems outside our own, around millions of stars in our galaxy and others external to it, systems in which life must have appeared and developed as on earth, to give rise to species more complex and intelligent. This, all astronomers today admit. Yet, there is no scientific reason to think that the human mental level represents a summit of cosmic biological evolution (even if it culminates here on earth). In other words, certain extra-terrestrial species should---keeping here a sense of proportion---transcend the human level of thought, as radically as we transcend psychically the level of animals.[18] Taking account of the enormous disparities of the ages of stars and their planetary systems (differences of billions of years), there is small chance that there is at this moment, in the galaxy, another living species possessing exactly our intellectual level and a fortiori of our degree of scientific-technical evolution. Depending upon which planetary system is concerned, we would find that either this level is not yet attained, or that it has fundamentally been exceeded (which would necessarily imply that the problem of interstellar liasons has been resolved in one fashion or another). The age of the earth is 4.5 billion years, and we do not know how much time it will yet take for man to go to the stars, if he is capable of doing so. One can in any case agree that at the end of five billion years, on the average, a planet which has given rise to life will spread out into the galaxy. As the galaxy is at least three times as old as the earth, it is evident that the first interstellar liaisons had no doubt been realized before the earth was born! In other words, the whole galaxy, whether it appears so to us or not, has been explored, even "colonized," for millions, or even billions of years.[19]

6) An intelligence, which is not our own, directs the UFO's. Everything takes place as if this Intelligence knows our own degree of scientific-technical evolution very well, and gives to UFO's forms which will appear plausible to the witnesses in part because they actualize, in each historical period, the human technological dream of the moment.[20] Similarly, everything takes place as if the Intelligence in question "shows" us landing scenes (utilizing, perhaps, the resources of provoked hallucinations) whose scenario is often inspired by the preoccupations of the witness, his culture, and his specialization. Finally, one recognizes the elusive and fugitive character of UFO's. The ensemble of its behaviors come together in diabolical fashion to leave the great majority of individuals who are not UFO witnesses in serious doubt as to the credibility of the accounts, supporting the belief that we are masters of the earth and the UFO

visits are only crackpot nonsense. And at the same time, the indefinite and monotonous repetition of cases of UFO appearance creates in a fraction of the population a progressive conditioning to the reverse idea, too often associated with an irrational and fatalistic renewal of religious beliefs.

Arrived at this point, it seems nearly useless to conclude, since the obvious conclusion imposes itself.... I have too often heard Jacques Vallée argue, making use of paradoxes, that one can find dozens of different explanations for the UFO phenomenon, all equally valid. I am not at all of this opinion. The dossier is sufficiently thick, it has already been utilized and analyzed with sufficient care, so that in the absence of knowledge as to how UFO's function, we can nonetheless with a high degree of probability infer from whom they derive. Appeal to mysterious entities come from Elsewhere (translation: "parallel" universes or other space-times not of this world), which certain UFOlogists like Vallée would like to oppose to the intervention of extraterrestrials belonging to our universe---revivifying the occultist illusion---can be explained historically by the discovery of the probable identity of the modern UFOnauts and the "little people" of the past,[21] which in no way invalidates the general rule, since the phenomenon merely takes on the appearance which the witnesses of each period expect. But this chameleon effect, which is a characteristic of the UFO phenomenon, should not make us forget two matters: 1) science has made some progress since the Middle Ages, and we understand better now where to look for external Intelligences; and 2) one must very probably "travel through" other space-times at the stars by the Royal Road; which is to say that the opposition between occult Entities of sub-space and extraterrestrials from our universe is only apparent and raises a false problem. The irrational, "magic" aspect of UFO's, which certainly holds for a portion of this spatio-temporal dimension which we do not know how to master, does not imply that we have to appeal to Entities which are not of this world, and of which science is ignorant; it would appear more simply as a necessary consequence of an extraterrestrial super-technology belonging to our own universe, whose existence science does lead us to expect.

And these extraterrestrials have not arrived to discover and study us; for everything leads us to believe that they have known us since our origins. We would be lost to conjecture about the meaning of their enterprise: why do they fashion UFO's---with the exception of the saucer-like discs---in the image of our representations at the moment? Why do they show us with persistence and monotony these frequently absurd scenes of overflight, pursuit, escape, and above all landings, which must certainly signify something, but what? What is certain in any case, is that the phenomenon operates here in the dominant role: we do not know how to intercept it nor understand it; and we sense very well that its power defies completely our technological possibilities, if not our mental ones.[22]

Are we then the colonized without knowing it? Ignorant, that is, because one must be intellectually capable of conceptualizing the type of manipulation of which one is the subject and know that one is being manipulated? The cow does not know that one expects it to produce milk, and the bull that we will cut it up for steaks; the cow and the

bull know the farmer, as we know UFO's; but they are ignorant of the dairy co-operative and the butcher. I do not understand my old friend Aimé Michel, when he says that he feels reassured that we are not the prey of these Beings who could surely wipe us out, and who nonetheless avoid a direct contact because it would traumatize us and cause us to abandon all effort to advance by ourselves, since they are so much in advance of us.[23] Michel deduces that the predatory instinct, which is a biological constant here on earth, even up to the human level, stops beyond it, and this arrest is the condition for admission to the "club" of those who have mastered space and time. Well! To be sure, we are not eaten---besides, would we be digestible? But can we know in what way, perhaps, we are utilized without our knowledge by the "The God of the armies of stars" of the Bible, aided by his soldiers the Elohim?

THE REASONS FOR INTELLECTUAL REJECTION

It is intentional that I have just invoked Yahweh Sabaoth. For ultimately, in the present context, the Biblical text takes on another dimension, to which the centuries of religious yammering have not accustomed us! More precisely, the comparison which we have just made obliges us to confront directly the truth that with UFO's, we are led to re-introduce---naturally in a completely new form---God and the celestial Entities in the field of physical reality. Let us examine this idea a little more closely, along with its consequences. For in its consequences lie the reasons for its rejection, and not elsewhere.

Men have believed from time immemorial in all-powerful divine Entities, residing in Heaven or Hell, to whom are attributed all earthly phenomena which they can not understand: the wind, the tides, thunder, eclipses, etc. The great monotheistic religions have not fundamentally modified this basic concept; for if, instead of the multiplicity of Gods, they have substituted a single God, they have added on other celestial creatures---the angels---whose power over men is well known. It is relatively recently, in the history of Christianity, that the single God has become a being totally spiritualized; from the time of the Biblical Eden, Yahweh---or what has been believed to be Yahweh---addressed himself directly to Adam, as a physical Being. What, on the other hand, has not changed, as long as there have been religions and men, is the power attributed to these divine or angelic Entities to influence the course of terrestrial events.

In reaction against these conceptions of an earth and its inhabitants subjected to Divine Will, science has been built upon the gamble that man, thanks to his reason aided by scientific method, can and should push back the limits of the unknown. He should reduce those phenomena which were attributed in earlier times to acts of divinity to natural and logical mechanisms which obey scientific laws. This fortunate trend of thought, which was at the origin of the immense philosophical, scientific and economic revolution which we know, unfortunately had the consequence---thanks to the law of the excluded middle---of accrediting little by little, in the mind of scientists who do science as well as that of intellectuals who think they comprehend it, two postulates which have become the credo of the intelligentsia:

First postulate: Since science progresses by ceaselessly pushing back the limits of the unknown, there is nothing in the universe which is in its essence inaccessible to the human brain, which thus finds itself promoted to the supreme level, that previously occupied by divinities. To be sure, certain scientists are still believers, but even these (with rare exceptions) have the greatest difficulty in conceiving of the non-preeminence of the human psyche in the hierarchy of levels of intelligence in the bosom of the cosmos, allowing perhaps an exception for God, whom they situate in any case outside the physical world.

Second postulate: Because all the phenomena of nature which science has attacked have shown themselves one after the other to be reducible to a series of logical mechanisms following from the laws of the material world, the hypothesis of an intervention by divine or transcendental Entities in the unfolding of phenomena is useless or even impossible. Science has learned the habit of only asking how things work, and never asking why they do, becoming exclusively reductionist. Confronted by a new phenomenon which it is unsuccessful at reducing, its basic instinctive reflex is to deny the facts or at least a possible interpretation of these facts in terms of an intervention by intelligent extraterrestrial Entities, which recalls only too much the obscurantism against which it is fortified. Science now believes in extraterrestrials, as we have said. But only on the condition that they stay light-years distant from the earth. Or, if they visit us, that they do not do so in a covert manner, manipulating us, and thus revealing their transcendental level which places them outside the reassuring order in which science has tried to confine the world with which we are confronted.

The aggressive militancy of the "rationalists" in regard to consideration of the reality of UFO's, the distrustful irony of the intelligentsia concerning UFOlogists, even the attempts made by some of the latter with the intention of escaping the extra-terrestrial interpretation of UFO's, give witness consciously or unconsciously to the fear which we have just analyzed.

It is quite clear that the generalized study of the UFO phenomenon, taken in hand by a tardily motivated scientific community and thus brought into the light of day, can only take place through the ruin of the current dominant ideology. It would be in any case illusory to think that the victory would be achieved thanks to the efforts of UFOlogists, who are now preaching in the desert. Even the national security services, who know (I believe) that there is indeed something to the reality of UFO's, but do not know how to grasp the problem, refrain from making the matter public, fearing both that it would create a panic which would destabilize world equilibrium and also that it would elicit a movement of denial by intellectual and political elites, who would refuse to accept these revelations. Certainly one can hope that the bastion of the opposing forces will fall like a ripe fruit, if new developments in theoretical physics---totally independent of UFO research---furnish a conceptual framework which permits UFO's to be integrated rationally into the edifice of science, in reducing to physics what now appears to us as "magic" in the functioning of these "machines." But are we capable of developing

science to this point? If the intellectual gap between ourselves and our visitors is in essence unbridgeable, the answer is no. Which is why I fear another eventuality: it is that we will remain unable to grasp the phenomenon, and that, by its ceaseless repetition, it will finally create a pernicious and stupifying wave of religious credulity, opening the door to a new regression of reason.

Perhaps this the goal that "they" seek?

Paris, 9 November 1978

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1. Although it only takes 8 minutes for the light of the sun to reach the earth, because of its speed of 300,000 km/second, it requires four years for the light of the nearest star to reach us, and 100,000 years to cross the breadth of the galaxy!

2. With the exception of flying saucers, I would say; for these have been observed from the times of the Romans, and they were also seen in the last century. The "flying saucer" would thus be an invariant of the phenomenon.

3. Bertrand Méheust, Science-Fiction et Soucoupes Volantes, Mercure de France, 1978.

4. Pierre Guérin, "Le problème de la preuve en ufologie," in Jean-Claude Bourret, Le nouveau défi des OVNIs (Editions France-Empire, 1976), p.304 et seq.

5. I have already suggested that, viewed from a distance, the "airship" of 1897 did not differ in any respect from our modern UFO's; which tends to the credit of the thesis of induced hallucination for close-encounter witnesses. In fact, a more profound study of the wave of 1897 reveals that numerous remote observers of the "airship" drew it in the same fashion as witnesses who approached it on the ground, which reduces the bearing of the "induced hallucination" thesis to the shorter distances, and allows one to think rather that the phenomenon imitates the form of objects it presents according to the period in which it appears. Having said this, the "air-ship" was a very good UFO; it had the same movements in the sky, and a certain type of well-known behavior toward the witnesses (display and departure, landing scenes, etc.)

6. Pierre Vieroudy, Ces OVNI Qui annoncent le Surhomme (Tchou, 1977)

7. Information communicated by Michel Picard.

8. Pierre Guérin, "Sur la profond unicite des diverses manifestations paranormales extraordinaires," in No. 1 of Sciences-Frontière (Autumn 1978), published at Le Chambon-su-Lignon.

9. The scientific method is the ultimate weapon of real scientists since Galileo, and its principle, which was magistrally propounded by Claude Bernard a century ago, consists in a constant confrontation between explanatory models and the data from observation--- all models, to be worthy of the adjective "scientific," have to be tested by the comparison between their deduced logical consequences and facts experimentally determined either previously or specifically to test the hypothesis. It can be remarked (Karl Popper) that such a comparison can immediately display the incorrectness of a model (as is the case with those that we are refuting here), but it cannot rigorously prove its correctness, in as much as one is never sure that new observations will not require it to be abandoned, or at least modified, even though up to that point it "agreed with" the facts.

10. See note 6, pp.133-155.

11. Nicolas Greslou, "Vagues d'OVNI et inquiétude de la population," in Bulletin du Comité Savoyard d'Études et de Recherches ufologiques, Nos. 4 & 5 (1978).

12. Nor is it true, contrary to a belief very dear to the intelligentsia, that UFO waves are induced by the media: a great number of important waves, recognized as such by UFOlogists who collect accounts, have remained partly or totally ignored by the press, who do not inform the public (as in the Spanish wave of 1954) or do so only after some months delay (the wave of 1973 in France). "Mass hysteria" thus explains nothing at all!

13. Michel Monnerie, "Et si les OVNI n'existaient pas"? (Les Humanoîdes associés, 1978).

14. Claude Poher, Etude statistique des rapports d'observation du phenomène OVNI (study carried out in 1971, completed in 1976, now part of the G.E.P.A.N. dossier). Poher's results have been confirmed by new and more extensive analyses which have received the approval of the scientific council of G.E.P.A.N.

15. Certain recent disciples of C.G. Jung to the contrary. One can thus see, on French television, the oldest translator of Jung's works into French argue, to the great satisfaction of the "rationalist" journalist of the network, that psychological material derived from the unconscious can largely explain UFO sightings. This however explains nothing, for even if this were true (which is not the case, as we will later see), it would still have to be proved that the emergence of this material actually creates the hallucinatory scene. However, this possibility is impugned by Jung himself, because in his book UFOs-A Modern Myth (Gallimard, 1961), he does not furnish, in support of his thesis any hallucinatory visions of UFO's, but only the dreams of some of his patients!

16. It is actually the opposite which takes place: instead of hallucinating its sight, the UFO witness (genuine or supposed to be) seeks in general to interpret this sight in terms already familiar.

17. Jean-Jacques Walter, "Les Soucoupes Volantes phenomène global," in Lumières dans la Nuit, No. 178 (October 1978).

18. See, for instance, Aimé Michel, Mysterieux Objets célestes (Arthaud, 1958, p.379 et seq.)--Translated as Flying Saucers and the Straight-Line Mystery (Criterion, 1958); Pierre Guérin, Planètes et Satellites (Larousse, 1967), p.294 et seq.

19. T.B.H. Kuiper and M. Morris, "Searching for Extraterrestrial Civilizations," Science, Vol. 196 (1977), p. 616 et seq.; and David W. Schwartzman, "The Absence of Extraterrestrials on Earth and the Prospects for CETI," Icarus, Vol. 32 (1977), p.473 et seq.

20. See the Méheust book cited in Note 3.

21. Such "little devils" have been called lutins and gnomes of all kinds (the Poitevin "farfadets," the Breton "korrigans," English "pucks," Norwegian "trolls," etc.) not to mention fairies and Satan himself. See Jacques Vallée, Passport to Magonia (Henry Regnery, 1969).

A FABLE

The big breakthrough for the parapsychologists finally came when Dr. Jeremiah Gizmo realized that a fully replicable experiment might best be attained by looking more closely at the "goats" than at the "sheep" in psi experiments. It had earlier been noted that some skeptics towards psi actually scored significantly below chance. Instead of refining new testing procedures to check purportedly "gifted" subjects, and seek to consistently demonstrate psi, he realized that it would be equally useful to seek to demonstrate psi missing. In addition, it would avoid complications since the skeptics would be involved and would not be likely to accuse themselves with fraud. So Dr. Gizmo set to work.

The results were astonishing. In gathering large pools of skeptical subjects, Dr. Jeremiah found he could consistently repeat the psi missing effect. Skeptics who sought to replicate the work soon found themselves able to produce psi missing. They had to admit that this proved the existence of psi since consistent below chance hits by skeptics proved psi as much as would above chance hits. It seemed that the repeatable experiment sought by parapsychology had been found. There was still no luck in getting believers to score consistently above chance, but proving the existence of bad luck was still proof of luck. Unfortunately, this converted the skeptics to believers and eliminated the phenomenon. --MT

SCIENTIFIC STUDIES OF THE "LUNAR EFFECT" AND HUMAN BEHAVIOR

COMPILED BY IVAN W. KELLY

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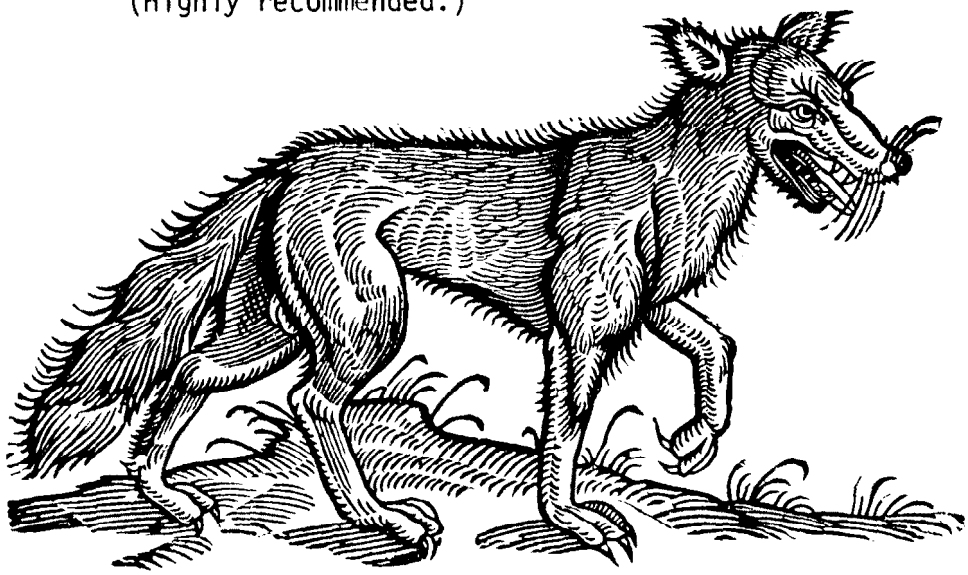

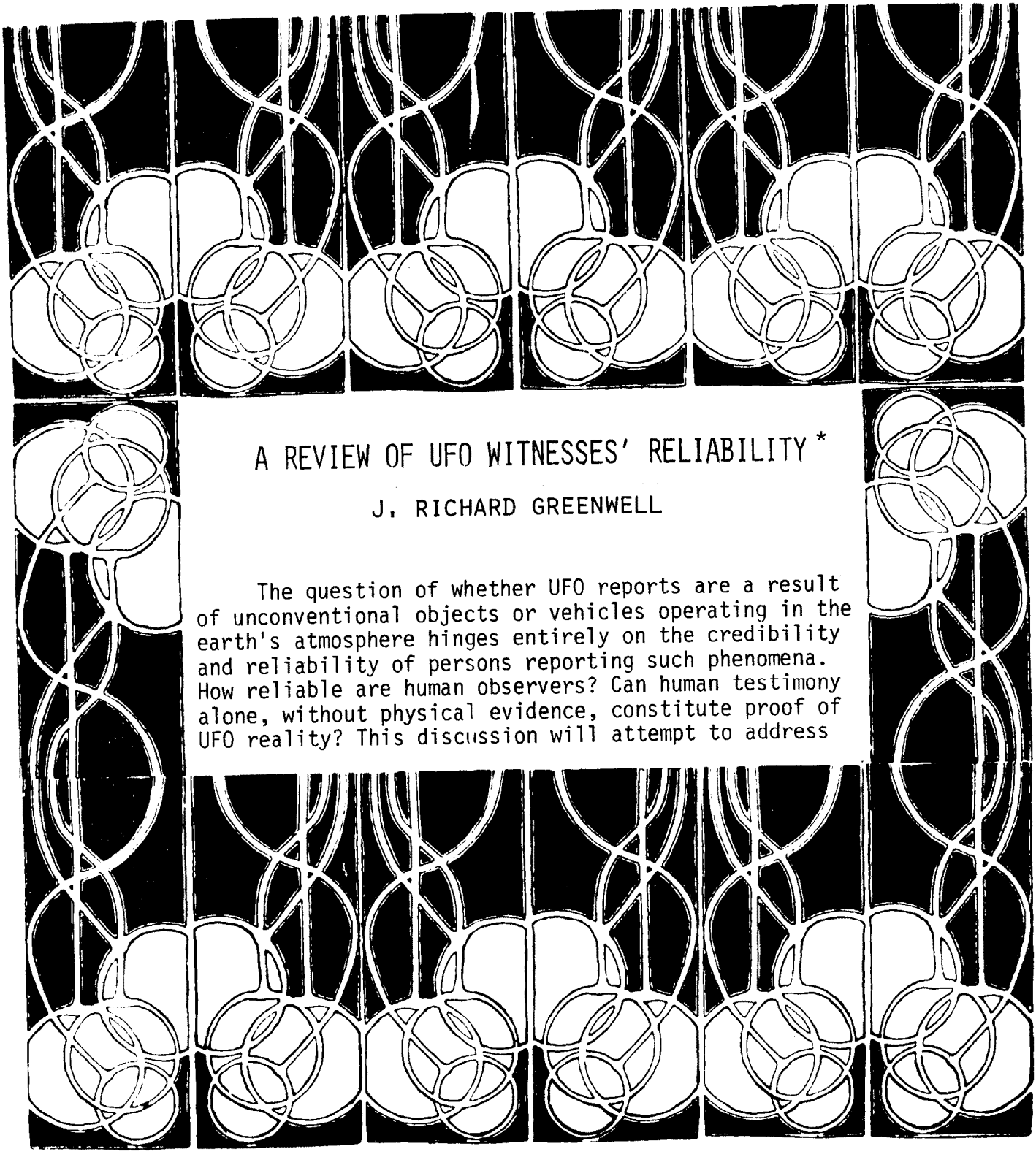

A REVIEW OF UFO WITNESSES' RELIABILITY*

J. RICHARD GREENWELL

The question of whether UFO reports are a result of unconventional objects or vehicles operating in the earth's atmosphere hinges entirely on the credibility and reliability of persons reporting such phenomena. How reliable are human observers? Can human testimony alone, without physical evidence, constitute proof of UFO reality? This discussion will attempt to address

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these questions.

