THE ZETETIC

Published by The Committee for the Scientific Investigation of Claims of the Paranormal

FEATURING

Wallis on Dianetics

Westrum on Expert Witnesses

Omohundro on von Däniken

Fix on Biorhythms & Sports

Hyman on Uri Geller

VOL. I NO. 1 FALL/WINTER 1976

ZETETIC 1. *adj.* proceeding by inquiry. 2. *n.* skeptic, seeker; *specif*: one of a group of Pyrrhonist philosophers.
—*Webster's Third New International Dictionary*

THE ZETETIC

Journal of the Committee for the Scientific Investigation of Claims of the Paranormal
Fall/Winter 1976 Vol. 1, No. 1

THE ZETETIC

THE ZETETIC is the official journal of the Committee for the Scientific Investigation of Claims of the Paranormal.

Manuscripts, letters, books for review, and editorial inquiries should be addressed to: The Editor; *Editorial Office:* THE ZETETIC; Department of Sociology; Eastern Michigan University; Ypsilanti, Michigan 48197.

Advertising, changes of address, and subscriptions should be addressed to: *Executive Office.* THE ZETETIC, Box 29, Kensington Station, Buffalo, New York 14215. Application for permission to quote from this journal should be addressed to the Executive Office.

Subscription rates: individuals, $10; libraries and institutions, $15; sustaining subscribers, $100 or more. Single issue, $5.

Change of Address: Six weeks advance notice to the Executive Office and old address as well as new are necessary for change of subscriber's address.

Claims for undelivered copies must be made within the month following the regular month of publication. The publishers will supply missing copies when losses have been sustained in transit and when the reserve stock will permit.

THE ZETETIC is published bi-annually and printed at Artcraft-Burow, Buffalo, New York.

Editorials

This journal, the official organ of the Committee for the Scientific Investigation of Claims of the Paranormal, is intended to communicate scientific information about the many esoteric claims that have shown a growing influence upon the general public, educational curricula, and scientific institutions themselves. In addition we hope its pages will reflect a growing intelligent dialogue between those making claims for the paranormal and their critics.

The word *zetetic* means "skeptical seeker" and refers to the ancient Greek followers of the skeptic Pyrrho. As a name for the journal it reflects our belief that the proper attitude of science toward unusual claims should be one of balance between openness to new facts and skepticism with critical attention toward facts which are anomalous. Science is a constantly changing body of knowledge characterized not so much by its content as by its method, an approach which seeks generalizations from empirical observations that can be intersubjectively verified and are falsifiable. What is considered "paranormal," or "beyond science," has varied historically. Some empirical facts (e.g., meteorites) were once considered occult or paranormal claims while others (e.g., the relationships claimed between head shape and personality by phrenology) may have once been accepted by science but are today rejected by it. Science must necessarily be selective in its attention to the empirical world, but the ultimate goal of science must be to finally account for what Charles Fort called the "damned facts," which empirically exist but will not fit smoothly into our currently limited theories.

At the same time that we must remain open to new and some-

times uncomfortable facts, it is necessary also to recognize the essentially conservative character of science. The history of science clearly demonstrates that known anomalies have always existed, and their incorporation into our theories has sometimes been a slow process. The presence of an anomaly does not and should not result in our immediately overthrowing a large body of scientific generalization that is useful in explaining most events. Though we should not dogmatically deny the existence of claimed anomalies, we must initially assign them a low probability and take a skeptical stance toward them. And when such claims are extraordinary, that is, revolutionary in their implications for established scientific generalizations already accumulated and verified, we must demand extraordinary proof. This is especially true for claims bordering on the miraculous, for Hume long ago showed us that false witness is a far more likely explanation for a miracle than is the contradiction of an established, lawlike generalization. And when those making the miraculous claims stand to profit from the claims and when the history of such claims has often been connected with fraud and misperception, we have added reason to be highly skeptical and critically questioning. To judge the claims it will often be necessary to obtain special expertise not usually available from a scientist. Thus, we may need to call upon the help of expert deceptionists (e.g., conjurors in the case of psychical research) to help us uncover and evaluate "normal" explanations as alternatives to the paranormal claim. Just as an expert forger might know more about validating the authenticity of a painting attributed to an old master than would the average art historian, so might those with special skills greatly aid science's investigation of paranormal claims. As in all of science, the burden of proof is on the claimant, and any claim for the paranormal must (by the rule of parsimony, which is basic to science) prove itself better than any alternative "normal" explanation.