Potential UFO events, if observed, generally proceed through four separate stages to the production of a final report. In sequential order, these are:

1) witness sensation of environmental stimulus;
2) witness perception of sensed stimulus as unidentified;
3) witness recall (memory) of perception;
4) investigator interpretation of witness recall.

By the time the final report is produced, sometimes weeks, months, or even years after the observation, the similarity between the original event and that described in the report may differ substantially. A review of the factors affecting these four stages is therefore of interest.

Sensation

Contrary to what most UFO debunkers believe, human sensation is extremely sensitive, and, in most cases, far superior to any man-made instrumentation. The human visual receptors, which are actually a part of the brain, are the culmination of the 100 million or so years of mammalian visual evolution. The arboreal and diurnal existence of our primate ancestors resulted in a formidable increase in visual accuity. Human detail accuity, for example, is capable of detecting a black telephone cable against a bright sky from a distance of 2.5 kilometers, about half a second of arc (recognition accuity is somewhat less refined). A single photon is sufficient to fire one rod (a photo-chemical receptive element in the retina of the eye) and the firing of as little as ten rods is sufficient to create a minimum threshold of visibility (about 100 photons have to enter the eye, however, for about 10 of the photons to "survive" and reach 10 retinal rods); that minimum threshold of visibility is equivalent to a night observer detecting a candle at a distance of no less than 250 kilometers (Hecht 1934).

The arboreal evolution of primates also resulted in the eyes shifting to the front of the face, producing binocular vision, of great importance today for man's everyday activities. Another result of an arboreal, diurnal habitat (due perhaps to fruit discrimination) is perfect color vision, although there is no known correlation between color vision and intelligence; that is, there is no direct evidence that color vision is one of the factors necessary for the development of intelligence. Most other mammals do not have as good color vision as do the primates.

Human auditory sensation, although restricted to under 20,000 cycles per second, is also extremely refined, being able to discriminate approximately 340,000 different tones, based on frequency and intensity. Some of these tones can differ by as little as .003 (that is, after hearing a tone of 1,000 cycles, one could tell that a tone of 1,003 cycles is different in pitch). In fact, if our auditory sensory organs were any more refined than they already are, we would

begin hearing the random movement of air molecules, and that would, of course, disrupt the very purpose of the organs.

While visual sensitivity, and its respective cortical representations, has increased during primate and human evolution, there has been a corresponding decrease in the cortical representation of the senses of olfaction and gustation, which are of less practical importance to arboreal primates. The volume of the olfactory component of the brain, for example, has decreased from .1062% in insectivores, to .0190% in lemurs, .0011% in Old World monkeys, .0007% in apes, and a miniscule .0001% in humans.

In conclusion, it can be stated that the visual and auditory receptors of the average UFO witness are extremely accurate, while the olfactory and gustatory receptors are not very sensitive or reliable.

Perception

Higher-order perception of a sensed image, what some writers mistakenly believe to be the responsibility of the sensory organs, is also remarkably accurate, but can at times be less reliable, producing an incorrect representation of the environment. It is also subject to cultural and social variables. One psychological theory, the "empiricist" approach, states that perception is a learnt process, based on experience; that is, the observer requires more information than is available on a two-dimensional retinal projection, and memory of previous experience enables the observer to organize the visual stimuli into three-dimensional objects. This would explain a far-away, strange object (or person) being erroneously perceived as a familiar one, only to be recognized properly upon closer inspection. Many modern psychologists, however, discount this theory, and have presented experimental data supporting the proposition that, if the complete retinal projection is used, and if the changing perspectives produced by a moving observer are included, more than enough information is available from visual sources alone to arrive at accurate perception.

Perception is a continuing area of investigation, and UFO researchers would do well to recognize the nebulous knowledge that currently exists on the subject. Advances in this branch of psychology will ultimately shed further light on the question of UFO witness reliability.

Recall

Knowledge on memory and recall is even less advanced. So-called "long-term" memory has almost an infinite amount of space for storing information (the number of possible interconnections among brain cells being greater than the entire number of particles in the known Universe). The physical basis for memory is still totally unknown; it is generally assumed that what makes it possible are permanent changes in the physical-chemical structure within neuron links, but these changes remain one of the mysteries of science.

It is also not known if information placed in memory is ever

"erased." One hypothesis predicts a permanent memory recording in the temporal cortex of each brain hemisphere wherever an individual is paying conscious attention to a stimulus (Penfield 1951). If the human brain does permanently record all conscious experience, it certainly does not make it easily retrievable. Some psychologists have proposed that future techniques could break down the barrier between the supposed permanent memory and the currently-inhibited recall (Adams 1967). Hypnosis appears, at times, to aid recall of information not consciously remembered.

Research in the areas of short and long-term memory and information storage, recall, and "forgetting" are actively being pursued today. Until more definitive theoretical frameworks are developed, the UFO investigator should consider human recall as subject to occasional, usually involuntary distortions.

Interpretation

The principal factors involved in the investigator's interpretation of the witness' recall are his attitude toward the witness, and, even more important, his attitude toward the UFO subject. Reports by persons of social, economic, or intellectual stature will be interpreted in a more favorable light (thus almost every UFO book repeatedly refers to sightings by "reliable witnesses," such as pilots, air-traffic controllers, engineers, businessmen, clergymen, etc.). As for attitudes toward the UFO subject, investigators who subscribe to the extraterrestrial hypothesis, or other unconventional explanations, may interpret the witness recall in terms of previously reported UFO events, often in a better light than it actually deserves. Likewise, investigators prejudiced toward a conventional explanation for all reported UFO events may interpret the witness' recall in such terms, in many cases altering the recalled information in order to do so.

Although sensation, perception, and the whole question of witness reliability are topics often mentioned, they have been given little and generally superficial treatment in the UFO literature. Some opinions have come from individuals with no training in psychology, although they may have excellent credentials in other areas. Some representative opinions from each of four fields, astronomy, sociology, psychology, and psychiatry, concerning UFO witness reliability are reviewed below.

Selected Opinions of Professional Scientists

Astronomers

Frank Drake, a proponent of intelligent extraterrestrial life, stated that, in some cases, "perception mechanisms, like eyes, have simply failed," and "sometimes witnesses see what they really want to see" (Drake 1972). He also reported that about half the details provided by meteor witnesses are inaccurate after one day, about three-quarters are inaccurate after two days, about ninety percent after four days, and, after five days, "people report more imagination than

truth." Drake erroneously concluded that "honest normal people make errors, because the human mind does not always have perfect sensors."

William Hartmann, an astrogeophysicist affiliated with the former University of Colorado UFO Project, used the term perception in place of sensation, and introduced the term "conception" in place of perception (Hartmann 1969). He paid particular attention to the re-entry of Zond IV, a Soviet spacecraft that disintegrated in the Earth's atmosphere on March 3, 1968, providing a controlled experiment in human perception; some observers had reported a "formation" of craft, others reported "saucer-shaped" or "rocket-shaped" objects, and yet others reported "windows." Hartmann termed these the "airship effect," and postulated that "conceptions have been heavily influenced by the 'flying saucer' concept in movies, T.V., and periodicals."

The former U.S. Air Force scientific consultant on UFOs, Allen Hynek, who is now a UFO "proponent," stated that "if our UFO reporter has by his past action and performance shown a high degree of reliability and responsibility and is known to be stable and not 'out of adjustment,' then we have no a priori reason to distrust his coherent report...the crux of the UFO reporter problem is simply that perfectly incredible accounts of events are given by seemingly credible persons" (Hynek 1972).

Carl Sagan, a planetary astronomer at Cornell University, believes, on the other hand, that "there are no cases that are simultaneously very reliable (reported independently by a large number of witnesses) and very exotic (not explicable in terms of reasonably postulated phenomena)" (Sagan 1973).

Sociologists

Robert Hall, of the University of Illinois, stated that "if we apply these [legal] criteria to the witnesses and the testimony of hard-core UFO reports, some of them stand up better than many a court case," but also that "human memory is fallible in such matters, and it is conceivable that witnesses are unconsciously influenced by information read or heard long before" (Hall 1972). He concluded that "either there must be a distinctive physical phenomenon which these witnesses have observed, or there must be a powerful and poorly understood motivation rooted in projection, or contagion of belief, or a similar mechanism. I find it more plausible to believe that there is a distinctive physical stimulus."

Donald Warren did not address the question of UFO witness reliability directly, but applied status inconsistency theory to UFO sightings in an attempt to demonstrate that they "are linked to status frustration and, especially, to perceived status deprivations relative to one's position on the social ladder" (Warren 1970). Working with 1966 Gallup Poll data published in the Condon Report, he found what he believed to be above chance examples of status inconsistent persons reporting sightings. Status inconsistency is a theory in which three variables, ethnicity, education, and income, are not on the same "level." An example would be a black physician, who would rate "low"

in ethnicity, "high" in education, but only "moderate" in income; a black laborer, on the other hand, would be "consistent," being "low" in ethnicity, education, and income, and would, as a result, be less likely to report a UFO than the black physician. Warren also claimed to have found evidence supporting the proposition that sharply status inconsistent persons would be more likely to interpret their sightings as having an extraterrestrial origin than would less or non status inconsistent persons.

Ron Westrum, a UFO proponent, concluded that "eyewitness testimony can be very useful - but only to the degree that one is aware of its limitations, and the forces that are likely to produce distortions in it" (Westrum 1977).

Psychologists

Peter Delin, from Australia, stated that "the credibility of witnesses must be assessed by the same sorts of techniques as might be used in a court of law, with the same scrupulous separation of the witness' report from his interpretation of that report" (Delin 1971).

Douglass Price-Williams stated that "the difficulties are formidable" when addressing the question of witness credibility, and that "most people are unused to angular estimation; most people tend to express themselves in thing-language and not in process-language" (Price-Williams 1972).

In one of the most scholarly treatments to date, Roger Shepard, of Stanford University, stated that the UFO problem is "amenable more to the methods of the psychologist than to those of the physical scientist," and that "the vast bulk of the data upon which we must base our scientific investigation comes - not from physical recording or measuring devices - but solely from one or more human observers" (Shepard 1968). Shepard, a perceptual psychologist, correctly pointed out that human powers of recognition "surpass anything that we have yet been able to accomplish by physical instrument or machine," and that "when an event occurs without warning, leaves little time for careful observation, and, indeed, occasions extreme fear or anxiety, the average witness often retains an accurate, almost photographic record of the event - a record, moreover, that can be largely recovered from him even though he lacks the words to describe it himself. Possibly then, in allowing our investigations to depend solely upon our informant's inadequate, his misleading and, yes, his sometimes even ludicrous choice of words, we have done him - and ourselves - a telling disservice."

Michael Wertheimer presented a clear review of UFO perceptual problems in the Condon Report, concluding that "details of specific reports are, by the very nature of the process of human sensation, perception, cognition and reporting, likely to be untrustworthy" (Wertheimer 1969). "Thus any report," he continued, "even those of observers generally regarded as credible, must be viewed cautiously. No report is an entirely objective, unbiased, and complete account of an objective distal event. Every UFO report contains the human element;

to an unknown but substantial extent it is subject to the distorting effects of energy transmission through an imperfect medium, of the lack of perfect correlation between distal object and proximal stimulus, and of the ambiguities, interpretations, and subjectivity of sensation, perception, and cognition."

Psychiatrists

In proposing some psychoanalytic theories in relation to UFO witnesses, Lester Grinspoon and Alan Persky referred to so-called primary-process thinking, "the source of all myth, magic, and fantasy," which man supposedly regresses to under conditions of stress, and which "affects the observer's sighting of a UFO and prevents him from making an objective report" (Grinspoon and Persky 1972). They further stressed the relationship between UFO sightings and phallic worship, which, like the symbol of the female breast, has a primal and universal origin; they emphasized the penis-shape of the cigar UFOs, and the breast-shape of the disks. "These considerations," they added, "may also help explain some of the emotionalism which surrounds the subject."

Mark Rhine, another Condon Report co-author, called eyewitness reports "a notoriously unreliable source of information," but that "an open-minded investigator, honestly endeavoring to understand UFO phenomena, cannot dismiss eyewitness reports... neither can he accept such reports without scrutiny, for there are many possibilities for error and distortion" (Rhine 1969).

Berthold Schwarz, a proponent of both UFOs and ESP, stated that "the psychiatrist can also be of help in evaluating the credibility of the witness...the more time we spend with the percipient, the more information we get" (Schwarz 1971). He concluded that, because somebody may have been hospitalized for emotional illness, it does not invalidate his observation: "It might strengthen it. It is complex, and each case must be studied on its own merits. The witness can be the most truthful person in the world, he can be your boss or somebody else you trust, but his UFO account must be questioned, like anyone else's report."

Sidney Walker, III, the only psychiatrist known to have undertaken complete medical, neurologic, neuro-ophtalmologic, psychiatric evaluations of a UFO witness (Walker 1968b), stated: "One recourse, of course, is to deal only with 'hard data' and to simply refuse to deal in any way with eyewitness reports, contending that such observations are unlikely because they are too bizarre or have previously been reported only by 'crazy people.' This kind of reaction reflects scientific closed mindedness. It is apt to be based in prejudice or fear of the unknown (particularly when that unknown, if taken seriously, would threaten one's safety or survival). Such an attitude is among those which the scientist who wants to be objective will guard against, in the interest of truth and progress. On the other hand, the opposite position of complete, unquestioning faith in observer reports is no better" (Walker 1968a).

Dr. Walker went on to suggest an attitude of "benevolent skepticism" and that "specific, specialized medical assessment of individual observers is essential to establishing the integrity of the observer system. Following careful, clinical investigation, much of the human error in observation can be placed into a perception which eliminates the 'blind faith' in eyewitness testimony and gives the reported data a confidence proportionate to its value....Such an approach offers both quantitative and qualitative assessment of central nervous system functioning as it would be reflected in observational reporting."

Additional Considerations

Some UFO investigators have utilized a "strangeness-probability matrix" to rate how unconventional the reported object was on the one hand, and how reliable the report is on the other (Hynek 1972). Strangeness (S) is the degree to which a given report is deviant from what would be expected from a conventional object or phenomena. Probability (P) is the degree to which the investigator can rely on the report, as reflecting a real event, or how much confidence or credibility he may subjectively assign to the witness/es. Low S-P reports are less interesting, high S-P reports being the most challenging, and also the most rare. While the S-P matrix cannot demonstrate the reliability of any particular UFO report, it is a useful analytical tool, particularly when examining a large number of reports. UFO debunkers tend to ignore the usefulness of the matrix.

No discussion on UFO witness reliability would be complete without mention of witness reliability studies in other areas, such as automobile accidents. As is commonly known, such witnesses tend to give varied accounts of such events. Psychologist Elizabeth Loftus studied the effects of numerous variables on subjects who watched filmed auto accidents (Loftus 1974). Fifteen of 100 subjects responded "yes" when asked by questionnaire if they had seen the broken (and non-existent) headlight, whereas only seven subjects responded "yes" when asked if they had seen a broken headlight; the one-word change in the questionnaire was sufficient to increase inaccurate responses by eight percent.

In another experiment, this one with 45 subjects, Loftus showed several auto collision films, and asked five experimental groups their estimates of the speed the cars were travelling at when they 1) "contacted," 2) "hit," 3) 'bumped," 4) "collided," and 5) "smashed." The mean estimates varied as a function of the verbs used: the mean speed of the cars that "contacted" was estimated at 31.8 m.p.h., those that collided at 39.3 m.p.h., and those that "smashed" at 40.8 m.p.h. (a total spread of 9 m.p.h.). In order to determine if memory of the accidents actually changed over time (rather than subjects being temporarily swayed by verbs in a questionnaire), two other experimental groups, one which had reported a faster speed (for "smashed") and one which had reported a slower speed (for "hit"), returned after one week and were asked if they recalled seeing broken glass in the filmed accidents, when none, in fact, had been visible. More than twice as many subjects queried with "smashed" the week before reported the non-existent glass than did those queried with "hit." Loftus concluded

that "the result is consistent with our interpretation that memory itself undergoes a change as a result of the type of question asked ...when you question an eyewitness, what he saw may not be what you get."

The implications of these kinds of findings to the question of UFO witness reliability are obvious. Not only can an observer's memory of an event become distorted, but an investigator may easily and inadvertently influence a witness by the wording of the questions. This is particularly true in relation to timing, speed, distance, and size, which are already known to be problematical areas for most observers (for example: "how many minutes was the object visible?" when it may have only been visible for 30 or 40 seconds). Lawyers are well acquainted with the purposeful use of such "leading" questions to induce a defendant or a witness to admit to something, or to "plant" a suggestion in the minds of the jury, or both (such as the famous "how long has it been since you stopped beating your wife?")

One final point on the topic of witness reliability should be addressed, and that is the often-quoted statement that the testimony of the average UFO witness would stand up in any court of law. That may well be so, but any implication that, as a result, his UFO report constitutes proof of a scientific nature is without foundation. The methodology of science is quite distinct from the rules of law, even Anglo-Saxon law. Legal judgements are often rendered when the evidence is far from conclusive, as demonstrated by the innocent who are punished and the guilty who are acquitted, whereas scientific judgements require far more analytical and repetitive types of evidence. Legal judgements must necessarily be made quickly, whereas scientific judgements are not subject to time constraints; it may take many years of careful study before a scientific judgement is ultimately rendered. Perhaps a court of law can afford to be kind, to be lenient, or to make occasional mistakes; but the court of science cannot, for if it did so consistently, human understanding of the environment would become a garbled one indeed.

Two questions were posed at the beginning of this discussion: "How reliable are human observers?" and "Can human testimony alone, without physical evidence, constitute proof of UFO reality?" As we have seen, human sensation is extremely accurate; perception is also remarkably accurate, but not fully reliable; recall or memory is subject to numerous kinds of distortions. Therefore, despite the excellent data reported by many witnesses from all over the world, the UFO phenomenon, perhaps by its very nature, has not as yet produced the kind of evidence which science requires to constitute proof of its reality.

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A VISIT TO THE CENTRE DE CRYPTOZOOLOGIE

RON WESTRUM

In the fall of 1978 I paid a visit to what might well be called the world headquarters for research on unknown animals. This is the Centre de Cryptozoologie, run single-handedly by Dr. Bernard Heuvelmans, author of books (inter alia) on the great sea-serpent, the giant squid, and most recently "dragons" in Africa (see the review of the latter elsewhere in this issue; more information on Dr. Heuvelmans is included in the Westrum/Truzzi article on the anomaly literature in Zetetic Scholar, Vol. 1, No. 2, p.77). When I visited him, Heuvelmans was working on his next book, which will be on the "man-beasts" of Africa.

The Center is located in a well-appointed building which is the old estate of Verlhiac. Elsewhere on the same estate is Verlhiac Primate Center, watched over by Dr. Scott Lindbergh and his wife Alika, who illustrates Heuvelmans' books and is an author and painter in her own right. The estate is located near the village of Chamassy, close in turn to the small town of Le Bugue. Out of this small center of research in the Dordogne has and will come some of the best literature on cryptozoology available. This literature has come solely from the pen of Heuvelmans, who for most of the year toils away in his study working hard at making sense out of an immense collection of reports from all the corners of the world.

Cryptozoology, of which Heuvelmans can be said to be the foremost living practitioner, is the study of animals whose existence is conjectural. Evidence for the existence of these animals consists of human testimony and/or puzzling physical traces. In the great majority of cases, the testimony and description of the traces are contained in places other than scientific journals, and the search for these puzzling (and fascinating) descriptions is a major literary endeavor. Perhaps at some future time better evidence-gathering methods, such as immediate de-briefing of witnesses and preservation of physical traces, will become more common. (researchers on Sasquatch have a marked advantage in this respect.) For the most part, however, cryptozoology has to get by with evidence largely gathered in the course of other pursuits. Bringing these pieces of evidence together and evaluating their import is a long and taxing job. Heuvelmans brings to this literary detective work the skills of a trained zoologist, and the library at his disposal is superb.

He estimates that his collection contains some 2,000 volumes, over 1,000 of them on zoology alone. Other subjects covered range from ethnography to the history of science and include over 85 volumes which are entirely devoted to cryptozoological subjects. The books are supplemented by an immense collection of articles, a card catalogue containing 12,000 entries on various subjects, and iconographic resources which include 5,000 illustrations or photos. To these printed resources one might add Heuvelmans' immense network of correspondents, in many cases readers of his earlier works.

Heuvelmans himself, although born in 1916, is strikingly robust and always seems to talk with a twinkle in his eye. Far from a literary recluse, he had been a well-known and celebrated jazz singer before becoming a zoologist. He has traveled widely in countries both wild and civilized. Since 1947 he has devoted his efforts largely to the research of subjects cryptozoological.

At present he is working on a series of books which will constitute a complete survey of cryptozoology. Here is the tantalizing series of titles he projects:

EUROPE: From the Beast of Gevaudan to the Alpine Legged Worm
The Last of the Wild Men of Europe

ASIA: The Hairy Colossi of Asia: Wild Men and Mammoths
The Hardly Abominable Snowman

TROPICS: The Hairy Dwarfs of the Islands
From Giant Worms to Winged Horrors of the Orient

AUSTRAL/ASIA: From Australian Bunyips to the Papuan Dragon
The Impossible Animals of New Zealand

NORTH AMERICA: The Forgotten Giants of North America: Mastodons and Giant Squids

SOUTH AMERICA: From the Blond Beast of Patagonia to the Dragons of the Amazon

AFRICA: The Last Dragons of Africa (published)
The Man-Beasts of Africa (in press)
The Astonishing Animals of the Dark Continent

INDIAN OCEAN: The Phantoms of the Islands

RIVERS/LAKES/SWAMPS: Fresh-Water Monsters

OCEANS: The Oceanic Trio: Siren, Kraken, and Sea-Serpent
The Bouillabaisse of Titans: Unclassifiable Marine Monsters

METHOD: The Shark-Hunter's Manual: Introduction to Cryptozoology

One can look forward with anticipation to the appearance of each of these titles. Considering that the first of the series is over 500 pages in length, the collection will be truly encyclopedic.

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LITERALISM AND SYMBOLISM IN ANTHROPOLOGICAL UNDERSTANDING: THE SASQUATCH IMAGE

ROBIN RIDINGTON

For Marjorie Halpin and to the Memory of Wilson Duff

In the clashing cultural tectonics of our encounters with other ways of life, anthropologists have typically been compelled to deal at a personal level with the fundamentalism of another culture's insistence on the literal reality of beings and happenings foreign to our own culturally instructed intelligence. Within the intimacy of participant observation we see the people with whom we have chosen to identify our professional lives holding as true ideas beyond the fringe of the world from which our mission is given meaning. We generally preserve our sanity by the compartmentalizing doctrine of cultural relativism. We seldom seek or find a bridge for the meaningful translation of another culture's fundamental realities into the reality of our own experience. We do not deny our subjects the validity of their experience of beings and phenomena foreign to our own knowledge and experience, but we qualify these as being valid only within the context of another culture's terms of reference. Our anthropology shelters us from fundamentalistic confrontation by assuring us that another culture's meanings are valid in terms of adaptive conditions that differ from our own. It is when our own culture presents us with images from beyond the fringe of our personal understanding that the insulating doctrine of cultural relativism fails us, and we turn to face something ultimate in our personal lives.

The Sasquatch image is such an ultimate intrusion into the very foundations of the anthropologist's own world. Because there are as yet no unequivocal concrete remains of the large humanoid creature, it exists within our own culture as an image of a realm of possibility at the threshold of our discrimination. It is a being from beyond the fringe of our expectations, yet it exists within our own continent and culture. We are not shielded from the hard thinking it imposes upon us by a comforting insulation of cultural relativism. Anthropologists are generally able to locate non-western monsters within the context of a foreign culture's overall system of meaning but the monsters of our own tradition have a reflexive relationship to us in that they compel us to examine the basis of our own system of meaning. The Sasquatch is meaningful to us now because it is an image of liminality we hold in mind. If the physical body of one were to be found, its symbolic meaning would be extinguished, no doubt to reappear in some other image of another order beyond the fringe of our ordinary knowledge and experience.

In the winter of 1968, I first encountered the image of the Sasquatch in a film made by the late Roger Patterson, an ardent Sasquatch hunter, stunt rider, and author of a book entitled, Do Abominable Snowmen of America Really Exist? The showing was for

the benefit of Professor Ian McTaggart-Cowan, the foremost authority on large mammals of the northwest, and I attended with my late colleague, Wilson Duff. As academics whose training and world view included some knowledge of primate evolution and behavior, Wilson and I were in agreement with McTaggart-Cowan that the Sasquatch was highly improbable as a member of the natural fauna of northwestern North America, but we could not deny its possibility because we were bound by the openness of an epistemology in which it is impossible to prove a negative. The Sasquatch as a naturally occurring large mammal is merely unlikely, not entirely out of the question. Patterson, small cowboy with big hat, awkward without a horse under him, wanted us to pronounce the Sasquatch literally real on the testimony of his film. Unknowingly we reflected the literalism of his request in our scepticism, impervious at the time to the power of this image to which we were being subjected. Nor did we know the power that images would have on our subsequent thought. Wilson Duff would later write, "It would be difficult enough---and terribly disconcerting---to explain what the images of our own culture 'mean'; because most of their meanings most of the time, are left below or beyond the view of ordinary waking consciousness. It is doubly difficult to explain the images of a different culture, whose unspoken visions and premises we may not share," (Duff 1975 12). Although our acceptance of Patterson's literalism compelled us to deny the reality of the image we experienced in that darkened room, the germ of another reality must have been generated by the passage of this creature whose only physical context was and remains 23 feet of film containing 951 separate images that merge in the singularity of the mind's eye.

We must have been aware that, like all film experience, those separate celluloid images perpetrated an artifice upon our minds at one level in order to communicate a truth to us at another; but then as now, the exact nature of that truth remains illusive, the paradox inherent in any study of the unknown, glimpsed fleetingly shambling away from where we are precisely as we fix our gaze upon it. In accepting Patterson's claim to the literal existence or non-existence of the Sasquatch shown in his projected strip of reality, we suppressed for a time the meaning of our experience, "left below or beyond the view of ordinary waking consciousness."

Although it remains "terribly disconcerting" to say just what the Sasquatch image means within the context of our own cultural experience, it is now clear that a level of symbolic meaning exists that is dependent on the literal existence of the creature only to the extent that it would probably be destroyed if an unequivocably "real" Sasquatch were to be found. As far off corners of the planet and even the surface features of other worlds become known to our everyday reality, we must look closer to home to find appropriate images of a zone of reality whose meanings are "left below or beyond the view of ordinary waking consciousness." What we saw in Patterson's film has grown in the intervening decade into a substantial image of our culture capable of carrying meanings otherwise lost to us. In the darkness and vision of that increasingly long ago shadow box, we saw what might have been a likeness of ourselves had we only recognized it at the time.

"The creature" as it has come to be called, was walking quickly but in no apparent agitation at an angle away from the camera, along Bluff Creek in and out of our sight as objects foreign to the encounter loomed in and out of the foreground. In contrast to the steadiness of its unagitated progression through a familiar terrain, the camera appeared to be near a state of frenzy, its motion at times entirely obliterating that of its subject. Patterson had been running as he shot, and the violence of his passage over the jumbled logs and boulders separating him from the creature's clear path down the sandbar accentuated the serenity of the other's uninterrupted journey across the 23 feet of celluloid. At some unrecognized level of response, we registered the profoundly reflexive nature of the image's communication with our minds. In the chaotic jumble and unsteadiness of the cyclops Patterson bearing our eye, we came into contact with his excitement and the liminality of the vision he had come to share with us. Patterson's own motion and emotion disturbed both celluloid and retina far more massively than the tiny black figure that moved from frame to frame to frame, then retreated into the obscurity of its unknown primaeval forest.

In straining to identify and track the passage of this sudden anomaly across our unaccepting retinas, we ran panting after the little cowboy, our sky falling, knocking us breathless, without coherent response to the impact of a personal encounter with the unknown. As it came to the end of the trail of frames, the creature turned its head and torso without breaking stride, and focused momentarily on the now steady platform provided by a Patterson who had somehow come to rest before our eyes. We saw the hint of a face surmounted by a surprisingly high forehead and an apparent saggital crest. We saw long arms vigorously swinging to balance the crouch of heavy thighs, and on the chest we saw the hairy pendulous signs of her capacity for nurturing others of her unknown kind. Then she was gone,- and with her, the world with which we had entered that tiny room.

The film was replayed and stopped and caressed in slow motion in pursuit of clarification, but what we saw continued to be real to us only as a creature out of our minds. To have accepted her as real in any other way, we would have been perceived by others of our own kind as having gone ourselves out of our minds. The people and minds that we then presented to the real world were unprepared to accept the totally reflexive quality of that experience, or any other. We were anthropologists firmly rooted to the improbability of such a creature existing here and now as a product of nature. As anthropologists we were as yet unprepared to accept her as a valid image of our own cultural experience. Reality seemed simplistically empirical to us then, for we had not as yet come to realize the immense and potentially overpowering validity of the symbols that even in that time and place were coming to organize our experiences.

The film shocked us both by its stark contradiction of probabilistic knowledge with the fundamentalism of direct experience. Either what we had seen was real or it was an artifice through which Patterson was attempting to coerce us. Our anthropological understanding told us that the creature was highly improbable, forcing us

reluctantly to suspect Patterson of deliberate fraud. In fact, we both attempted to extricate ourselves from an otherwise hermetic dilemma by seeing what we thought to be the line of a zipper down the creature's back. I suggested, in defence of Patterson, that he might have made the film as a kind of dramatic re-enactment of a reality in which he firmly believed but had been unable to demonstrate to the world of science. With Wilson, I discussed the possible analogy between Patterson's portrayal of the Sasquatch and dramatic representations of creatures like the Hamatsa in northwest coast ritual.

After that initial encounter with the moving image of the Sasquatch in Patterson's film, it lay dormant in my life for a decade. During that time, Wilson and I, each in his own way, developed our thinking about the images coming down to us from the native cultures with which we had identified our lives.

It was only after Wilson's death in 1976 that the image of the Sasquatch returned to my mind with any impact. In the spring of 1978 a conference entitled "Anthropology of The Unknown: Sasquatch and Related Phenomena " was held at the University of British Columbia. The conference was the creation of Marjorie Halpin, Wilson Duff's student and colleague in the Department of Anthropology at U.B.C. Unlike the majority of anthropologists unwilling even to discuss the existence of the Sasquatch or even the few who felt the evidence justified a belief in it as real, Marjorie viewed the entire Sasquatch phenomenon as worthy of study. In so doing, she has forced us to confront the untimately reflexive nature of all anthropological inquiry.

The terms with which we translate the fundamental reality of experience in the conduct of other lives must reflect the fundamental reality of our own life experience. The special kind of intersubjective meaning that is the product of encounters between anthropologists and other human beings must be a consciously negotiated symbolic transformation of two different contexts onto some bridging level of organization. In the Sasquatch conference anthropologists and Sasquatch hunters came into formal contact for the first time. This contact forced each of us to examine our own reality as well as the reality of the Sasquatch. In rejecting the Sasquatch as being unreal, we who were anthropologists had tacitly acceded to the literalism of the Sasquatch believers themselves, thereby cutting ourselves off from the other reality of the Sasquatch image growing by leaps and bounds at the edges of our own culture's consciousness. In denying the literal reality of a large hairy biped in the forests of our physical environment, we closed ourselves off from the possibility of understanding the image of this creature and following its larger than life size tracks beyond the boundary of our comfortable academic reality and into the forest of our own wider cultural environment.

The lesson to be learned from Marjorie's initiative in leading anthropology onto the trail of the Sasquatch is that the terms of our own reality must be examined as closely as the terms of the people we study. Our anthropological understanding of other

people's experience is as much a product of our own assumptions as it is of theirs. Taking phenomena that are experienced literally by others into our own culture to be accepted or rejected as literally true or false to our own experience is to deny their meaning as images "left below or beyond the view of ordinary waking consciousness." Because the realm of meaning is inherently symbolic and goes beyond the level of naive realism, there is no such thing as "just a symbol." Symbols are put together from information arising out of culturally instructed experience. As anthropologists we cannot deny the reality of phenomena below or beyond the realm of ordinary waking consciousness because of their symbolic meanings any more than we would deny the reality of phenomena we possess in the clear light of day on the same grounds. We cannot write of the Sasquatch image as "just a symbol" because it is meaningful to us symbolically.

During the years that have passed since my first sighting of Patterson's Sasquatch, the creature's physical existence has been neither confirmed nor disconfirmed, but its meaning as an image in the mind has grown substantially. Along with the casts of footprints and stories of sightings, the figure of Patterson's film has come to life within the mind and experience of most people within my own culture. Although anthropologists were among the first to be shown the image, we have been among the last to allow it to enter our lives. In the life of this image within our minds, the eye is directed inward to the contemplation of meaning. Whatever the fundamentalistic reality of the Sasquatch as a natural animal, its fundamental reality is as an image through which we may reflect upon our own condition. Whatever physical animal may or may not inhabit the back country of our physical wilderness, the real image of a creature is in powerful occupation of the back country of our collective minds. Our vision of her is real as she reflects the image of ourselves we hold in mind. We turn toward the camera, an artifact of our own intelligence.

In being like us in form but existing just beyond the threshold of our ability to grasp it with certainty, the creature compells us to look into the meanings that are obscured by ordinary waking consciousness. Her image acts upon us with the symbolic imagination's power to generate meaning. Both literalism and relativism deny the authority of symbolic experience. We need the Sasquatch image or one like it to incorporate what is unknown into the meaningful pattern of our unfolding knowledge. If that image should be lost to us through the death of an elusive hominid primate, just as our images of the North Pole and the face of the moon were lost to us through exploration, we could expect it to rise again like the Phoenix on the trail of some other being, a reborn creature of our imagination. The Sasquatch image leaves a trail of impressions on the surface of our reality that we can follow only for a few astounding paces until it becomes liminal, disappears into a dense and impenetrable forest of the unknown for which we are in possession of neither form nor meaning. The image derives its power from a subtle balance of concreteness and elusiveness against a background of improbability. The experience of Patterson's

celluloid Sasquatch is immediate, authentic and undeniably real. On encountering it for the second time in my life,I could find no trace of a zipper.

The Sasquatch is liminal in more than its habitat on the fringes of our culture's comfortable dominion. It is also at the verge dividing watersheds of scepticism and belief. It is a creature inhabiting the high country precariously constructed from an interplay of judgement and observation. No one has ever been able to follow the trail of a Sasquatch as they would that of any other animal. Like the human being, the Sasquatch is anomalous in the natural world, but unlike the human being, its anomaly arises from its lack of culture rather than from its cultural excess.

The culture within which we now find ourselves is excessive in its intelligence as an organized system of information but impoverished in the scope it allows for the individual sapient comprehension that has characterized our success as a species. The inhuman intelligence of a culture whose product includes the insanity of nuclear confrontation threatens our very survival. In the Sasquatch we perceive a creature apparently devoid of cultural intelligence but possessed of some other cunning that draws her into a living space where we are unable to follow. Her only artifacts are the tracks of her passage back into the unknown from which she came, while the artifacts of our cultural intelligence threaten to kill the sapient mind that builds up and sustains our world of meaning. In the Sasquatch image we recognize survival against apparently overwhelmingly adverse natural conditions. In our own situation we wish fervently for survival against apparently overwhelming cultural conditions. Her living image is a representation of the hope we hold out for ourselves in a future that is as yet unknown to us beyond the tracks in which we now stand.

My personal encounter with the Sasquatch image ended with my participation in a plenary debate on whether or not to shoot a Sasquatch. I strongly opposed the idea primarily on the grounds that to encourage hunters to fire at humanoid images would inevitably result in human death. The opposing position, taken by a physical anthropologist whose career has been set back by his endorsement of the Sasquatch as real, and by a well known author of several books on the Sasquatch, was that only by producing a dead body could the creature be shown to have existed. The debate carried meanings that extended below or beyond the level of ordinary waking consciousness. At issue was not just the fate of a type specimen of a species as yet to be given a latin name. The real issue was whether or not to bring death to our image of the unknown, whether or not we had to kill our image of survival in order for it to be real. I argued that we must let the image live within us. To attempt to kill it would result in the death of our own kind. For my opponents, only the destruction of that image would make it real. Beneath the level of what felt to me like an utterly surrealistic engagement, we held at issue the fate of our own species, the destruction of the creative intelligence that defines us as human. After the conference ended,

I brought my thoughts into focus in a sonnet.

DO NOT SHOOT THE SASQUATCH: A REFLEXIVE PARABLE

She moves before us. Beauty in the beast
Abounds within the true beholder's eye
The beast within her every cell released
And blown into a galaxy of sky

Unyielding absolute geometry
Of every wave, earth body, vision
Through time unending in a circle, free
At last, at first, at orbit of the sun

Her apprehension in and out of time
Begins to tell the story of her flight
Dreamer returning, passage of a mind
Reflecting in defiance of the night

Do not shoot beauty. Let her live within
Your sheer transparent envelope of skin.

Reference cited: Duff, Wilson. 1975. Images: Stone: BC. Hancock House, Saanichton, B.C.

A SPECIAL ANNOUNCEMENT

In ZETETIC SCHOLAR issue #2 (page 154), an invitation was extended to readers and scientists generally to participate in a special precognition experiment which included two new elements: (1) The research design was to be agreed upon by both proponents and critics of psi to gain a common denominator whereby each might specify in advance the meaning of both what would constitute positive results and the implications of such. And (2) the guesses of the subjects would be pre-posted publicly in ZETETIC SCHOLAR well in advance of the target date at which time we could judge the success or failure of the subjects. It seemed a worthwhile project in line with the general purposes of this journal. Unfortunately, the degree of cooperation this effort produced was minimal. A bit to my surprise, I found the skeptics less willing to participate than the proponents, many of whom offered excellent advice for better controls in the study. The lack of active participation by a significant number of skeptics plus the increased complexity of the originally simple research design necessitated by the criticisms has therefore placed this project into limbo for the present.

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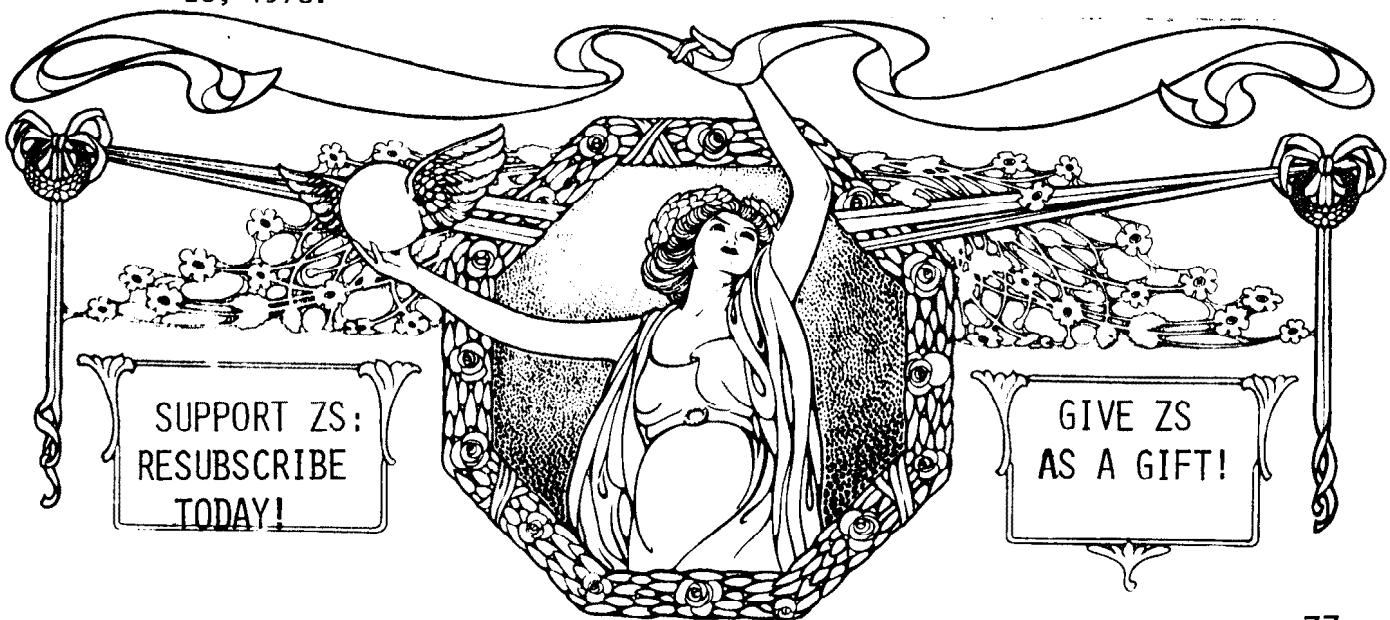

"THE HERESY OF A NEW SYNTHESIS": THE AUTHOR RESPONDS

JOSEPH MAY

The paper, wide-ranging as it was, engendered comments from a number of directions. While all contributions are appreciated, it is perhaps best to restrict further discussion to two areas: (1) the validity of particular Velikovskian propositions, and (2) the treatment of new hypotheses.

I

Some critics, like David Morrison, seem to believe that everyone espousing Velikovsky's ideas belongs in the same monolithic camp. This is shown to be false by the reactions of the respondents themselves. This diversity itself is a sign that a new paradigm of some importance has been presented. It is an inevitable stage and there is no reason to deplore its arrival.

Morrison represents a class of critics who rather dogmatically assume that certain unproven assumptions about the universe are true. I would place Donald Goldsmith, Peter Huber, and Carl Sagan in this class also. From their standpoint there is no discussion possible, for Velikovsky is automatically wrong. Morrison illustrates this when he says Velikovsky is "fundamentally opposed to essentially all geology (whether called uniformitarian, catastrophist, or phrased in terms of more contemporary concepts) because of the absurdly short time scales that he would assign to such fundamental geological processes as mountain building, continental motion, and changes in the Earth's rotation and magnetic field." This is the crux. If Morrison, speaking as a defender of conventional geology, is correct, then Velikovsky is wrong. The time scales are absurdly short. But how can Morrison be so sure about so important a matter?