We hope that the work of the Committee will not be redundant to the scientifically relevant efforts of the numerous other organizations concerned with paranormal claims. We hope to work with and complement and extend their work. Unlike such organizations as the American Society for Psychical Research, we are interested in paranormal claims in many areas (biology, astronomy, medicine, etc), and we begin our investigation with a far more scientifically neutral stance. We do not begin with the assumption that paranormal

events exist and then seek to document them. We will publish negative as well as positive results.

Though it is likely that most of our results will prove negative, out central purpose is not one of debunking paranormal claims. When we come upon positive results, we will publish them, encourage further research and replication, and hope to make some scientists receptive to investigating claims they might earlier have dismissed. And when our conclusions are negative, we hope to shed light on the processes by which past investigators have been misled into false beliefs. If we can thus help prevent future errors, we will have made an important, positive contribution to science that goes much further than mere debunking.

Since this is our first issue, it must consist of one-sided presentations for the most part. We hope that future issues will include many debates and exchanges of viewpoint, both in the articles and in the letters section. Our object is not to close the door on discussions of the paranormal, but to open it to serious and rational debate in the context of the basic ground rules of scientific method. The supporters and Fellows of the Committee are by no means unified in their views on claims about the paranormal. Some who are most critical of claim X may be believers in claim Y. But all agree to allow the basic methods of science to arbitrate the differences.

Since there is wide diversity of opinion among our readership, even among the Fellows of the Committee, it is important to emphasize that viewpoints expressed by those writing in *The Zetetic* and the conclusions of any of the sponsored investigating subcommittees do not speak for the Committee as a whole. The Committee will frequently *sponsor* investigations, but it will never *endorse* research claims. The Committee's primary function is to promote serious research and communication, and it will lend its name to sponsor both. But few research findings—whether positive or negative—will be completely acceptable to all our readers. Our commitment is to open and rational dialogue about paranormal claims from a science standpoint. Our concern is not with being the final arbiters of truth in such matters. By presenting a forum wherein skeptics and claimants can carry on discourse for the benefit of the scientific community, we hope that truth will emerge and that science will benefit.

Finally, a word might be said about our exclusive concern with *scientific* investigation and *empirical* claims. The Committee takes no

position regarding nonempirical or mystical claims. We accept a scientific viewpoint and will not argue for it in these pages. Those concerned with metaphysics and supernatural claims are directed to those journals of philosophy and religion dedicated to such matters.

—Marcello Truzzi
Editor

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

The Committee for the Scientific Investigation of Claims of the Paranormal was organized on 1 May 1976 at an annual meeting of the American Humanist Association devoted to "The New Irrationalism: Antiscience and Pseudoscience." It had as its sponsoring members some twenty-five scientists, authors, and scholars.

The announcement of the Committee has attracted considerable attention both in the scientific community and the press. Many hundreds of scientists and other inquirers have written to us approvingly asking to be apprised of the work of the Committee. Criticisms have also been received attacking the formation of the Committee.

The Committee came into being because of frustration with the widespread growth of belief in the "paranormal." This term is being used to describe not simply parapsychological phenomena, but all claims that go beyond the "normal" range of data. There has been an uncritical acceptance by wide sections of the public of many claims of "paranormal" phenomena as true, even without testing. The Committee was therefore founded to act as a forum for the critical examination of such claims.

Some of the critics of the Committee have accused it of being engaged in an "inquisition" or "witch hunt," of being "biased" or locked in by established scientific views. We regret any such implication. For in our original statement of purposes, published in *The Humanist* magazine, we stated unequivocally: "We wish to make clear that the purpose of the Committee is not to reject on a priori

grounds, antecedent to inquiry, any or all [paranormal] claims, but rather to examine them openly, completely, objectively, and carefully." Although it is true that many of the initial members of the Committee are skeptical of many claims of "paranormal" phenomena, we wish to reiterate that we are committed to the methods of science. Thus (1) we are willing to consider and investigate areas however strange or anomalous they may seem to the existing state of knowledge. We are receptive to the creative formulation of hypotheses, but we insist that (2) to introduce or entertain a hypothesis does not constitute confirmation of it and that (3) there must be sufficient evidence directly or indirectly to verify such hypotheses before they can be accepted. However, we surely do not wish to limit or narrow the range of scientific inquiry and reject possible new sciences or new fields of inquiry.

It is obvious, I hope, that the Committee for the Scientific Investigation of Claims of the Paranormal is committed first of all to the quest for truth, and that we wish to stimulate significant inquiry and to follow it wherever it leads.

In this venture we are ready and willing to work with all those who are committed to scientific techniques and methods, including objective inquiry into new areas of research, and we invite those who are so committed to join in the work of the Committee, to send us information about their studies, to bring to our attention new data and theories. We welcome suggestions about plans and projects that the Committee should undertake.

—Paul Kurtz
Co-Chairman

"Poor Man's Psychoanalysis?" Observations on Dianetics

Roy Wallis

The sociology of marginal medicine is a neglected field of endeavor. There exist only a mere handful of brief accounts of unorthodox therapeutic practices and their development in Western societies. I shall seek, therefore, to provide a contribution to this scant literature, in the belief that such studies are not only of intrinsic interest but also provide insight into a number of strains and tensions generated by advanced industrial societies and into some of the less orthodox methods of coping with them. This essay offers an account of a marginal therapeutic system, Dianetics, which briefly emerged into prominence in the early 1950s.[1]

Dianetics was the discovery of L. Ron Hubbard, an individual with a colorful past who began in 1948 and 1949 to formulate a theory of mental functioning and a therapeutic practice, with which he experimented on a small group of followers in New Jersey. Among these followers was a book publisher and the editor of *Astounding Science Fiction* magazine, John W. Campbell. Campbell, a man of considerable influence on the magazine's substantial readership, had a persistent sinusitis condition that was alleviated by Dianetics. He thereafter actively promoted the theory in his magazine (Campbell 1949, p. 80).

---

1. This article is based on extensive research into Dianetics and Scientology undertaken by the author. This research involved interviews with members and former members, the use of questionnaires, and the examination of a wide range of documentary sources. The study was funded in part by a generous grant from the Social Science Research Council. The research is reported in full in my book (Wallis, 1976a). This article draws heavily on Part II of this book and is an abbreviated and modified version of a longer paper (Wallis, 1976b). I am grateful to Dr. Bryan Wilson, Fellow of All Souls College, for advice and encouragement throughout, and to Professor John Lee, Scarborough College, University of Toronto, for his comments on an earlier version.

Campbell's excitement with this new discovery was readily conveyed to his readers, and inquiries began to flow into the magazine's editorial offices asking for treatment by, or information on, the practice. In April 1950 Hubbard and his associates established the Hubbard Dianetic Research Foundation. In the following weeks a lengthy article by Hubbard appeared in *Astounding* (Hubbard 1950); and, shortly after, he published a book describing the theory and practice. The trickle of inquiries turned into a deluge, and his book *Dianetics: The Modern Science of Mental Health* (Hubbard 1968; original ed. 1950) rapidly became a best-seller.

The theory of Dianetics

Hubbard construed the mind as possessing two parts. The first part, the *analytical mind*, was a thoroughly rational unit that operated with computerlike efficiency and had capacities considerably greater than those exhibited by most human beings. The second part, the *reactive mind*, was thoroughly irrational and interfered with the analytical mind, thereby preventing it from achieving its full potential. The reactive mind had evolved, Hubbard believed, as a means of protecting the delicate machinery of the analytical mind. In the face of pain, emotional trauma, or other threat to the individual's survival, the analytical mind would "shut off," and the reactive mind—a more robust mechanism—would come into operation. The reactive mind was a perfect recording device. It stored all perceptual details of the entire period when the analytical mind was not operating and directed the organism in ways which had, according to information stored in its "memory bank," previously led to the organism's survival. Hence, even during periods of what were normally construed as unconsciousness, the reactive mind would be recording. Known as *engrams*, these recordings contained all the sensory (or what Hubbard referred to as "perceptic") details of periods of pain, unconsciousness, emotional loss, or trauma, and all the associated affect.