Under the assumption that the craters on planetary surfaces were caused by impacts, it was at first estimated that the surface of Jupiter's satellite, Io was "possibly less than 10 million years" old.[1] Then, through the luck of catching some volcanoes in action, it was realized that the surface "was changing even as Voyager I looked on."[2] Likewise, it appears likely that the ages of other bodies in the solar system have been incorrectly derived. The numerous tests indicating otherwise are based on an assumption of the constancy of one or more conditions that would have been different under the catastrophic situation Velikovsky posits. In that situation the atomic clocks would be reset.[3]

I am aware, of course, of the book Scientists Confront Velikovsky to which Morrison and Goldsmith refer and which was the subject of my last footnote. However, it is beyond the scope

of my paper to assess the particular merits of this point or that. This has already been done in Kronos in its special issue, "Scientists Confront Scientists Who Confront Velikovsky," and by a booklet by Shane Mage, Velikovsky and His Critics.[4] However, it remains my impression that what fundamentally troubles this school of critics is their belief that religion motivates Velikovsky's work. Sagan calls it an "attempt to rescue old-time religion."[5] Martin Gardner calls him "a devout believer in orthodox Judaism."[6] In this area we are not dealing with an interpretation, a philosophy, or a way of assessing the evidence. It is either a fact or not. And it is not a fact. Velikovsky is not a creationist. From his perspective, while the catastrophism of the creationists is preferable to the uniformitarianism of the skeptics, the naturalism of the skeptics is also preferable to the supernaturalism of the creationists; ironically, each side is closer to the truth in the area of its least expertise.

Morrison notes my citations to the pro-Velikovsky literature without taking into account that Velikovsky and his followers for the most part have been barred from the standard journals, a situation in which there is the beginning of a change. But since he asks, I refer him to Peter Warlow's article published in the Journal of Physics entitled, "Geomagnetic Reversals?" This brings together a host of geologic phenomena making them coherent in the context of a reversal of the Earth's geographic poles.[7]

Goldsmith and Morrison also question my discussion on electromagnetism in the cosmos. Goldsmith's observation that every physicist he knows continues to veiw Velikovsky's scenario as impossible is not a compelling one. This could be a result of "group-think." How many physicists have assessed the matter from the standpoint of the issues I raised? Morrison visualizes a kind of "shoot-out at OK Corral" between electromagnetism and gravitation, with the former hopelessly outgunned.

It would be well to remember I was addressing only one point in the discussion: the possibility of such a scenario. An extremely weak case establishes possibility; a much stronger one is needed for probability. As for the latter, Velikovsky recognizes the problems and will have more to say about them in his forthcoming book, Before the Day Breaks. In the meantime it would be well to avoid the pitfalls of dogmatism. The two forces may be related in a number of ways other than through one against one opposition. Though electromagnetism may be understood as well as gravitation, it does not follow that either is understood very well. Melvin Cook's conception recognized by Malcolm Lowery deserves more attention. And there may be other approaches as well. Has the solar system been thoroughly explored?

Michael Jones wrote the longest and most detailed critique. One might gather from reading it that Velikovsky had not dealt with most of the issues he raised. Yet implicitly and sometimes explicitly Jones is answered point for point before he takes up

his pen. The basic difficulty is that Jones seems unprepared to to consider any other approach than the one to which he is accustomed. And he writes with a chip on his shoulder. His receptivity to John Dayton's Minerals, Metals, Glazing & Man which was mentioned by Leroy Ellenberger would be a good test of his closing prescription to assess "all the data available for study in whatever form it occurs."

Here are a few points that should be answered:

Hamza's views on the tiles were presented and Velikovsky asks, "How does it happen that all the multitudinous hieratic signs were left out and only those seeming to resemble Greek letters were profusely used?"[8] As for the designation of Persians, Velikovsky points out that the P-r-s-tt are referred to in the Canopus Decree, thus clinching the identification with Persia.[9] As for the discrepancy between the beards of the Persians and the clean-shaven Peleset, Velikovsky notes that a reform had been introduced requiring the soldiers to shave their beards so as to deprive the foe of an easy hold.[10] Jones's assertion that Ramses III's temple is unlike those of the Ptolemaic Age runs counter to the views of numerous independent observers.[11] Velikovsky also did not fail to mention the temple at Philae, as Jones asserts, but places it in the fifth century.[12]

I will leave to others the evaluation of the alleged hieroglyphic mistakes. But I don't really expect the verdict to be unfavorable. Velikovsky has in these two books presented such an array of evidence from all sides that Jones's lengthy critique appears as a preconceived quibble. The evidence accumulates. While the facts from any one line of reasoning may be persuasive, the cumulative effect from so many is devastating.

In Ramses II and His Time Velikovsky did not say there were no "extant Egyptian tests" from the 26th Dynasty, but rather that the "true activities" of these rulers are conveyed from Biblical and Classical sources. Additional work employing the private archives from that period is certainly in order, but Jones has provided no reason to believe that Velikovsky is fundamentally in error from the smattering he gives from this field. Although the name was fairly common at the time, the Pharoah Horemheb has no links with the 18th Dynasty, as Jones assumes.[13] Thus the workmen who began under his reign could have had normal lifetimes. Many of the questions Jones asks--"How is it that the Viceroy of Nubia, Paser, appointed by Horemheb is followed in office by his son two hundred years later under Sethi I?"--result from Jones's imputation to Velikovsky of a mistaken date for Horemheb. A careful reading of Peoples of the Sea would have prevented the mistake.[14]

Likewise, Jones writes with assurance on a much debated topic, the co-regency between Sethi I and Ramses II. Jones's statement that the Munich statue settles the question of the

reign lengths of the two is not supported by other competent authorities. Concerning Bakenkhons's son and successor as High Priest, Roma Roy, it is not certain that he was inducted by Ramses II, as stated by Jones. As noted by Murnane, "... we do not know who was king when this last advancement occurred. This being the case, the data derived from the Munich Glyptothek statue of the high priest Bekenkhons (Rome-Roy's predecessor) loses the significance it is presumed to have..." and further "...Rome-Roy's accession to the top ranking post in the Amun hierarchy seems not to have occurred in Ramesses II's reign...."[15]

When we come to Lowery, we meet an entirely different mind-set from the commentators thus far discussed. Lowery might agree with some of Jones's specific points but what a different attitude Lowery takes towards the matter! To him Velikovsky might be wrong on a number of questions without one or two errors invalidating the whole reconstruction. Both Lowery and Huber, for example, question the 360 day year; to the latter this is just another reason for wholesale dismissal; to the former, the point would not shake the general theory. Lowery's approach is certainly preferable between the two.

The real problem with critics is that Velikovsky's work places an enormous strain on human nature. For one thing it takes a long time and an awful lot of work to learn what it is. Only an extraordinarily patient person will work his way through to an understanding. And yet until the theory is understood, it cannot be properly assessed. I am trying to "blow the whistle" on premature evaluations on the basis of intuitive judgements. To date I have yet to read a negative appraisal of Velikovsky's work in which the writer convinced me that he understood the system he was criticizing.

Huber asks why a specialist should refute Velikovsky's assertion that Hattusili III and Nebuchadnezzar II are one and the same person. I would not ask a specialist to refute it. I would ask him to consider it, to list all the evidence pro and con, to weigh the possibilities and finally, to arrive at a judgment that he can defend before the world. In the battle for public opinion, the "trust us" argument relied upon by specialists raises a red flag. The public is more inclined to trust the experts when the experts do not rely upon authority, but lay out their reasoning for all to see.

II

I am very much concerned that other systems worthy of intense study and evaluation may receive even less attention than the one I have been discussing. For this reason I laid down two preliminary tests (Is it coherent? and Does it make a prima facie case?) which could serve to reverse the presumption against unorthodox theories at least temporarily. Otherwise, new theories will be ignored by the prestige-oriented, rather than truth oriented, institutions

of scientific discourse.

Leroy Ellenberger's comments elaborate in fine form some of my themes and deserve to stand alone. I will confess that he is quite correct in his perception that one of the principal targets of my remarks was Michael Polanyi's proposal that scientists in a discipline should ascertain the "plausibility" of a theory and then other scientists outside the discipline should close ranks and blindly follow their lead. Besides taking professional courtesy to an absurd length, I cannot conceive of a standard more likely to stifle the progress of knowledge. It is not a question of banning the term from the language; if a theory is rejected for reasons which can be specified in detail, one may loosely say that it was "implausible," but the real cause for the action was because it failed some objective tests. However, Polanyi seems to be encouraging scientists to make this most important, initial screening decision rather casually. They need no urging as it is. My contention is that until some objective criteria, which are hopefully simple (no nine point programs and 20 point procedures), somehow get assimilated into the sacred "scientific method," the future treatment of novel suggestions will repeat the past---pre-conception will govern plausibility.

Of course, even so, it is another question whether the two simple standards I suggested would actually be as useful in practice as I would hope. Dolby sees an inconsistency between the two in operation and McAulay doubts that even if adopted somehow, they would make much difference. Perhaps one point needs to be emphasized: we are speaking here of the right to reject a theory without extensive investigation, not the imperative to "take seriously." Not only are these two entirely separate things, there is a vast gulf between the two. I avoided this phrase, "taking seriously" which is common in scientific parlance because of its gross imprecision. Procedures can be prescribed; attitudes cannot and should not be.

This removes, I hope, the apparent internal inconsistency of my own approach that Dolby perceptively detected. I do indeed take seriously continental drift, and if I were a teacher of geology, I would be spending a good deal of time explaining it, but without the presumption of truth and with the still unresolved difficulties and alternatives also presented. At the same time I would also teach it without the presumption of falseness, especially since it is debatable how serious the conflict is with Velikovsky's catastrophism. One must be on his toes constantly to search out and weigh the evidence.

McAulay's point that different people who have <u>properly</u> scrutinized a theory will come up with different assessments is, of course, quite right, but the success of my strategy does not depend upon unanimity. I am principally concerned about insuring a flow of communication during that crucial first stage when a new approach is struggling for attention. At that point, when there is least understanding, the important decision about how much attention to focus seldom relates to the merits of the theory,

but rather to the prestige or political clout of the proposers. Although I have no idea what the outcome of attempts to open up the reception system of science will be, or whether any will be made, I am convinced that reforms along these lines are achievable.

And reform is necessary as McAulay illustrated so well in the article he referred to in his comments, "Velikovsky and the Infrastructure of Science," Theory and Society 6 (3), 1978. Here he demonstrated conclusively that ideological motivations of a religious-philosophical sort have determined the response of a portion of the scientific community to date. It is extremely naive to think that the free marketplace of ideas will operate in a case where the dominant ideological sensibilities of a community are offended and entrance into the market is denied. On the other hand, a coverup of the actual prejudice is more difficult to pull off when one is required at the outset to place on the record the specific reasons for rejection.

My concern about this goes well beyond the Velikovsky case. Being realistic I have to allow for the possibility that my thinking may be influenced by a personal friendship. If, in the course of study, I should be compelled to alter my view of the Velikovsky case, it would not affect my conviction that science should reform its approach to novel ideas. To illustrate, take the situation regarding D. B. Larson's Reciprocal System of physics,[17] a theory to which I have no emotional ties and certainly do not accept or reject. Yet how many readers of The Zetetic Scholar have even heard of this system? How many could accurately summarize its postulates on a sheet of paper? How many could show where the theory lacked coherence or where the facts do not support it or where they would not even if his definitions were granted for the sake of the argument? Larson began publishing his books in the 1950's, but there has been no extended analysis or even reference to them in the standard journals of physics in the USA and only a few references and book reviews in the journals in Britain, Germany, and Australia, but nothing extensive even so.[18] Given the evident sophistication of his system and the degree to which qualified professionals have been attracted to it, this silence is a scandal. It calls for something more than rushing off a preconceived article or volume of essays in order to relieve the embarrassment of the situation.

Coming to the end, what does this all suggest? Here I am tempted to begin a new paper---on the need for procedural reforms to insure due process for ideas in science. That day will come when there is greater appreciation of its necessity.

NOTES

[1]Science News, March 10, 1979.

[2]Science News, March 17, 1979.

[3]See Ralph Juergens, "Radiohaloes and Earth History," Kronos, Vol. III, Nr. 1, 3-13.

[4]Velikovsky and Establishment Science, Kronos, Vol. III, Nr. 2; Scientists Confront Scientists Who Confront Velikovsky, Kronos, Vol. IV, Nr. 2; Shane Mage, Velikovsky and His Critics (11560 128th Ave., Grand Haven, Michigan 49417: Cornelius Press), 1978.

[5]Donald Goldsmith (ed.) Scientists Confront Velikovsky (Ithaca, New York: Cornell University Press), 1977, 93.

[6]New York Review of Books, June 14, 1979.

[7]Peter Warlow, "Geomagnetic Reversals?" Journal of Physics A, Math. & Gen., Vol. 11, Nr. 10, October 1978, 2107-2130. Reprinted in the SIS Review, Vol. III, Nr. 4, Spring, 1979.

[8]Immanuel Velikovsky, Peoples of the Sea (Garden City, New York: Doubleday), 1977, 11.

[9]Ibid., 35.

[10]Ibid., 33.

[11]For more parallels see Lewis M. Greenberg, "Peoples of the Sea: An Art Historical Perspective," Kronos, Vol. II, Nr. 4, 77-88.

[12]Peoples of the Sea, 76.

[13]Immanuel Velikovsky, "The Correct Placement of Haremhab in Egyptian History," Kronos, Vol. IV, Nr. 3, 3-22.

[14]Peoples of the Sea, 194.

[15]William J. Murnane, "The Earlier Reign of Ramesses II and His Coregency with Sety I," Journal of Near Eastern Studies, Vol. 34, Nr. 3 (July, 1975), 189.

[16]Unfortunately, this is the result of Huber's paper, "Early Cuneiform Evidence for the Existence of the Planet Venus," which was reprinted in Scientists Confront Velikovsky. A convincing answer to Huber's arguments can be found in Lynn E. Rose, "'Just Plainly Wrong': A Critique of Peter Huber," Kronos, Vol. III, Nr. 2 and Vol. IV, Nr. 2.

[17]Dewey B. Larson, The Structure of the Physical Universe (1959), The Case Against the Nuclear Atom (1962), Beyond Newton (1964), New Light on Space and Time (1965), Quasars and Pulsars (1971), all published by North Pacific Publishers, Portland. Oregon.

A simplified introduction to Larson's theory can be found in Ronald W. Satz, The Unmysterious Universe and can be obtained by sending $2.60 to the author at Transpower Corporation, 4518 Cedar Lake Road, St. Louis Park, Minnesota 55416.

[18]Frank H. Meyer to Joseph May, February 9, 1979.

The proponents of Larson's theory have formed an organization named New Science Advocates, Inc. For information contact F. H. Meyer, 146 Ross Hall, University of Wisconsin, Superior, Wisconsin 54880.

ZS Dialogues

REJOINDER FROM JOSEPH AGASSI TO GEOFFREY DEAN AND ARTHUR MATHER (ZS, # 3/4):

Thank you for your kind invitation to respond to the response of Geoffrey Dean and Arthur Mather, the authors of Recent Advances in Natal Astrology, to my review of their book. I hesitate. My review was too long, and, as they say, goes off in a tangent to discuss the nature of superstition. My response cannot be longer than theirs since they summarily dismiss me as they think I dismiss them. And perhaps they are right. I therefore do not know whether to respond; but I leave the decision to you since I have no judgement: if you think it advisable you may publish this letter.

It is a second time I leave the decision to you--not because I am an undecided fellow but because we are in an area where standards are unclear, and not only to me, but generally so. When I spend a few days with a book, judge it necessary to spend many more days on it in order to be able to be in full control over all of its technicalities, but unwilling to spend that time on that task, then I have the choice between refusing to review it and simply explaining my attitude to the readers of my review. In this case I could not simply explain; I therefore had to leave the decision in your hands. I am in the same predicament now.

The reason in this case I could not either simply explain or simply decide is, to repeat, the very absence of agreed standards. Opponents think every minute spent on astrology is wasted. I disagreed and explained why I benefitted from my study of Recent Advances in Natal Astrology. The adherents to the authors' attitude demands more time expenditure than I am willing to invest. Hence, judging this very matter amounts to prejudging the matter at hand.

I will say a bit more, with your kind permission (otherwise do omit this paragraph). You as an editor of a journal dedicated to the boundary between the paranormal and science, have this problem constantly on your hands, and I am fascinated by you precisely because I cannot say whether or not you are wasting your time, as I think astrologers do. And my question was whether the authors are wasting their time and their readers', and I ended up in the affirmative. This prevented me from spending more time on the book than I had: when I learned why the book had fascinated me so, I felt the need to spend time on it satisfied. I hope when you have enough of editing the Zetetic Scholar you will pass the editorship to a person who might still find it useful and interesting, or else close it down.

If I am still there and talking, I would now take up the authors' responses one by one in the order they are presented.

I reject astrology out of hand, they say, as a superstitious nonsense, thereby violating your journal's policy of fairness and

objectivity. Now fairness and objectivity do not decide the issue, especially since I never used the word "nonsense" but tried to see what the current claim that astrology is superstitious amounts to, beyond the claim that it is silly.

I admit, the authors report, not having read the book properly. Not so: I admit having spent a few days on it yet not enough to be as technically proficient in all the matters it presents as I am of most books I review. What exactly is proper in this situation I do not know. They conclude that my comments on what the book does or does not contain are worthless. The conclusion may be true, but it does not follow, as I can prove with the aid of the standard logician's tool: a counter-example. Here it is. I once read the ancient Eyptian Book of the Dead, and I know it requires years of study before one can be proficient in it; yet I can comment on it to say, what anyone can guess anyway, that for the purpose it was written for, it is these days useless. I say the same of astrology. I am a bit more proficient in astrology than in ancient Egyptian necrology, but then my proficiency for that purpose is somewhat excessive, to my mind.

The authors briefly cite as examples a brief list--five--of mistakes in astrology which I have committed in my review.

I state erroneously that signs do not divide the sky equally. Of course, if the sky is arbitrarily divided into equal twelve parts with, say, the sky right over Greenwich on next New Year's Eve put as zero, then that is that. But the signs house the constellations, or else they are nothing. And then they have to be juggled to be made equal, which was my point: I was discussing juggling.

They say I give constellations an importance they have not held for millenia. (At most centuries, really; but never mind.) I ask, why should the signs house the constellations, then? Why not divide the sky in total disregard for them? But how exactly? Why? The arbitrariness of it all was my target of criticism.

I am ignorant of modern attitudes towards precession, they say. True. How can an astrologer develop an attitude, let alone attitudes, toward precession, which is secular and thus can hardly influence natal astrology, which, being natal, concerns at most individual lifespans? The book's index, whose use the book's introduction expressly recommends, has the heading "Precession, known before ancient Egyptian time"; it refers to the test where the word "probably" qualifies the wild claim; the text refers to prehistoric stone circles and to a popular book that fully accords with the book's introduction's admission, "much of the work cited is merely opinion." So much for the authors on modern attitudes--in the plural--to precession, of which I am guiltily ignorant. In passing I should add, of all the wonderful things people have read into stone circles, the precise ones were all based on carbon dating, yet they are almost all defunct because of the recent carbon dating revolution. But no matter. The superstitious will use the latest dating techniques and the best astronomical computations to read some possible meaning into Stonehenge. And probably they will have some measure of success.

I imply erroneously, the authors continue the short list of my sins, that there is more than one ecliptic. Erroneously? Does not every planet have one? Have all but one lost theirs? I was, may I repeat, discussing the havoc Copernicanism caused to traditional astrology. Should we not study the love-life of a soldier by the time Venus appears on Mars? At the ecliptic; where else? The introduction of the ecliptic was a subterfuge from the very start.

The authors claim, finally, that I failed to recognize that they were merely reviewing the astrological literature, not generating a set of claims susceptible to refutations. I am dumbfounded. How could I fail to recognize the brief datum of my own essay? Was I writing it in my own sleep? I said, the new stance of the superstitious in the age of science is the same as the new stance of the dogmatic in the age of science. A Catholic, a Jew, a Marxist, a logical positivist, wants merely--I like the word "merely"; spend your intellectual life (like our fearless authors) merely--to see the literature on his concern reviewed. It is all right if a review comes up negative, provided it is thorough enough to make the reviewers spend enough time on it to enable the faithful to generate more literature for them to merely review. Merely. This way the business can merely go on. (The business of evading life!) My question was, is the game worth the candle? My request was that authors tell their readers why they should take the trouble. Our authors say, and this is their parting shot, the amount of work put into astrology worldwide today surely justifies the much smaller effort spent on assessing the underlying assumptions of astrology. I deny this, the authors' "surely" not withstanding. Surely, rather, there are no underlying assumptions stated clearly anywhere in this very huge volume that takes so long to study. Surely, likewise, the authors do not strictly confine themselves to a review of the literature but also express hopes--some of which I have quoted--for a true future astrology. This hope, I say, is utterly silly; yet is is this hope which is the defining characteristic of their activity and their field as superstitious rather than dogmatic. Catholicism and Judaism and Marxism and logical positivism have much clearer underlying assumptions, are much less elusive, and therefore better qualify as dogmas than as superstitions. (This is not to deny that a Catholic or a Marxist may reject official dogma yet stay; being dogmatic, he would do so in order to avoid being dogmatic; and he would be successful at that. Except that he would thereby become superstitious. This claim is empirical.)

My aim was to demarcate criticism, valid or invalid, that is worthwhile. My aim was also to understand what is superstition, to demarcate it better from both pseudo-science and dogma than I did earlier. [Strangely, I accept Lynn Lindholm's view that, historically, modern science in its inception was superstitious (rather than pseudo or dogmatic or naive).]

Popper identifies rationality with criticism and science with empirical criticism. He lumps together metaphysics, pseudo-science, dogma, and superstition. My concern was to criticize him and to offer a better theory than his. The volume at hand offered both an occasion and assistance in performing a part of this task. Though the

authors are naturally peeved by my review on account of its calling them superstitious and silly, I am rather grateful to them for the occasion they gave me.

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REPLY TO PROFESSOR AGASSI FROM GEOFREY DEAN AND ARTHUR MATHER:

Prof. Agassi's response has confirmed his disbelief in astrology but has contributed nothing to the basic issue, namely the factual evidence for astrological beliefs. Here are his main points together with our response:

1. Astrology is superstition; superstition is worthless; therefore astrology is worthless. However he defines astrology as superstition a priori and is therefore guilty of prejudgement. But then he does say that "fairness and objectivity do not decide the issue." Unfortunately he does not tell us what does.

   Furthermore his own acceptance of early science as superstitious is incompatible with superstition being necessarily worthless. To have condemned early science would have been mistaken, yet it was open to all the charges that Agassi levels at astrology. Hence the facts alone can decide the issue: are astrological beliefs actually true? Only if they are not is Agassi's conclusion justified.

2. Astrology is unsupportable on technical grounds because it contains inconsistencies. It is true that it contains inconsistencies, and different schools resolve them by opting for one side or the other (a situation not uncommon in science). In effect Agassi is complaining that we have not eliminated them. But this is the task of research; a critical review merely examines the evidence. We fully agree that the inconsistencies deserve attention, and hopefully by the time our next edition is ready there will be sufficient evidence for their resolution. (The next edition is currently in preparation and we invite the participation of all interested ZS readers.)

   Agassi seems to imply that astrology is impossible to investigate without getting hopelessly bogged. But our review shows how a methodical examination of the facts can greatly clarify the issues involved. Many more studies are needed but with modern computing facilities the whole question may well be disposed of, or more profitable areas of enquiry defined, within a few years.

   We are neither for nor against astrology. In our view the massive worldwide interest in astrology is justification in itself for assessing the underlying beliefs, which are simply that planetary configurations relate to human character and affairs. Astrology may or may not be as worthless as Agassi suggests, but we will not find out by refusing to study it.

ZS Dialogues

C. LEROY ELLENBERGER COMMENTS ON THE ZS DIALOGUE (ZS, #3/4/) ON THE THEORIES OF IMMANUEL VELIKOVSKY:

We're concentrating on how astronomy--the cosmos--relates to human beings...in terms of the history of life on earth having been determined by cosmic events, and how our philosophies and myths are in many ways tied to astronomical themes.

Carl Sagan[1]

Is the universe Gödelian in the sense that there is no end to the discovery of its laws? Perhaps. It may be that no matter how deeply science probes there will always be laws uncaptured by the theories, an endless sequence of wheels within wheels.

Martin Gardner[2]

These quotes provide an interesting point of departure from which to discuss the reaction to Joseph May's "The Heresy of a New Synthesis." It is ironic, given their outspoken antipathy towards Velikovsky, that the quotes above show Sagan and Gardner expressing, however unwittingly, patently Velikovskian positions. Sagan appears to be expropriating the very theme of Worlds in Collision while Gardner appears to allow a rationale for admitting to scientific discussion the supposedly "impossible" events in Worlds in Collision. These statements illustrate how readily expressible a Velikovskian position can be in a neutral or non-specific context.

The point is that if the evidence in Worlds in Collision shows that it happened (and many objective readers have been persuaded that some cosmic catastrophe devastated all civilizations in the middle of the second millennium B.C.), then all the so-called "laws" would be irrelevant and the intelligent course of action would be to attain the next wheel of deeper understanding. Hamlet's observation that "There are more things in heaven and earth, Horatio, than are dreamt of in our philosophy" preserves a worthwhile caveat elaborated by Rose and Vaughan.[3] Naturally, once it is admitted that we do not know all the laws of nature, Velikovsky cannot logically be accused of violating them.

After reflecting upon the Velikovsky article in the April issue, I offer the following remarks with special attention to Morrison, Jones, Huber and Goldsmith. As conceived, the dialogue on Velikovsky was a meritorious undertaking. However, round one appears to have undershot the objective substantially. Commentators such as Morrison and Goldsmith have shown themselves not up to the task of addressing May's arguments at the requisite level. Rejection without refutation

satisfies neither the canons of exposition nor the objectives of The Zetetic Scholar. The dialogue promised more than bald assertion and statements to the effect: "Velikovsky is wrong; read our book which proves it." Despite May's partisanship, he did offer pro and contra evidence (e.g., see footnote 35), a balance lacking among the critics.

From their remarks, Morrison and Goldsmith appear not to be aware that May's last footnote discussed their book. Thus, Morrison is wrong when he accused May of "ignoring responsible criticism," (presumably Scientists Confront Velikovsky). May also cited the Kronos book, Velikovsky and Establishment Science, which besides being "A detailed analysis of their effort" is no less than a full-scale rejoinder. Since both of them have seen the Kronos book, their not mentioning it raises grave questions about the ethics of their participation.

Because Velikovsky never claimed to have all the answers, no meaningful dialogue will exist until critics address themselves to the secondary literature in Pensee, Kronos and S.I.S. Review. May's footnotes 6,16,30,31, and 38 cited all but one of the key articles in the secondary literature up to 1978 supporting the scientific merit of Worlds in Collision. To that list should be added Juergens' "Radiohalos and Earth History."[4] Not one critic addressed any of this material, much less countered any of V&ES. This failure undercuts the spirit of true dialogue.

Tellingly, May wrote "As a rule, critiques of the scientific aspects have rested on uniformitarian assumptions, so that the negative appraisals amount to an exercise in circular reasoning. There is yet to appear a refutation that does not fundamentally beg the question, that comes to grips with the argument as a whole and under its own terms" (p.40). After the 1974 A.A.A.S. symposium, the editor of Pensee made the same observation.[5] Unfortunately, what was true in 1974 is just as true now. Not one of the critics in the Zetetic Scholar even began to make the effort. This failure, in itself, is presumptive of an inability of the critics to deal with Velikovsky's thesis.

Morrison is a particularly ill-suited commentator because of his predisposition towards Velikovsky expressed in 1972: "...the simple truth is that Velikovsky is not worth discussing scientifically."[6] He evidently still believes this because he did not comment on May's article as would a scientist searching for truth, but as a debunker. His comments are as dogmatic as any of those made by the Aristotelians at the University of Padua who "refuted" Galileo. Interestingly, I have had the pleasure of discussing Worlds in Collision with several scientists who, while not accepting it, can discuss its dynamical aspects evenhandedly without the air of condescension.

Thus, Morrison views the subject merely as a study of the "response of organized scholarship to an outside critic." He does not appear to comprehend the significance of May's observation in the opening of the last footnote that "preconceived ideas can skewer the judgmental processes." This idea is not new. Lippman once

remarked: "We do not first see then define. We define first, then see."[7]

The stuff of science and the results of space missions are rooted in interpretation of necessarily selected and often ambiguous data.[8] That NASA scientists do not report explicit corroboration for Velikovsky is by no means indicative of its absence. Mainstream scientists tend not to be looking for Velikovskian interpretations of the data. Even when an opportunity hits them between the eyes, they seem to entertain every conceivable alternative except a Velikovskian one.[9]

With his mind-set, Morrison is thus prone to dogmatic statements as when he writes that Velikovsky's ideas "contradict by many orders of magnitude all we know about geological time scales." All we know of anything is a product of our expectations and receptivity. The time scales he reveres, inexorably rendered by uniformitarian assumptions, are not sacrosanct. For example, if the Earth gets inundated by radiation from a supernova about every fifty million years, as many scientists believe, simple calculations show that (n, gamma) reactions would "wipe out geologic time."[10] Further, Juergens has suggested that if electrically charged bodies at differing potentials passed close enough for their space-charge sheaths to touch, the resulting electrical discharges would greatly accelerate radio isotope decay, thereby resetting atomic clocks.[11] Whatever the true age of the Earth, the ability of current techniques to reveal it is by no means certain. Other of Morrison's uniformitarian assumptions are examined by Juergens in Kronos.[12]

Morrison's comments on celestial mechanics were adequately answered in advance by Lowery's fifth paragraph which summarized Bass' comments at Glasgow in 1978. Bass believes that Cook's theory of gravity provides the framework, another wheel so to speak, within which the orbital changes of Worlds in Collision can be explained.

The disingenuousness of Morrison's criticism that May's citations "are almost wholly to other pro-Velikovsky literature" is incredible. After the Establishment labelled Velikovsky a "charlatan" and "pseudo-scientist" and barred serious discussion of his ideas from the scientific journals, now Velikovsky and his supporters are criticized for not being able to cite the standard literature for discussion of Velikovsky's ideas. That can only be called cheek. If discussion of Velikovsky's ideas had been welcomed in the standard journals, perhaps the articles by non-supporters would be more respectable, not the out-and-out hatchet jobs they usually are.

Morrison exhibits a guild mentality, not to mention arrogance, when he alleges the pro-Velikovsky literature is written "largely by those who are not competent to set themselves up as judges of these unfortunately rather technical fields." However, he neglects to specify what criteria he uses for this judgment. Presumably he is playing the disciplinary academic degree game reminiscent of the medieval guilds. The fallacy of this conceit lies in the fact that no recognized discipline was ever founded by someone with a degree in it. However, his charge loses its sting because Morrison does not even deign to address the writings of such as Bass whose credentials cannot be faulted.[13] This matter of competence can be used both ways.

Does the fact that in SCV (p. 161) Morrison did not report the correct solution to the differential equations for conduction and radiation in series cast doubt on the validity of his entire paper?[14]

It is ironic that some who should be competent to judge "these unfortunately rather technical fields" expose either their true incompetence despite their degrees, or their true disregard for honest scientific discussion. For example, when Sagan purports to show in the Appendix 3 to his criticism of Worlds in Collision[15] that in 3500 years Venus would cool by radiation from 6000K to 79K, he is egregiously wrong. All his computation shows is the identity that the heat radiated in 50 minutes at 6000K equals the heat radiated in 3500 years at 79K.[16] By no stretch of the imagination is Sagan's computation meaningful to the discussion. He ignores the essential parameters of mass, surface area and specific heat of Venus. Interestingly enough, a correct cooling computation has shown that Venus, in fact, would cool to approximately her observed temperature of 750K. This result is strong presumptive thermodynamic evidence for the validity of Velikovsky's history of Venus.[17]

It is a truism in Operations Research that seminal advances tend to be made by people whose academic preparation was outside the field of the breakthrough. Specialists tend to become defenders of knowledge rather than its extenders. Interestingly, in support of these conditions is the recent discovery of the link between vitamin B-6 and arteriosclerosis by two neurophysiologists in the Research Laboratory of Electronics at M.I.T. Almost predictably, this discovery is adamantly opposed by the director of the Arteriosclerosis Center at M.I.T. who is an advocate of the cholesterol theory.[18]

The advancement of knowledge is too important to become mired in peer review and inter-disciplinary politics. Morrison would be wise to keep in mind that degree programs have probably smothered more genius than they have created. He would also be wise to remember that in 1974 the A.A.A.S. relied upon a statistician (who was also a self-proclaimed hobby Assyriologist) to debunk Velikovsky's interpretation of cuneiform evidence, Peter Huber.[19] Does Morrison believe that Huber is incompetent to judge such highly technical matters outside his area of formal training?

From Jones' opening sentence, he shows he has not the slightest understanding of how powerful are "vested interests and ego involvement." Their power was demonstrated by Graff and Bennett in my initial comments and is confirmed by Shapley's behavior in 1946 and 1950.[20] Jones' confidence in the objectivity of disciplinary scholars and their ability "constantly to modify their views" with the "appearance of new evidence" merely reveals his naivete. His position is easily contraindicated by my original Batson reference. Max Planck remarked sadly, "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents die, and a new generation grows up that is familiar with it."[21] No less an institution than the British Museum has been shown to have suppressed radio-carbon dates that do not support the conventional chronology of ancient Egypt.[22] "No-one, however eminent, can appear a fool" so long as serious and significant

challenges to the status quo are ignored and barred from the archival journals.

A good test of Jones' position would be for him to review the new book by John Dayton[23] and get that review published in an archaeology journal. Comments on Jones' detailed remarks I leave to May and other specialists, both here and in the pages of Kronos and S.I.S. Review. Just note that Dayton's independent refutation of the orthodox chronology using metallurgical data is a very strong indication that Velikovsky is essentially correct in his revised chronology. In a footnote to his book, Dayton applies to Velikovsky's work the motto he claims for his own: "It is better to be roughly right than precisely wrong."

For both brevity and audacity, no one exceeded Huber. With Rose[24] having fully exposed Huber's own abuse of the cuneiform material, (to which Huber has never replied), accusing Velikovskians of maltreating their sources is a certifiable case of chutzpah. More than denial will be required for Huber to refute the former existence of a 360 day year. "The Canopus Decree states that at some period in the past the Egyptian year was only 360 days long, and that the five days were added at some later date; the Ebers Papyrus shows that under the Eighteenth Dynasty the calendar had a year of 360 days divided into twelve months of thirty days each..." with no intercalations.[25] These two documents make it clear the 360 day year was real, no simplification or manifestation of a primitive mentality as Sagan would have it.[26] Where is Huber's counterevidence?

Goldsmith states he does not know any physicist who takes Velikovsky seriously. The scope of his reading is evidently very narrow because Peter Warlow, a British physicist, recently published an article "Geomagnetic Reversals?" that is very supportive of Velikovsky's ideas.[27] Warlow explains how the close passage near Earth of a large cosmic body could flip the Earth upside down, producing geomagnetic reversals. Warlow cites evidence for several reversals in the last 40,000 years which contradicts Sagan's "reversals occur about every million years, and not in the last few thousand."[28] Such pole flipping would also produce the illusion of the Sun standing still, an alternative to rotational stasis originally mentioned by Velikovsky and essentially ignored by critics who delight in lampooning the idea of the Earth's rotation stopping and restarting. However, as Juergens suggested,[29] it is even theoretically possible for an electrically charged Earth almost to stop and then to speed up.

It is not to Goldsmith's credit that he thinks "we would be in awful trouble" if "once rejected theories will eventually be accepted." Does he not know, for instance, that authorities at one time rejected the first law of thermodynamics, Semmelweis' germ theory of infection and Mendel's genetics? The British navy knew the cause of scurvy 75 years before citrus fruit to supply vitamin C was required in the sailors' rations. Of course, these examples by no means ensure Velikovsky's vindication, but they certainly ought to leaven one's mind-set with a touch of humility. As a precurser to Gardner's quote above, Hackerman put it more forcefully: "For those to whom truth is an invariant, that is, something engraved in stone, it must be

unsettling to be told that even long-standing natural 'laws' are subject to alteration in the light of fuller understanding....it might be helpful to remind ourselves regularly of the sizable incompleteness of our understanding...of nature and the world around us."[30]

The performance so far of those opposed to Velikovsky's ideas lends credence to Willhelm's observation that "Any test of an unacceptable perspective, e.g., the instance of Velikovsky, is a charade precisely because there is no sincerity toward factual examination inasmuch as loyalty is to science that must survive as an autonomous enterprise."[31] Morrison's closing sentence is borrowed and altered to say that Velikovsky is contradicted only by the uniformitarian interpretation of the natural record, not the natural record itself. It remains to be seen whether or not mainstream scientists can sufficiently detach themselves from the prevailing Weltanschauung in order to entertain intelligently alternate hypotheses regarding the interpretation of data of all kinds.

NOTES AND REFERENCES:

[1] Science Digest, June 1979, p.38.

[2] Scientific American, July 1979, p.16

[3] L.E. Rose and R.C. Vaughan, "Velikovsky and the Sequence of Planetary Orbits" in Pensee Editors, Velikovsky Reconsidered (New York 1976) pp.110-132. The last two paragraphs merit quotation: "We close with a cautionary note regarding energy disposal, eccentricity damping, and electromagnetic processes in astronomy. These may indeed be consequences or ramifications of Velikovsky's theory, but at this point it is largely a matter of attitude whether one sees these as vulnerable points of the theory or as strong opportunities for discovery. An analogy could be drawn to the Copernican theory, which entailed that either there was a measurable stellar parallax or the stars were fantastically farther away than anyone had guessed. Since there was at that time no measurable parallax, some saw this "problem" as an insuperable barrier to the Copernican theory and rejected the theory as absurd. Others, like Giordano Bruno, saw clearly that the Copernican theory entailed an enormous universe, and Bruno concluded that the stars were so far away that they must be suns. The parallax "problem" which some saw as a reason for rejecting the Copernican theory, Bruno saw as the key to his own discoveries that the stars are suns and that those suns are probably centers of planetary systems of their own.

"Let us not suppose that our relative ignorance, at present, of the processes that damp eccentricities or dissipate energy or produce electromagnetic effects in the solar system shows that Velikovsky is wrong. Instead, let us study the consequences of Velikovsky's theory and engage in serious and systematic search for the information and the understanding that will enable us to evaluate the role of those processes toward which Velikovsky points. We do not stand before a wall: we stand before a door."

[4] Kronos III:1 (1977), pp.3-17.

[5]Pensee IVR VII, p.26.

[6]Physics Today, February 1972, pp.72-73.

[7]W. Lippman, Public Opinion (1922). Quoted in N.Y. Times Sunday Magazine, May 13, 1979.

[8]For example, see V.A. Firsoff, "On Some Problems of Venus," J. Brit. Astron. Assoc. 89, 1 (1978) pp.38-46. Firsoff explains how the apparent slow axial rotation of Venus might be a spurious conclusion derived from an erroneous interpretation of radar data.

[9]L.M. Greenberg, "Velikovsky and Venus: A Preliminary Report on the Pioneer Probes." Kronos IV:4 (1979) pp.1-12 (1-4). By all preconception, the higher than expected concentration of argon-36 discovered on Venus could only have meant that Venus was significantly younger than Earth and Mars. Yet, because this is unthinkable (and also coincidentally accords with Velikovsky's recent origin of Venus), several ad hoc theories were formulated to account for the excess argon-36. At a press conference, at the mention, Velikovsky was ridiculed by a scientist who admitted he had never read Worlds in Collision.

[10]M.A. Cook, Prehistory and Earth Models (Max Parrish, London 1966) p. 54.

[11]R.E. Juergens, op. cit. Note 4, pp.11-13.

[12]Kronos III:2 (1977) pp.113-120, Kronos IV:2 (1978) pp.70-79 and Kronos V:2 (1979) forthcoming.

[13]See May's original footnotes 30 and 31. Bass, a Rhodes Scholar and professor of physics and astronomy at Brigham Young University, took his PhD in mathematics at Johns Hopkins under the late Aurel Wintner--then one of the world s leading authorities on celestial mechanics--and studied celestial mechanics under Carl Ludwig Siegal, an acknowledged giant in the field.

[14]Morrison is flatly incorrect that the cooling rate for conduction of heat through a solid and its subsequent radiation to space is proportional to the sixth power of the absolute temperature. A naive solution to the relevant differential equations (which erroneously confuses surface temperature with interior temperature) would show that cooling time is proportional to the inverse sixth power while cooling rate is proportional to the seventh power. As the solution shown by M.N. Özisik (Boundary Value Problems of Heat Conduction (Scranton, PA 1968) pp. 319-323) indicates, no simple power rule relates cooling rate with temperature for this problem.

[15]Scientists Confront Velikovsky, pp.100-101; Broca's Brain, p.326.

[16]G.R. Talbott, Kronos IV:2 (1978) pp.15-16.

[17]Ibid., pp.9-14.

[18]Technology Review, June/July 1979, p.90. See also Atlantic May 1979, pp.59-65 for an extended description of the theory and its verification.

[19]The A.A.A.S. Program in 1974 (p.23) described Huber as a "Prof. of Ancient History" when, in fact, he is a professor of statistics with no formal training in ancient history or cuneiform.

[20]H.M. Kallen, "Shapley, Velikovsky, and the Scientific Spirit," Velikovsky Reconsidered, Editors of Pensee (New York 1976) pp.20-32. See also, C.L. Ellenberger, "Heretics, Dogmatists and Science's Reception of New Ideas," Kronos IV:4 (1979) pp.60-74.

[21]T.S. Kuhn, The Structure of Scientific Revolutions, 2nd. Ed. (Chicago: University of Chicago Press, 1962), p.151.

[22]"ASH," Pensee IVR VI, pp.5-19 and "ASH (Continued)," Kronos IV:2 (1978) pp.101-104. Reeds and seeds from Tutankhamen's tomb (a 14th century B.C. ruler as conventionally reckoned) were carbon dated to the 9th century B.C. in accord with Velikovsky's revised placement. These dates were set aside because they diverged so far from the expected dating and were never published. The amount of contamination from recent carbon can be calculated to be so large as to be visible. Therefore, if the samples had been contaminated as was alleged, they would not have been dated in the first place. Upon initial inquiry, the existence of the datings was denied.

[23]John Dayton, Minerals, Metals, Glazing and Man, London 1978. Reviewed by Peter James in S.I.S. Review III:4 (1979) pp.81-83. Dayton identifies key errors made by 19th century archaeologists which results in "a card-house of interrelated 'facts'...each system building upon the flimsy evidence of the past--often, indeed, on a single find of doubtful date and stratigraphy, as with the cylinder seal from Platanos of the Old Babylonian period." (p.6) Dayton observes that Petrie, "one of the pioneers of Egyptian archaeology and on whose work in the 1890s so much modern chronology rested, had had a fatal idee fixe of a high chronology and had, on scanty evidence and in the face of an XVIIIth Dynasty scarab, dated all the objects in the deposits of Abydos and Hierakonpolis to the Ist Dynasty or earlier, thus causing much of the archaeology of the Near East that has been built up on the evidence of the important objects in these deposits to be founded on a profound error." (p.6) The only reference to Velikovsky is in a footnote on p.444.

[24]L.E. Rose, "'Just Plainly Wrong': A Critique of Peter Huber," Kronos III:2 (1977) pp.102-112 and Kronos IV:2 (1978) pp.37-69. During the question and answer period at the A.A.A.S. Symposium in 1974, Huber said, "...we have intercalary months from documents which were written in the old Babylonian times, and I thought I made quite a fuss about the fact that seven intercalary months were recorded in contracts written in the time of Ammizaduga, and that these same intercalary months could be established from the Venus tablets." Rose points out that"there are actually eight or even nine attested intercalary months from the time of Ammizaduga, and only four of these clearly fit the months that would be required for a uniformitarian reading of the Ninsianna tablets. Further, there are three

months required for a uniformitarian reading of the tablets that are not attested from the time of Ammizaduga. Huber's claimed seven-for-seven fit is a fabrication." See also pp. 54-62 in Kronos IV:2.

[25] I. Velikovsky, Worlds in Collision (New York, 1950) "The Year of 360 Days" pp.330-359.

[26] Scientists Confront Velikovsky, pp.51-53; Broca's Brain, pp.39-90.

[27] Journal of Physics A, 11, No.10 (October 1978) pp.2107-2130. Reprinted in S.I.S. Review III:4 (1979) pp.100-112.

[28] Broca's Brain, p.102; similarly in Scientists Confront Velikovsky, p.66.

[29] R.E. Juergens, "On the Convection of Electric Charge by a Rotating Earth," Kronos II:3 (1977) pp.12-30.

[30] N. Hackerman, Science 183, 907 (8 March 1974).

[31] S.M. Willhelm, "The Velikovskian Upheaval: A Temporocentric Challenge," Kronos III:2 (1977) pp.49-61 (p.57).

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BOOK REVIEWS

Dowsing: The Psi Connection. By Francis Hitching. Garden City, New York: Anchor Books, 1978. 306 pages. $3.95 paperback.

Reviewed by Ray Hyman

Francis Hitching believes dowsing--the finding of water, minerals, ancient artefacts and almost anything else with the aid of the divining rod or pendulum--works, is important, and involves forces or aspects of human capacity that are revolutionary when finally recognized by orthodox science. As the co-author of the only skeptical book on dowsing (Vogt, E.Z., & Hyman, R. Water Witching U.S.A., University of Chicago Press, 1979, paper), I disagree with Hitching's beliefs about its validity. Surprisingly, I find that he and I do agree about many important issues about the topic.

We both agree that the rod or pendulum moves because of involuntary movements by the dowser. And we have no doubts that the trigger for these movements is controlled by the diviner's unconscious expectations (although we disagree about the source of the unconscious knowledge). Both of us assess the evidence for divining in the same way. We find that the data that has been collected according to scientific standards does not not support the claims of dowsing. The anecdotal data that fails to live up to scientific standards often does seem to offer striking examples of success.

At this point we part company. Vogt and I, taking into consideration the variety of ways uncontrolled human observation and experience can deceive us, conclude that dowsing's failure to pass scientific muster over a 500-year period, justifies the current indifference by scientists towards its claims. Hitching, firmly convinced of dowsing's omnipotent validity, implies that there must be something wrong with science and scientists. Because he knows that dowsing works, he seems to be asking that science grant dowsing a special dispensation from its regular standards. If dowsing cannot succeed by current scientific standards, then let's weaken the standards in such a way that it can succeed!

In many ways this is a "good" book. In my opinion it is the best book available that is written in behalf of the case of dowsing. Hitching not only writes well, but he has been encyclopedic and thorough in his coverage of all the major questions. It constitutes a complete handbook on the subject. He begins with a characterization of the phenomenon and its many applications. He includes short biographical sketches of a number of master dowsers in England and the U.S.A. The history of the practice is well-covered and various contemporary applications in warfare, criminal investigations, and other areas are illustrated.

The middle third of the book deals with an extensive account of attempts to provide an explanation of the phenomenon in terms of known scientific forces. An entire chapter is devoted to magnetism. If some of the experimental claims are to be believed, the human dowser can detect variations in magnetic fields with sensitivity many orders of magnitude greater than any known physical detector. It is not made clear how we can check on this if we have no independent way of measuring these minute variations. Another chapter considers the pros and cons of attributing the effects to various portions of the electromagnetic spectrum.

Hitching does a very good job of introducing the layman to the scientific ideas of magnetism and electromagnetic radiation. Even if Hitching is wrong on dowsing, his readers will gain a good scientific introduction to some of the major physical forces. The message from these chapters on physical forces is that humans may be directly sensitive to portions of the electromagnetic continuum and to magnetic fields sufficiently to account for alleged divining successes on site.

The paradox, which Hitching does little to try to resolve, is that, according to his accounts, dowsers can succeed just as well when divining over a map thousands of miles from the site. Such an ability, if real, would make all the accounts based on known scientific forces superfluous. Instead, we would require some sort of a paranormal account. And this is what Hitching devotes the last third of his book to, and it is also what gives the book its sub-title, "the psi-connection." One gets the impression that a small group of dowsers is valiantly trying to demystify the practice by providing evidence to show how physical forces could conceivably account for successes for on-site searches. But other dowsers seek an account in paranormal possibilities and look to psychic phenomena for their explanations.

Hitching reviews many contemporary events in parapsychology such as psychokinesis, remote viewing, and psychic detectives, and he attempts to fit them to current theoretical developments in quantum physics. The implication is that dowsing is a very practical and, in the hands of the master dowsers, a very reliable form of psi.

As an added bonus, or perhaps even as the showpiece of the book, Hitching devotes a chapter and an appendix to a description of his own intercontinental dowsing experiment. He obviously considers his experiment to be a milestone of sorts and characterizes it as "the most thorough and extended test of map-dowsing yet undertaken." Bill Lewis, a British dowser, picked sites from maps of parts of the Northeastern and Northcentral United States at which he felt that Hitching would discover ancient megaliths or burial sites. Hitching then visited the U.S.A. and checked out the sites

against the descriptions given by Lewis. He interprets the results as "conclusive and astonishing." By this, he means that Lewis's locations and descriptions were well beyond what could be expected by "Chance."

It is clear that a lot of time, effort and expense went into this experiment. But in spite of Hitching's optimistic pronouncements to the contrary, the results are scientifically meaningless. The tragedy is that given his design the results were foredoomed to be useless even before the data were collected. The number of elementary precautions that were overlooked in conducting this investigation were many. For one thing no meaningful attempt was made to take into consideration the knowledge about location of settlements that one can gain from a map (e.g., intersections of rivers would be more likely than places far from water, etc.). The success rate of Lewis depends crucially on the comparison with the control sites. These control sites were selected in such a way as to practically guarantee that they would be less likely to match their targets than would Lewis's choices. We are not told what instructions or motivations guided the selections of these control sites by John Stiles. But it was a serious flaw to present Stiles with the maps with Lewis's locations already marked upon them and then ask him to pick a random control site. For example, if Lewis always placed his choices at reasonable places on the maps, then, in trying to pick a control site in the vicinity of Lewis's choice, but not too close to it, Stiles would inevitably be forced to put most of his choices in non-optimal locations.

We are not told if Stiles was highly motivated to try and find control sites that would be most likely to contain "megaliths" or if he was simply instructed to pick a site at random (as implied by Hitching's account). We are told, however, that the dowser Lewis has had a long-standing interest in locating such burial sites and is an expert on them. But we are not told if Stiles has any knowledge or expertise in this area. It would seem to be important to compare Lewis's map-dowsing not with a random selection of sites, but with sites chosen from the same maps by experts in this aspect of archeology.

Hitching tells us that he is aware of the possibility of subjective bias in judging the goodness of fit between the sites and Lewis's and Stiles's descriptions. Yet here as well in other places in the book Hitching assumes that such awareness is sufficient to protect him and the readers from these biases. It is because such awareness is not adequate that scientists insist upon various safeguards such as double-blind procedures, reliability checks, etc. But no such safeguards were employed.

These and many other weaknesses that I have not described were

tragic blunders just because it would have been so easy to have avoided them in the initial planning of the study. I am sure that any Department of Psychology in a British university, just as would be the case in this country, has at least one staff member who teaches research methodology and who would have been only too happy to advise Hitching in advance about the flaws in his intended investigation and to suggest a variety of ways that he could have avoided them. The mistakes that Hitching made are just those that we teach studnets to avoid in their very first course in research methods.

Where should we place the blame for this ambitious miscarriage of the scientific method? In part, I suspect it has something to do with the way British and American educational systems deal with science. Hitching is obviously intelligent, cultured and motivated to produce arguments favorable to dowsing that will also be scientifically acceptable. But in several places in his book he betrays a muddled grasp of what scientific methods are and what they are designed to achieve. Let me illustrate with just one example.

In a revealing chapter on "Dowsing versus Science," he cites the book by Vogt and myself as one of "the two enquiries in the United States that have done most damage to official confidence in dowsing.." Hitching then mentions out arguments that there are many ways dowsers can deceive themselves into falsely believing they have succeeded. He goes on to say, "But this leads the two authors to the premise that dowsing never works, and in support of this they are ruthless in selecting, overwhelmingly, from the individual records of dowsers, cases where dowsing has failed, and ignoring times when they have spectacularly succeeded." In support of this last assertion, Hitching accuses us of ignoring the data on Henry Gross which the late novelist Kenneth Roberts "was meticulous in chronicling his development from traditional water-divining to more advanced techniques of map-dowsing for other substances..."

Just in this apparently simple accusation Hitching has confused so many issues, contradicted statements in other parts of his books, and misrepresented the issues in such a way that I hardly know where to begin with a rebuttal. In the first place, Hitching himself has already made it abundantly clear that the scientific evidence for dowsing is almost uniformly negative. What he labels as our "ruthless" selection of cases of failure is simply our attempt to survey every known scientific study available. If Hitching knows of scientific studies in favor of dowsing that we have overlooked, he should cite them. In our book we make it very clear that the anectdotal or non-scientific evidence, such as the stories Roberts tells about Gross, do sound impressive, especially to laypeople who do not realize the limitations of such accounts. Our describing a particular failure of Gross in detail was to show

that Roberts' absurd claim that Henry Gross was infallible and never made wrong diagnoses was easily refuted.

The point that Hitching does not seem to adequately grasp here as well as elsewhere is that scientific procedures in various domains of inquiry were devised to protect us from strong human tendencies towards self-deception. In our book, Vogt and I first document in detail how many of these tendencies operate in observation and testimony and the conduct of inadequately controlled investigations. And then we show how almost all the evidence in favor of dowsing comes from situations in which such tendencies for deception are likely to operative. When controls are instituted to remove the possibility of such deceptive factors, the findings indicate that the divining rod acts no better than chance.

Given these circumstances, our position is that it is up to the dowsers and their supporters to obtain scientifically acceptable data in favor of dowsing before they can expect scientists to take their claims seriously. Neither Vogt nor I accept the blame for the fact that dowsers have consistently failed to make a scientific case for their craft. And, as I have indicated, Hitching agrees that the dowsers have been unable or unwilling to meet the standards of scientific credibility. In the light of these admissions by Hitching, it seems incoherent for him to blame the skeptics for lack of acceptance of dowsing. Would he want us to ignore the scientific evidence and give our approval to dowsing on non-scientific grounds? This latter suggestion seems, in fact, what he is demanding from us.

Although I disagree with some of his implications, I do recommend reading Hitching's book, and especially the chapter on "Dowsing versus Science." Much of the writing is clear and the issues well stated. Only in places does he become obviously muddled; other inconsistencies or weaknesses of his arguments come through only after more careful textual analysis. The most important issue that runs through this presentation is the role of personal, subjective experience in establishing the reality or validity of any phenomenon. Hitching clearly believes that such non-scientific, but compelling, experiences should be allowed to outweight the negative verdict of scientific, but impersonal, evidence. At least on some occasions and for some phenomena. In my opinion, such a view--which he obviously shares with many believers in the occult and paranormal--misses the entire point of scientific inquiry. If you make special exceptions or dispensations from scientific validation for one's special pet theories and beliefs, then the point of scientific inquiry is lost. Each person can push for special exemptions for his or her own beliefs. Such arguments are frequently put forth on the grounds that there are other ways of "knowing." Such a claim confuses the source of ideas with the justification or testing of them. Scientists, like anyone else, can get their ideas from dreams, inspiration, meditation, logical deductation,

accidental occurrences, reading, poetry or any other source. All these are useful sources of ideas or hypotheses. But to call them alternative ways of "knowing" is to confuse the generation of possible explanations with the evaluation of them by objective testing.

In my opinion the value of this book is that it brings to the fore the real, but often neglected, basis for the differences between believers and skeptics.

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The UFO Handbook: A Guide to Investigating, Evaluating, And Reporting UFO Sightings. By Allan Hendry. Garden City, New York: Doubleday, 1979. 207 + xiii pages. $8.95 paperback.

Reviewed by Ron Westrum

This is one of the few books in the UFO field that anyone seriously interested in UFOs must read. Its author, principal investigator for the Center for UFO Studies, here reports on a year's worth of investigations for the Center during 1976-7, covering roughly 1300 cases. The results of this research are of great value both to those who investigate and those interested in evaluating the phenomenon. It is certain, however, to become controversial, for it attempts to be not only a "how-to" book for the investigator, but also an overall assessment of the UFO phenomenon. Its content falls into three sections 1) events reported as UFOs which turn out to be ordinary things (IFOs), 2) an evaluation of the IFO/UFO phenomenon and 3) a discussion of various techniques of investigation. It is the second section which is likely to become controversial, and for very good reason. But let us consider each of these sections in turn.

The first section is pure gold. Hendry is perhaps the only paid full-time UFO investigator in the country, and he has used his time well. The added advantage of a WATS line has enabled him to track down quickly and effectively the 89% of cases he studied which turned out to have natural explanations. This section is probably the best evidence one could put forward that UFO investigation would benefit immeasurably if more of its practitioners were able to pursue it on a paid, full-time basis. One could only wish that every UFO investigator had been able to read these pages before discovering, in slow, painful fashion as Hendry did, the basic truth of UFO research; people in general are lousy observers of things seen in the sky at night. One could wish that this section were read before any observer telephoned an investigator with a report. Since this is not to be, however, let every investigator

read this section carefully, for the great bulk of his time will be spent investigating cases like these. Personally, I found the section on advertising airplanes and their illusions the most astounding; even as an investigator (of an irregular sort) I found some of the cases surprising.

The section on evaluating the implications of IFOs and UFOs is far more problematical. In general Hendry's approach is to demonstrate that in many respects the "solved" cases and the unsolved ones have a great deal in common. In terms of type of witnesses, their UFO reading, and reactions during sightings, the two categories are remarkably similar. There are small differences in the number of witnesses per case and in the tendency of witnesses to be repeaters (IFO reporters see more). But the differences are not overwhelming, and would hardly be detectable without statistics. Are UFOs just unexplained potential IFOs then? Hendry admits later in the "statistics" chapter that in terms of the location of the sighting, the UFOs and especially the close encounters are noticeably different. But then he points out that the "law of the times," the steep peaking curve of UFO reports with the maximum at about 10 PM, is just as characteristic of his IFOs! When everything is taken together, he makes a strong case that UFOs and IFOs form a single class. This impression is reinforced in the "tools" section where he gives the reader the impression that the Close Encounter cases, admittedly the tough ones to explain, can be dealt with too once one considers how fallible hypnosis is, etc.

During this comparison, Hendry does not restrict himself to the cases he investigated but inserts references to other cases from the literature, particularly those for which Hendry has found at least a partial explanation. His treatment implies that these cases too would probably fall into his explanation schemes if only he had the time to investigate them in depth. One example of this cursory treatment will illustrate its dangers.

The Kelly/Hopkinsville case is suggested to be the result of an escaped circus monkey. (p.152) "The stage for an emotional climate would then have been set when one of the witnesses announced that he had seen a 'UFO' land nearby." No one who has read the report on this case published by the Center (which Hendry must have read) written by Isabel Davis and Ted Bloecher will take this explanation seriously. The "emotional climate," for instance, was virtually non-existent; the earlier "flying saucer" (not "UFO") sighting was not taken seriously by the other members of the eleven-member family. But I am curious as to how Hendry can explain why the glowing, large-headed monkey was so singularly resistant to shotgun blasts and how he can explain its rapid changes of position? I feel Hendry has done the field a real disservice by the summary treatment of this and other similar cases.

Even more curious is Hendry's embrace of Carl Jung's

mythological approach, which he feels is useful in explaining many aspects of the IFO/UFO phenomenon, including hoaxes. It must be observed that Hendry does an excellent job of illustrating in various places in the book the influence of the folklore that has grown up around UFO sightings. This folklore is indeed a living thing, in constant process of change, and one can find its influences throughout the UFO sighting business. However, the specific gains from accepting a Jungian interpretation of this folklore are very questionable. No study of this folklore outside of the two specific contexts of UFO sightings and UFO cults has been done. We do not know its overall contours. Are UFO sightings a myth? If so, what is meant by "myth"? Have the themes of this myth changed since the 1950's, between, say "Earth Versus the Flying Saucers" and "Close Encounters of the Third Kind"? It is unfortunate that Hendry has not seen fit to consider other approaches to this folklore.

Hendry makes a serious mistake, I think, in equating the "emotional climate" which surrounds UFO sightings and their interpretation with "desire to see a UFO." On one hand he notes that persons have reacted in terror to advertising planes which they wrongly perceived as UFOs; on the other hand he argues that such persons desire to see a UFO. He manages this seeming contradiction by the assertion of the unconscious need for UFO sightings postulated by Jung. It seems quite possible, however, that it is rumors of the dangers posed by UFO encounters (similar to the stories of "francs tireurs" in World War I) which are responsible for the extreme reactions. When Hendry discusses (p.105) why IFO witnesses so frantically search for an answer, he insists that they have "a frantic need to satisfy curosity about a commonplace phenomenon," and states that more than "ignorance" is involved. But of course what they perceived was not commonplace, what they perceived was a "UFO," which they have heard can be very dangerous. It is not ignorance which drives their search but rather the need to resolve the uncertainty created by this (to them) unusual event. To Hendry, who does know the explanation, their search appears absurd, because he cannot mentally put himself in their place.

Hendry later (p.157) suggests that UFO photo hoaxers "feel the need to create an external, concrete expression of the same inner, emotional, UFOlogical turmoil which they probably don't understand themselves." Yet the one, quite typical example of a UFO photo hoax whose origins he presents appears to have a much more banal explanation (p.205): "we could play a joke on a family and friends to see their reaction, and then tell them the truth." Do most whoppers told by teenagers then have a hidden, inner, emotional meaning? Well, perhaps. But perhaps also teenagers simply fashion their tall tales and spoofs out of available cultural materials, UFOlogical or otherwise.

It appears from many comments in the book that Hendry is a disillusioned man. He scores the ineptitude of UFOlogists, their tendency to deal with sightings uncritically, their failure to write something as useful as his present work. He is especially short with scientists who are UFOlogists, pointing out examples of the credulity of the "pro" scientists and the dogmatism of the "anti" scientists.

He is not very kind to his predecessors. One would hope that even if he is forced to stand on the shoulders of dwarfs, that he would at least acknowledge the existence of these dwarfs and his dependence on their previous work. Let us hope that those who build on Hendry's work will be more charitable to him!

Disillusionment, however, is an emotional force itself, and it is important to understand the distortions it can create. Hendry's book is not a good exploration of close-encounter cases, and his comparison of IFOs versus UFOs is not the only possible approach. One can also choose to examine the best close encounter cases by themselves, to see what coherence emerges. I certainly agree with Hendry's suggestion about the utility of dividing the UFO types on occasion for separate analysis. In particular, I am surprised that he does not speculate more about the role that "ball lightning" and similar plasmas appear to play in UFO sightings. Quite a number of sightings both in this book and others appear to be due to free plasmas, and it might be very valuable to try to separate this group out from other UFO sightings. Beyond this, however, there are quite a number of features of UFO close-encounter sightings that Hendry does not discuss, to say nothing of the cases in which such features occur. And his statement that close encounters are not the same from one country to another is ill-considered. CE-IIIs, for instance, are quite diverse, and it is questionable whether there is greater variance between countries than between individual events. This is not a very good book on "unexplained" sightings.

The "tools" section, with chapters on hypnosis, animal reaction, the press, radar, statistics, lie detection, instrumentation, and so forth, makes many valuable contributions to the literature. The section on hypnosis is particularly useful. Surprisingly, there is no chapter on interviewing, although relevant points occur at many places in the book. A chapter on pitfalls in the UFO literature would also have been useful. It should be said that these chapters are more involved with an evaluation of the validity and results of such tools than with directions on how to use them. As such they are substantial and make some points that very much need to be made about the limitations of various techniques. I think the rather pessimistic tone that pervades these chapters is unnecessary; Hendry's despair is overdone.

Summing up, I feel this is a book with great strengths. Its discussion of IFOs is superior to anything else in UFO literature. His discussion of techniques, taken as a whole, is also probably the best in the literature. The book's weakness is that it seeks to evaluate the UFO phenomenon as a whole, and this it manifestly should not be doing without explicit consideration of cases that are mentioned only in passing. The treatment of UFOs as a residuum of the population of reports ignores the possibility of finding coherences that the small sample of "good" close encounters (16 reports!) Hendry investigated can hardly be expected to reveal. Finally I find the "20th century mythology" explanation vague; in a book otherwise so concerned about precision, it is disconcerting to find vague subconscious (sic) forces so easily accepted.

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ALLAN HENDRY COMMENTS ON DR. WESTRUM'S REVIEW:

I would like to thank Marcello Truzzi for the opportunity to comment on Ron Westrum's review of my book, as well as Ron Westrum for his kind compliments.

I do submit, however, that Westrum's criticisms of the book represent, by and large, a misunderstanding of the spirit of my IFO/UFO arguments. It was not my intent to demonstrate that UFOs and IFOs "form a single class"...but that IFO and UFO witnesses do! In social demographics, previous UFO exposure and motives for reporting their sightings they seem quite identical. This is laid out in pp. 87-90 of the book. Since they represent the same caliber of reporting "instrument," then, I chose to use the IFO witnesses as a gauge to assess the reliability of human testimony as UFO "data" (see p. 90, second column)...NOT to imply that UFOs are probably just IFOs as a consequence. This would have been poor logic, indeed. It does not matter to me that IFO witnesses are not reporting worthy UFOs...only that they are providing me with honest descriptions that are limited to perceptually-available information. For example, if witnesses of advertising plane IFOs simply expressed their bafflement over a string of flashing white lights accompanied by a flashing red light that flew slowly across the neighborhood, I would regard them as fine UFOlogical instruments because they gave me the information I needed to learn the truth (by calling the local companies). I would feel confident that I could work with their descriptions at face value, and because my data demonstrates that the IFO and UFO witnesses are a homogeneous group, I would extend that confidence to the UFO witnesses' testimony and similarly adopt it at face value.

What I found instead, however, was a systematic misattribution of appearance, motion, silence, proximity, size and special UFO effects which was contoured by a preanticipation of what a UFO experience is "supposed" to be like. The majority of the ad plane witnesses described rotating domed discs that could be as large as a football field, hover silently, shoot off faster than the eye could follow (when the message sign turned off), and create "EM" effects. Also, the emotional reactions elicited by the IFOs were as vivid as those described by UFO witnesses.

Thus, the central point of my IFO/UFO analysis: that human testimony alone is unlikely to elevate the status of the UFO phenomenon above its present "folk science" doldrums. That is why I turned to an analysis of the effectiveness of the various tools and systems that are currently available to help bolster the soft anecdotal evidence. Animal reactions, hypnosis, lie detectors, magnetic detectors, multiple witnesses, optics, photographic analysis, the news media, radar, radiation detectors, statistics and UFO organizations were all considered. I concluded with the conviction that we require a different kind of methodology with different kinds of tools to make any headway with the UFO phenomenon in its currently elusive state.

Similarly, Westrum gained the impression that I was offering a "partial explanation" for classic cases like Kelly/Hopkinsville to

imply that they would be IFOs if more were known about them. I brought up the "escaped monkey" hypothesis simply because it was raised in the Davis/Bloecher report. I don't believe it, either, and I think a great deal of this particular CE III case. My real point, however, is that there is a stigma that surrounds even the best classic cases, namely: if a skeptical scientific community chooses to believe that Kelly/Hopkinsville has a prosaic explanation, the current state of the UFO evidence does not permit us to effectively disprove it. We accept the Kelly/Hopkinsville case as a "classic" because, quite frankly, we are willing to be patient with what appears to be an elusive phenomenon. But the excited "spaceship" assessments that multiple witnesses have provided me for stars, planes, and kite wires, and the claims that IFOs were linked to radar images, physical traces and photographs, prevent me from being incredulous that a monkey could "glow" and seem resistant to bullets.

In a similar vein, I toyed with a fungus ring "explanation" for the classic Delphos physical trace case. But as I stated in the book, I did it as an exercise to demonstrate that even this celebrated CE II case has not, to date, posed unambiguous characteristics that point the way toward a unique (and mysterious) origin. This type of critical reassessment of the UFO evidence is vital and is in no way a disservice to the field.

I see no way to avoid the conclusion that many people desire to see a UFO.

* Why are there nine times as many IFO sightings reported as UFOs?
* Why have adults seen fit to make stars and bright planets the #1 IFO?
* Stars most certainly are "commonplace;" they surround us all our lives. Prior to 1947, police chiefs were not being pestered with "UFO" calls based on stars and planets. Why now?
* While many IFO witnesses did voice an anticipated sense of danger apparently based on UFO rumors, distant star IFOs pose a minimal "threat" to an observer. Now begins the "frantic search" for an answer. Instead of calling a logical source like a planetarium, they phone airports, Air Force bases, newspapers, and finally, the Center for UFO Studies, with all of its attendant public connotations. I noticed that very few of the star IFO witnesses ever considered out loud in their lists of alternative possibilities the notion that they were just watching a star...a fact that disturbs me.
* Besides, if the IFO witnesses are truly motivated by pure curiosity, if they are simply trying to "resolve their uncertainty" as Westrum suggests, why do the majority of them get mad when I suggest a prosaic solution?

I am provided with more "domed disc" descriptions in my provable IFO reports than in the "UFOs." Thus, I am forced to accept the idea that people want to read in a certain model of UFO into partially-seen forms. This mass reaction reopens the Jungian idea of "archetypal symbols," the meaning of which are known to all on a subconscious basis. So does the hallucination research at UCLA, with its finite number of simple and complex image constants. So do the hypnosis experiments by

McCall and Lawson in California with imaginary abductees. Admittedly, the influence of a cultural UFO mythology is more easily extended to the more distant, poorly-resolved kinds of UFO sightings - Nocturnal Lights and Daylight Discs - than to the more spectacular Close Encounters. Indeed, I state (on p. 159) that I cannot "prove" that a Jungian interpretation, attractive as it may be for the more exotic claims, is the answer. Yet any successful UFO "fantasy" theory will need the presence of a powerful, compelling emotional climate to "fuel" it...and such a climate exists.

Now for the lesser issues:

* Westrum states that the book is not a good exploration of Close Encounter cases and unexplained sightings. Absolutely correct! I stated in the introudction (p. xii) that this was not a goal of the book. Nor am I trying to evaluate the UFO phenomenon as a whole in their absence. I only wanted to take a fresh approach toward the issues of witness reliability, testimony as data, theories and supportive tools in terms of the IFO report implications. An examination of the best classic cases is a future project I have in mind, but the goals will have to be different because such cases are rooted in the past. Besides, isn't it a sad commentary on UFOlogy that we can boast of having "thousands" of cases on the one hand, but have to resort to a handful of "classic" cases that happened years ago to have something worthwhile writing about?

* If I am rough on my UFOlogical predecessors, it is partly because I adopted an attitude in the beginning of the book that the way out of the scientific limbo UFOlogy has been deadlocked in for thirty years might be stricter standards...a "get-tough" policy on what we judge real UFO evidence to be. It's also because I should have been able to read through the hundreds of UFO books written in the last three decades to get information on everything from the application of radar to the characteristics of ad planes as UFO imposters...I had to look it all up from scratch. Why? Because most UFO books have been too concerned with pursuing the idea of cherry-picking the best Close Encounters to concern themselves with non-glamorous but fundamental information.

* Again, my despair over the tools of our trade is a deliberate posture on my part. I am still utilizing radar searches and physical trace analyses in my current work wherever possible... even though the scientific payoff remains dubious. Yes, I'm rather disillusioned, not with the existence of UFOs but with the ability to force them to reveal something convincing. Until that happens, and until we can transcend the limited confidence we place in soft anecdotal information, I will refuse to engage in pointless personal theorizing. The truly interesting thing about ball lightning research is that when an above-board science like meteorology is presented with exactly the same obstacles afflicting UFOlogy (anecdotal accounts, transient phenomena, controversial photographs, a huge variety of manifestations and effects, etc.), the result is the same: a century of research but no good working explanation of the phenomenon.

* To be sure, there are greater variations among CE III events than among the countries' CE III collections. There is little discussion of the differences that do exist on this level, however, and I wanted to offer something along this commonly-asked line.

But all of these quibbles aside, I stand flattered by Westrum's overall endorsement. If a thoughtful investigator like Ron Westrum says we must go out and buy this book, then, by God, who am I to disagree with him?

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The Reception of Unconventional Science. Edited by Seymour H. Mauskopf. Boulder, Colorado: Westview Press, 1979. 137 + x pages. $13.25. (Can be obtained through Frederick Praeger, Publishers.)

Reviewed by Ron Westrum

This slim volume, containing five papers presented at a recent AAAS symposium and an introduction by Seymour Mauskopf, is a valuable addition to the literature on innovation and deviance in science. The volume can be compared to the much larger and more recent work, edited by Roy Wallis, On the Margins of Science (Sociological Review Monograph #27). Acupuncture and parapsychology, for instance, are covered in both; nor is this the only common element. The two might ideally be combined into a single work, and certainly ought to be read in concert. The introductions to both are complementary.

What do we learn about the reception of unconventional science from this book? It, like the Wallis volume, provides no final answers, but rather a number of case studies.

In the first paper by Paul Forman, we find that acausal quantum theory had very different intellectual repercussions in England and Germany in 1925-1927. These different reactions, Forman feels, were rooted in the intellectual and cultural currents of the two countries. Acausal quantum theory strongly appealed to the Zeitgeist of Weimar Germany and scientists used it in their rhetoric to support the public position of science. In Britain, on the other hand, where science was much less under attack, the acuasal implication of quantum theory struck no such responsive chord, and was simply overlooked.

The paper by Henry Frankel on the reception of continental drift handles a most interesting problem: why did continental drift theory at first meet such stiff resistance only to be accepted decades later? Frankel, using an approach to theory choice developed by Laudan, suggests that the explanation lies both in different versions of the theory and in the problems which the theory was called upon to solve at different times. The version of continental drift eventually accepted was different in critical respects from the original one of Wegener. More, the problems which the

newest version could solve were only partially those which Wegener took into consideration. This is a case history well to keep in mind when it is asserted that a certain period of time is all that is necessary to determine whether a new theory is valuable or not. The theory originated in 1915 but was not widely accepted in the 1960's. An evaluation of the theory after ten years would have yielded few clues to the fruitfulness of later versions. At a 1926 symposium of the American Association of Petroleum Geologists, one of the participants felt that if Wegener were right, it would require the junking of most current geological theory. It is well to remember such utterances when current unconventional theories are being considered.

The third rather short paper on acupuncture by John Z. Bowers, traces the vicissitudes of Western attitudes toward this form of traditional Chinese medicine. There are some very suggestive observations about the role of motivation on the part of individual researchers in the West who took an interest in acupuncture, or, if one will, the role of interests in innovation or at least intercultural acceptance.

The paper by Seymour Mauskopf and Michael R. McVaugh continues their work on the acceptance of parapsychology by the wider scientific community. In this case their focus is the debate over the statistical validity of proofs of ESP which took place in 1934-1938. Assailed by psychologists who felt that the statistical claims of Rhine and his co-workers were based on incorrect mathematics, the parapsychologists received aid from an unexpected quarter. Statisticians, without passing on the validity of the experimental methods used by Rhine, staunchly defended the mathematics involved in the probability calculations carried out to prove that the results in card-guessing could not be due to chance. Defeated, the anti-ESP psychologists beat a retreat; they were to return with different criticisms at a later date.

The final paper by Marcello Truzzi provides a useful short review of the recent literature which attempts to demarcate science from pseudo-science.

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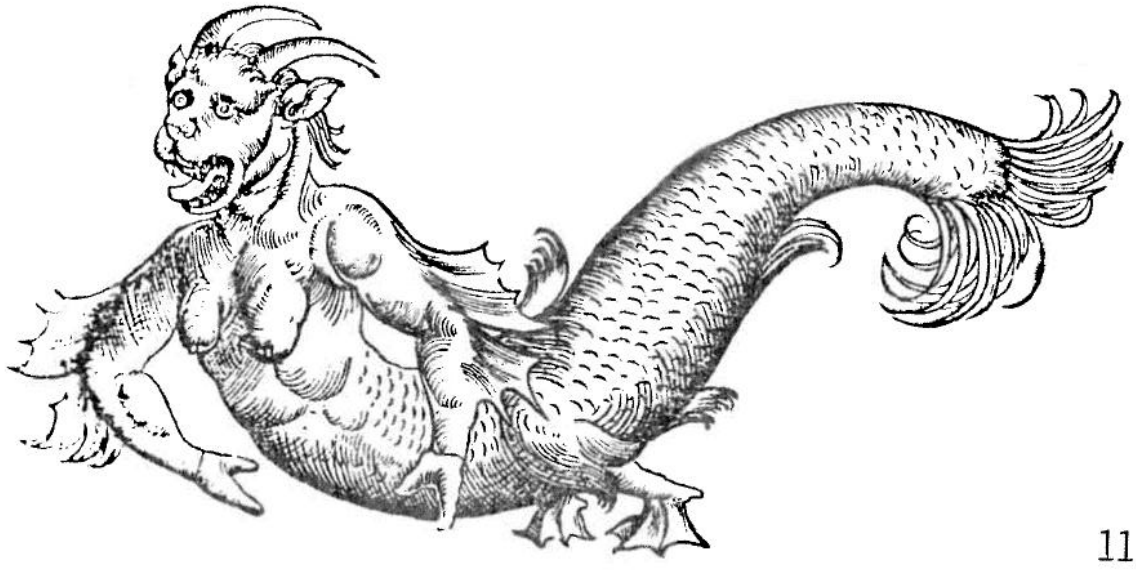

Les Derniers Dragons d'Afrique. By Bernard Heuvelmans. Paris: Plon, 1978. 507 pages + 48 pages photographs. No price given.

Reviewed by Ron Westrum

Of all the writers about unknown animals, there is no question that Bernard Heuvelmans is the master. The book reviewed here is so well done that one wishes it would become the model for all future books in this area which Heuvelmans has called "cryptozoology," the study of unknown animals. But such is not to be. For Heuvelmans possesses a unique set of resources (described elsewhere in this issue) and a very special temperament and, finally, he has devoted the last thirty years entirely to this kind of study. What we have here, then, is a research report concerning one sector of cryptozoology from a zoologist who is, without question, the foremost authority in these matters.

The purpose of this work is the examination of evidence that certain unknown large or flying reptiles still exist or recently became extinct on the African continent. The mass of testimony that Heuvelmans brings forward, analyzes, and uses to form his conclusions is staggering. He considers not only a number of apparent "surviving dinosaur" (brontosaurus, pterodactylus) stories, but also quite a number of animals which are totally unknown to the (Western) popular imagination. He discusses giant snakes and sabre-tooth tigers, anomalous fish, unknown sirenians and strange crocodiles -- weighing evidence, discussing habitats and modes of existence, and making some very clever guesses, most of which he will never be able to verify. For the reader, however, the book provides an incredible safari through the African continent's possible wildlife.

Heuvelmans has to do here, to be sure, a considerable amount of documentary detective work, work that is so often missing in the "unverified but true" genre of "monster" writing. He seems willing to accept virtually nothing at face value. He examines the witnesses and their qualifications, he considers the biological likelihood of the animals alleged, he looks at the relations of stories to one another, finding plagiarisms, hoaxes, and misidentifications. He considers various popular legends, showing considerable knowledge of the ethnographic literature involved, and also some very sensible attitudes toward interpretation of folklore. The discussions are sprinkled with glimpses into the ecology of the areas under consideration, and supported by a ready knowledge of the sociology of anomalous information and its peculiarities. His knowledge of human nature and his sense of humor are seldom in default.

It is evident not only that he knows the printed literature but that he has the advantage of a long and varied correspondence with persons involved in various parts of the research. This correspondence has sprung up in the wake of his first book (Sur la Piste des Betes Ignorees) on cryptozoology and its many successors since 1955. Heuvelmans disposes of a world-wide network of correspondents and through it there are few corners of the cryptozoological literature which he does not know. It is for this reason that one finds in this work what is often so lacking in others--the pursuit of a story to its source, reliable or unreliable. One finds that Heuvelmans has

already written to the persons who ought to have been written to, has found the hypothetical article in the Bulawayo Chronicle, or discovered the real source of the pterodactyl-bites-man story. As one who has participated in his researches, I can testify to his passion for checking everything with the original sources.

The book is divided into three sections. The first examines accounts of giant serpents on the African continent, and suggests inter alia that there may be specimens quite a bit larger than anything yet in captivity. The second section is concerned with tropical water monsters. Some of these, Heuvelmans suggests, may be unknown reptiles, but others may well be anomalous fish, sirenians, or sabre-tooth tigers. The last section considers the possible survival of pterodactyls in Africa.

It is to be hoped that this book will soon be translated for English-speaking readers. In the meantime, those of us who can read French will eagerly await the next of the series, The Man-Beasts of Africa.

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OVNIS: El Fenomeno Aterrizaje. By Vincente-Juan Ballester Olmos. Barcelona: Plaza y Janes, 1978. 382 pages. No price given.

Reviewed by Ron Westrum

Persons interested in the UFO controversy are used to hearing that UFO sightings are world-wide and that similar phenomena are manifested in other countries. This knowledge is seldom supported, however, by familiarity with more than one or two incidents, since the majority of UFO sightings stay within the literatures of the countries where they occur. Persons interested in cross-cultural comparisons, therefore (if they can read Spanish), will welcome this book as an addition to the literature of type-I sightings. The similarity of "close encounters" in different countries will become evident to even the casual reader of this work.

The Landing Phenomenon is both of a study of UFO events in general and a survey of close-encounter cases in Spain. Its most valuable contribution is a descriptive catalogue of 200 CE cases from Spain, all of which are given in some detail and some of which are given at length. These cases allow interesting comparisons to be made with cases from other countries. The book is worth acquiring for this catalogue alone.

The theoretical section of the book is useful and provocative. Mr. Ballester has read extensively in the UFO and related literatures in many languages, and his bibliography cites 550 items. There are many points at which one may disagree with the author, or at which later studies have shed more light on the matter in question, but the discussion of a multitude of points is well handled and scholarly throughout.

A translation of this book would be very desirable.

BOOKS BRIEFLY NOTED

Listing here does not preclude later full review.

Adler, Margot, Drawing Down the Moon: Witches, Druids, Goddess-Worshippers, & Other Pagans in America Today. N.Y.: Viking Press, 1979. 455pp. $16.95. An excellent new and the best to date survey of neopaganism in America. Highly recommended to those interested in the nouveau witches,but the book contains surprisingly little on traditional or "authentic" witchcraft as represented by such non-Murrayite folklorists as Vance Randolph.

Applebaum, Stephen A., Out in Inner Space: A Psychoanalyst Explores the New Therapies. Garden City, N.Y.: Doubleday, 1979. 529pp. $14.95. An exceptional study sponsored by the Menninger Foundation surveying a host of radical therapies from bioenergetics to yoga with insight, criticism, and cool but sympathetic analysis. Probably the best book of this kind so far produced, particularly remarkable given the psychoanalytic orientation of the author that might have led many of us to expect far less objectivity. Highly recommended.

Bateson, Gregory, Mind and Nature: A Necessary Unity. N.Y.: E.P. Dutton, 1979. 238pp. $11.95. A remarkable philosophic and interdisciplinary work arguing that evolution is a mental process. A provocative look at what Bateson calls the metapatterns of nature. Intriguing, poetic, and brilliant, but probably more metaphysics than science.

Bird, Christopher, The Divining Rod: The 500-Year-Old Mystery of Dowsing. N.Y.: E.P. Dutton, 1979. 340pp. $24.95. A lavishly illustrated and produced compendium on water-witching but one remarkably and cavalierly dismissing the negative works and evidence toward dowsing while also uncritically accepting works on psychokinesis that remain scientifically dubious. Since the book does contain much information and a host of photographs and illustrations, it is worth having in one's library on dowsing; but the one-sided presentation makes it a poor starting point for anyone with serious scientific interest.

Briggs, Katherine, and illustrated by Yvonne Gilbert. Abbey Lubbers, Banshees & Boggarts: An Illustrated Encyclopedia of Fairies. N.Y.: Pantheon, 1979. 159pp. $10.95. Essentially an abridgement from Dr. Briggs' Encyclopedia of Fairies with additional new materials on the topics and attractive fanciful illustrations by a talented new British illustrator.

Brown, Harold I., Perception, Theory and Commitment: The New Philosophy of Science. Chicago: University of Chicago Press, 1979. 203pp. $4.95 paperback. A highly recommended and lucid discussion of the new directions in the philosophy of science. An excellent introduction which should be of special value to readers of ZETETIC SCHOLAR.

Carrington, Hereward. Laboratory Investigations into Psychic Phenomena.N.Y.: Arno Press, 1975, 255pp. $16.00. A very welcome reprinting of the 1939 edition. A basic book for any serious parapsychologist's library.

Catoe, Lynn E., UFOs and Related Subjects: An Annotated Bibliography. Detroit, Mich.: Gale Research Co., 1978. 411pp. $24.00. A reprinting of the excellent 1700 item bibliography published by the Library of Congress Science and Technology Division in 1969, supplemented by the 1976 bibliography compiled by Kay Rodgers and an introductory essay by Leslie Shepard. Indispensable for anyone interested in UFO scholarship and a welcome volume.

Chitrabhanu, Gurudev, Shree. edited by Lyssa Miller, The Psychology of Enlightenment: Meditations on the Seven Energy Centers. N.Y.: Dodd Mead, 1979. 91pp. $6.95. A handbook on Jain meditation based on lectures by the Jain leader in the United States.

Cinnamon, Kenneth, and Dave Farson, Cults and Cons: The Exploitation of the Emotional Growth Consumer. Chicago: Nelson-Hall, 1979. 106pp. $9.95 hardcover; $5.95 paperback. A very glib debunking of many of the new "emotional growth" therapies. Probably a useful consumer antidote but highly superficial and unscholarly presenting more mockery than evidence. Disappointing.

Clarie, Thomas C., Occult Bibliography: An Annotated List of Books Published in English, 197 through 1975. Metuchen, N.J.: Scarecrow Press, 1978. 481pp. $20.00. An outstandingly useful volume listing over 1800 books (many of them reprints of earlier works). Indispensable for any scholarly library. Annotations are usually very informative and valuable. Highly recommended.

Cohen, Daniel, Mysteries of the World. Garden City, N.Y.: Doubleday, 1979. 128pp. $7.95. An excellent survey for juveniles on a variety of esoteric topics from human combustion to the Tunguska explosion.

Cohen, Daniel, Ceremonial Magic. N.Y.: Four Winds Press, 1979. 152pp. $7.95. Another well done introduction for the juvenile market. Typical of Cohen's skeptical but sympathetic approach to such topics.

Coover, John Edgar, Experiments in Psychical Research. N.Y.: Arno Press, 1975. 642pp. $37.00. A reprint of the classic 1917 study. A very welcome addition, this book is a landmark but neglected work both because of its date and its negative conclusions. Highly recommended and hopefully soon available in most university libraries.

Coren, Stanley, and Joan Stern Girgus, Seeing is Deceiving: The Psychology of Visual Illusions. Hillsdale, N.J.: Lawrence Erlbaum Associates, 1978. 255pp. $18.00. An important and comprehensive new theoretical work. Highly recommended.

Cumberland, Stuart, A Thought Reader's Thoughts. N.Y.: Arno Press, 1975. 326pp. $19.00. A reprint of the 1888 autobiography of a travelling mentalist. A welcome reprinting but probably of greater interest to historians of conjuring than to psychic researchers despite its inclusion in this reprint series.

Dass, Ram, Miracles of Love: Stories About Neem Karoli Baba. N.Y.: E.P. Dutton, 1979. 414pp. $9.95 paperback. The former Dr. Richard Alpert relates stories about his guru mentor, Maharajji.

Denning, Melita, and Osborne Phillips, The Magical Philosophy, Boook IV: The Triumph of Light. St. Paul, Minn.: Llewellyn Publications, 1978. 251pp. $10.00. The fourth in a set of five volumes constituting a "complete curriculum of study and practice of Magick" and a "guide to the Western Mystery Tradition." Allegedly constitutes a training system based on a contemporary occult order founded in 1897, the Aurum Solis. Includes some quasi-scientific elements but generally for the would-be initiate and little of interest for the scholar.

Dickinson, Peter, and illustrated by Wayne Anderson. The Flight of Dragons. N.Y.: Harper and Row, 1979. 132pp. $17.50. A delightful and beautifully illustrated presentation of the "truth" about dragons including full descriptions of their method of flight, their production of flaming breath, etc. No dragon owner should be without this guide. Everything you always wanted to know about dragons but were afraid to ask.

Downton, James V., Jr. Sacred Journeys: The Conversion of Young Americans to Divine Light Mission. N.Y.: Columbia University Press, 1979. 245pp. $12.95. An excellent sociological analysis of the followers of guru Maharaj Ji and of that movement's remarkable rise and decline. Recommended.

Ebon, Martin, editor, The World's Weirdest Cults. N.Y.: Signet/New American Library, 1979. 198pp. $1.95 paperback. A nice collection of popular writings, mainly from occult-oriented publications, ranging from Jim Jones's People's Temple to the Hell-Fire Club. Good reprintings and some new essays at a bargain price

Edmonds, I.G., The Magic Brothers: Carl and Alexander Herman. N.Y.: Elsevier/Nelson Books, 1979. 166pp. $7.95. A nicely done biographical volume for the juvenile market. An excellent book for the young conjuror since it is accurate history and also explains many trick secrets.

Fortune, Dion. The Secrets of Dr. Taverner. St. Paul, Minn.: Llewellyn Publications, 1979. 276pp. $3.95 paperback. This edition of some of the fiction tales of the well known occultist includes 12 stories, one previously unpublished, plus an essay on "the Works of a Modern Occult Fraternity" by Gareth Knight.

Gillespie, L.Kay, Cancer Quackery: The Label of Quack and Its Relationship to Deviant Behavior. Palo Alto, Cal.: R&E Research Associates, 1979. $9.00 paperback. An important sociological appraisal, and a valuable contribution to the sociology of deviant science.

Godwin, David, Godwin's Cabalistic Encyclopedia: A Complete Guide to Cabalistic Magick. St. Paul, Minn.: Llewellyn Publications, 1979. 466pp. $14.95. Essentially a "dictionary of cabalism as understood and interpreted by the Christian (or Christian-based) mystery school and hermetic societies of the West, in particular the Hermetic Order of the Golden Dawn" according to its author. A useful compendium for those interested in ritual magic or its history.

Gooch, Stan, The Paranormal. N.Y.: Harper and Row, 1978. 313pp. $10.00. A strange and idiosyncratic survey which is both critical and believing in odd mixture. The author decries much work with an apparent lack of familiarity with the literature. On the other hand, he claims mediumship experience and belief in a great deal on experiential grounds. Surprisingly, this psychologist seems to feel that much of the paranormal is permanently beyond science, making it actually preternatural and in effect dismissing much of parapsychology. The book makes some interesting observations and is worth attention, but so many of its opinions are unsupported by or even contradicted by evidence that the reader should be most cautious in examining the book.

Goss, Michael, Poltergeists: An Annotated Bibliography of Works in English, circa 1880-1975. Metuchen, N.J.: Scarecrow Press, 1979. 389pp. $15.00. A very welcome and useful scholarly compendium. Highly recommended.

Gruner, Mark, amd Christopher K. Brown, Mark Gruner's Numbers of Life: An Introcution to Numerology. N.Y.: Taplinger, 1978. 192pp. $3.95 paperback; $9.95 hardcover. An attempt to do for numerology what Linda Goodman did for astrology, i.e., make the author and publisher a fortune. Scientifically worthless but will probably achieve some popularity for its "party game" value. Literally metaphysics and personality by the numbers.

Haines, Richard F., editor, UFO Phenomena and the Behavioral Scientist. Metuchen, N.J.: Scarecrow Press, 1979. 464pp. $18.50. This book belongs in any serious Ufologist's library. Thirteen generally excellent scholarly studies in four areas: cultural factors, eyewitness factors, eyewitness reporting factors, and selected UFO research data and theory. A serious, responsible, and balanced set of contributions which should help legitimate Ufology as a scientific and unsensationalistic endeavor. Highly recommended,

Hall, Trevor H., Search for Harry Price. London: Duckworth & Co., 1978. 237pp. 7.95 pounds (U.K.). An important contribution to the history of psychical research setting the record straight on much of the work and biography of "ghost hunter" Harry Price who seems to have misled many, including his biographers, as to the actual events of his life and investigations. Consistent with Dr. Hall's usual high level of scholarship and responsible debunking efforts. Recommended.

Hansen, Harold A., The Witch's Garden. Santa Cruz, Cal.: Unity Press-Michael Kesend, 1978. 128pp. $4.95 paperback. A fine study, translated from the Danish by Muriel Crofts, with an excellent bibliography, on the hallucinogenic plants involved in witchcraft. It even includes an analysis of the brew described by Shakespeare in his Macbeth. Recommended.

Haskins, Jim, Voodoo & Hoodoo: Their Tradition and Craft as Revealed by Actual Practitioners. Braircliff Manor, N.Y.: Stein & Day, 1978. 225pp. $10.00. A popular presentation of contemporary folk magic, mildrly scholarly but ignoring of much of the major folklore work done in this area. The author's own direct research is a valuable addition, however, and the book is a welcome one since there has been far too little published on American black people's magic.

Hogarth, Peter, with Val Clery, Dragons. N.Y.: Viking Press, 1979. 208 pp. $ A heavily illustrated history and analysis of dragon legendry, Many beautiful reproductions of art works depicting dragons and an intelligent text.

Horowitz, Irving Louis, editor, Science, Sin, and Scholarship: The Politics of Reverend Moon and the Unification Church. Cambridge, Mass.: MIT Press, 1978. 290pp. $12.50. A serious look at the Unification Church, its leadership, funding, recruitment practices, and implications for American society. A major expose which poses many important questions. Recommended.

Johnson, Harry M., editor, Religious Change and Continuity: Sociological Persepctives. San Francisco: Joseey bass, 1979. 359pp. $13.95. A collection of original sociological papers dealing with sources, patterns, and conseuquences of religious changes going on today. Originally a special issue of the journal Sociological Inquiry.

Jones, David E., Visions of Time: Experiments in Psychic Anthropology. Wheaton, Ill.: Theosophical Publishing House, 1979. 404pp. $7.50 An impressive study, far superior to other works on "psychic archaeology" now available. Controversial and path breaking but deserves serious attention thus far not received.

Keightley, Thomas, The Wold Guide to Gnomes, Fairies, Elvers and Other Little People. N.Y.: Avenel Books, 1978. 560pp. $. A reprinting of The Fairy Mythology originally published in 1880. A useful classic apparently published as a response to the remarkable success of other recent books on gnomes and their kin. Particularly welcome for its bargain price in many bookstores.

Klein, Aaron E., Science & the Supernatural: A Scientific Overview of the Occult. Garden City, N.Y.: Doubleday, 1979. 184pp. $7.95. A rather hard-line debunking book for the juvenile market. Parts are excellent but at other times, as when lumping the parapsychologists into the chapter on astrology, the author seems excessively negative in his approach.

Krippner, Stanley, editor, Advances in Parapsychological Research 2: Extrasensory Perception. N.Y.: Plenum Press, 1978. 308pp. $ An exceptional collection centering around three lengthy essays: Robert L. Morris's "A Survey of Methods and Issues in ESP Research," John Palmer's "Extrasensory Perception: Research Findings," and K. Ramakrishna Rao's "Theories of Psi." I was deeply impressed by this volume and consider it to be among the very best and most intelligent works espousing the cause of psi that I have yet read. John Palmer's discussion of the critics of psi is particularly valuable and deserves serious attention. A very responsible and impressive pro-psi book which goes far in elevating the level of discourse on the controversial science of parapsychology.

Laing, Jennifer, Britain's Mysterious Past. North Pomfret, Vt.: David & Charles, 1979. 160pp. $12.50. A well illustrated survey, ideal for the tourist, of exotic and bizarre historical sites including suggested solutions to many of the mysteries. Well done light reading and an excellent introduction for the novice to such matters.

Levine, George, and U.C. Knoeflmacher, editors, The Endurance of Frankenstein: Essays on Mary Shelley's Novel. Berkeley: University of California Press, 1979. 341pp. $16.95. A dozen scholarly essays on the great doctor and his creation and their impact on life, literature and popular culture. More than most people would ever want to know about these matters, but a very welcome collection for those of us interested in the ultimate do-it-yourself project.

Lindow, John, Swedish Legends and Folktales. Berkeley: University of California Press, 1978. 227pp. $10.95. A fine scholarly introductory essay followed by around one hundred short texts from oral tradition of rural superstition in Sweden. Everything from trolls to werewolves. Commentary on each short tale and extensive bibliography. Impressive scholarship and a welcome addition to the literature.

Meek, George W., editor, Healers and the Healing Process. Wheaton, Ill.: Theosophical Publishing House, 1977. 304pp. $5.75 paperback. A significant report of research by 14 investigators into paranormal healing, including four medical doctors and magician/psychic David Hoy. Highly controversial reading but certainly the most medically serious book thus far put out by proponents of psychic surgery and similar treatments; certainly essential reading for anyone, like myself, critical of such claims. Recommended.

Monter, E. William, Witchcraft in France and Switzerland: The Borderlands during the Reformation. Ithaca, N.Y.: Cornell University Press, 1976. 232pp. $. An important sociological and historical examination of national and jura (cross-culturally mixed) forms of witchcraft. An important addition to this growing body of modern literature.

Morris, Scot, The Book of Strange Facts & Useless Information. Garden City, N.Y.: Doubleday, 1979. 143pp. $5.95 paperback. A Ripley-like assortment of esoterica frequently relevant to the history of science and paranormal claims. Some errors but generally very well done and extremely fascinating reading. Recommended entertainment.

Nicholls, Peter, editor, The Science Fiction Encyclopedia. Garden City, N.Y.: Doubleday, 1979. 672pp. $12.95 paperback. A monolithic, three columns of small print, compendium that should prove indispensable to all interested in this genre and its creators, many of whom have been active contributors to the literature of and on the paranormal.

Peters, Edward, The Magician, the Witch & the Law. Philadelphia: University of Pennsylvania Press, 1978. 218pp. $15.95. An important new historical analysis of witchcraft and magic in the Middle Ages and the public and legal views upon and reactions to such practices as they evolved into maleficium. A broad cultural analysis of the contexts involved, and a fine scholarly review.

Phillpotts, Beatrice, The Book of Fairies. N.Y.: Ballantine, 1979. 56pp. $8.95 paperback. An excellent collection of 40 paintings of subjects centering on fairies including works by artists such as William Blake, J.M.W. Turner, Richard Dadd and Henry Feseli. Excellent caption information for each work and a well done introductory essay on the history of this artistic genre.

Podmore, Frank, Studies in Psychical Research. N.Y.: Arno Press, 1975. 458pp. $26.00. A reprint of the 1897 edition. As a leading early critic within the Society for Psychical Research, Podmore's writings are essential reading for anyone interested in early spiritualism. A very welcome reprinting and essential to any serious scholarly library dealing with apparitions.

Price, Harry, Fifty Years of Psychical Research: A Critical Survey. N.Y.: Arno Press, 1975. 383pp. $23.00. A reprint of the 1939 edition. Like the other excellent volumes in this reprinted series of books on psychical research and parapsychology, this long unavailable work is most welcome.Despite recent criticism of Price by many (e.g., see the Trevor Hall volume reviewed above), his works, especially his criticisms of others, remain most valuable for any understanding of the history of psychical research.

Price, Harry, and Eric J. Dingwall, Revelations of a Spirit Medium. N.Y.: Arno Press, 1975. 327pp. $22.00. A reprint of the 1922 edition of what may be the most important debunking work on spirit mediumship. The book remains essential reading for any modern investigator.

Prince, Walter Franklin, The Enchanted Boundary. N.Y.: Arno Press, 1975. 348pp. $20.00. A reprint of the 1930 edition, this is one of the finest books I have read on criticizing the alleged scientific debunkers of psychical research. Dr. Prince takes each of the major critics up until 1930 (e.g., Jastrow, Houdini, etc.) and exposes most of the criticism as the result of poor scholarship and irresponsible misrepresentation of the proponents. Though the book is far from perfect, it beautifully reveals the sloppiness and gross prejudice found in the attacks of much of the early debunking literature--much of which is still quoted without the corrections necessitated by reading Prince's book. Reading his book, one is impressed with how little things have really changed in the debates that go on between proponents and critics. Though the book will not convert the skeptic (and is not intended to), it should make one very wary of glib acceptance of the arguments put forward by fellow critics. Highly recommended.

Ramage, Edwin S., editor, Atlantis: Fact or Fiction? Bloomington: Indiana University Press, 1978. 210pp. $10.95. An outstanding scholarly set of essays on this fascinating topic. Certainly among the top six books among the almost 2000 that have been written on this topic.

Randi, The Amazing, and Bert Randolph Sugar, Houdini: His Life and Art. N.Y.: Grosset & Dunlap, 1976. 192pp. $6.95 paperback. A generally well done pictorial volume dealing with the great magician and escapologist which includes some surprisingly critical materials.

Richet, Charles, Thirty Years of Psychical Research, Being a Treatise on Metaphysics. N.Y.: Arno Press, 1975. 646pp. $37.00. A reprint of the 1923 edition of this classic early work in psychical research. Particularly interesting for its early uses of statistical analysis.

Rogo, D. Scott, The Poltergeist Experience: Investigations into Ghostly Phenomena. N.Y.: Penguin Books, 1979. 301pp. $2.95. A fascinating excursion into one of the more exotic areas of psychical research by a prolific writer on the paranormal. Whatever one may think of such phenomena, and the psychokinetic theory put forward by Rogo and others, the narratives present entertaining enigmas and Rogo's grasp of the historical materials is admirable.

Rossman, Michael, New Age Blues: On the Politics of Consciousness. N.Y.: E.P. Dutton, 1979. 271pp. $6.95 paperback. An important critique of the Human Potential Movement, the "New Age Carnival" and its many manifestations from EST to Uri Geller, by one long associated with (and largely sympathetic to) the counter-culture. The essays are somewhat uneven --the final one on the psychic arms race is rather naively silly--but generally provocative, frequently insightful and important in their warnings of new authoritarianisms posing as therapies.

Shumaker, Wayne, The Occult Sciences in the Renaissance: A Study in Intellectual Patterns. Berkeley: Universsity of California Press, 1979. 284pp. $6.95 paperback. An exceptional and important scholarly summary and analysis of five Renaissance occult sciences: astrology, witchcraft, white magic, alchemy, and Hermetic philosophy. Lengthy translated quotations from primary documents (mostly in Latin) and all presented without condescension or credulousness. This should prove highly useful to many scholars interested in the history of the paranormal. Highly recommended.

Swinburne, Richard, The Concept of Miracle. N.Y.: St. Martin's Press, 1970. 76pp. $ paperback. An excellent philosophical analysis of the miracle and its literature in philosophy. Recommended.

Tanous, Alex, and Katherine Fair Donnelly, Is Your Child Psychic? A Guide for Creative Parents and Teachers. N.Y.: Macmillan, 1979. 200pp. $8.95. Since this book starts out with the assumption of the reality of psi and then proceeds to define therapy with children as needing to take account of psi in a positive way, the book may in fact be damaging to those who use it if children's psi "experiences" are in fact examples of poor cathexis with reality. Even if such children need supportive reactions such as this book recommends, testing of other children and labelling them as "psychic," when they are in fact normal,might prove damaging. Caution advised.

Tyl, Noel, The Missing Moon, and Other Case Studies. St. Paul, Minn.: Llewellyn Publications, 1979. 165pp. $4.95 paperback. A collection of ten stories featuring a hero-astrologer, Michael Mercury, by the prominent astrologer and editor of Astrology Now. A pleasant anthology of fiction for the astrology fan.

Tyrell, G.N.M., Science and Psychical Phenomena. N.Y.: Arno Press, 1975. 380pp. $22.00. A reprint of the 1938 edition. Originally a semi-popular text, this presents psychical research's facts and theory as Tyrell saw it. Modern readers will be surprised at the quantitative studies of mediumistic materials and trance personalities. A welcome reprinting with much still timely material.

Underwood, Peter, Dictionary of the Supernatural: An A to Z of Hauntings, Possession, Witchcraft, Demonology, and Other Occult Phenomena. London: Harrap, 1978. 389pp. 5.50 pounds (U.K.). A useful and seemingly reliable short compendium but with some unsual inclusions (e.g., Inedia) and exclusions (e.g., Hereward Carrington). References given are also occasionally odd, but generally a valuable quick aid that many would find practical.

Vallee, Jacques, Messengers of Deception: UFO Contacts and Cults. Berkeley, Cal.: And/Or Press, 1979. 243pp. $6.95 paperback. Dr. Vallee remains one of the more creative writers in Ufology, and this new analysis will lose many of his past admirers who may find the new explanation with its postulation of superior manipulators (shades of the Illuminati!) a bit too creative and fanciful. Nonetheless, Vallee is always interesting reading, and his criticisms of alternative conjectures are valuable independent of his own conclusions.

Vogt, Evon Z., and Ray Hyman, Water Witching U.S.A. Chicago: University of Chicago Press, 1979. 260pp. $5.95 paperback. This is the new (2nd) edition of the classic 1959 study of dowsing. This new edition also includes a special preface and a "Postcript 1979" epilogue which brings the reader up to date. Still the outstanding scientific evaluation of dowsing claims, welcome both for its updating and new paperback format.

Wallis, Roy, editor, On the Margins of Science: The Social Construction of Rejected Knowledge. University of Keele: Sociological Review Monograph #27, March 1979. 337pp. $12.00 paperback. An exceptional collection of essays on the sociology of deviant science. Highly recommended. (To be reviewed in detail in a future issue of ZS.)

Wallis, Ray, Salvation and Protest: Studies of Social and Religious Movements. N.Y.: St. Martin's Press, 1979. 231pp. $22.50. A major series of essays on the sociology of new moevements including an extraordinary account of the author's own past interactions with the Scientologists over his book on their religion.

Waring, Philippa, A Dictionary of Omens and Superstitions. N.Y.: Ballantine Books, 1979. 265pp. $2.50. A bargain reference volume which should prove useful for many ZS readers.

Wasson, Barbara, Sasquatch Apparitions (A Critique on the Pacific Northwest Hominoid). Bend, Or.: Published by the author (P.O.Box 5551; Bend, OR 97701), 1979. 174pp. $6.95 paperback. An insider's view of contemporary Big Foot hunters and hunting.

Yarmey, A. Daniel, The Psychology of Eyewitness Testimony. N.Y.: Free Press, 1979. 302pp. $15.95. An excellent survey and analysis of research in this area so vital (but so neglected) by serious investigators of claims of the paranormal. Highly recommended.

-- M.TRUZZI

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JOSEPH AGASSI is a Professor of Philosophy at Boston University and the author of Towards an Historiography of Science.

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