If, in the formation of the engram, words were spoken, these words might have a later effect similar to that of a posthypnotic suggestion. If the words were subsequently repeated, the engram would be "keyed-in," or partially restimulated, and could subsequently lead the individual to behave in "aberrated" ways, suffer physical or mental illness, or otherwise impair his capabilities.

One of Hubbard's more radical "discoveries" was that the most important engrams, in terms of their effects on later behavior and perform-

ance, were formed during intrauterine life. He gives examples of this in *Dianetics: The Modern Science of Mental Health*, most of which involve violence of some kind, either directed *at* the fetus in the form of an unsuccessful abortion attempt or between the parents of the fetus. One case which he reports involved 81 abortion attempts, which even Hubbard admits was "an incredible number" (Hubbard 1968, p. 314).

An example of such an engramic situation is described as follows: "Fight between mother and father shortly after conception. Father strikes mother in stomach. She screams (first percepts [sic] are pain, pressure, sound of blow and scream) and he says, 'God damn you, I hate you! You are no good. I'm going to kill you!' Mother says, 'Please don't hit me again. Please don't. I'm hurt. I'm hurt. I'm frantic with pain!' Father says 'Lie there and rot, damn you! Goodbye!' " (Hubbard 1968, p. 262).

Hubbard observes that such an engram can produce disease ("and rot") or lead the "aberree" (i.e., the individual with the engram—in this case, the fetus in later life) to feel other people are no good ("You are no good") and to feel hostility toward them ("I hate you").

The purpose of Dianetic therapy (known as auditing or processing) was to locate and gain access to engrams and to "erase" them from the reactive mind, thus eradicating their effects in the form of psychosomatic illness, emotional tension, or lowered capability, by permitting the analytical mind to operate unimpeded.

Exhausting the reactive mind of engrams would, hence, have a number of highly desirable consequences. The individual would become "self-determined," rather than having his actions determined by his engrams. The analytical mind, being a perfect computer, would always supply the correct answer from information fed into it when relieved of the engrams which led to error. The individual's IQ would rise dramatically. He would be free of all psychological or psychosomatic illness, his resistance to physical illness would be vastly improved, and he would be able to cure himself of other illnesses or injuries much more rapidly. He would, in short, be a "clear." In order to achieve this state, however, it was necessary to locate and release the earliest, or "basic-basic," engram, which had usually occurred shortly after conception.

Therapy

Therapy proceeded in the following manner: The pre-clear (or patient) lay on a bed or couch in a quiet room, while the auditor (or therapist) sat be-

side him. "The auditor tells him to look at the ceiling. The auditor says: 'When I count from one to seven your eyes will close.' The auditor counts from one to seven and keeps counting quietly and pleasantly until the patient closes his eyes. A tremble of the lashes will be noticed in optimum *reverie*" (Hubbard 1968, p. 159).

Hubbard insisted that the process of inducing "Dianetic reverie" was quite different from hypnosis. To ensure against hypnotic suggestion, however, a "canceller" was installed. That is, the pre-clear was told: "In the future, when I utter the word *Cancelled*, everything which I have said to you while you are in a therapy session will be cancelled and will have no force with you. Any suggestion I have made to you will be without force when I say the word *cancelled*. Do you understand?' " (Hubbard 1968, p. 200).

The pre-clear was assured he would be aware of everything that happened. When the pre-clear had entered the state of reverie, the auditor would, after various preliminaries, direct the pre-clear to return to "basic-basic." Generally, however, the basic-basic engram was not located so simply, and other, "later-life" engramic material would be brought up. This material had to be "reduced," that is, the pre-clear was asked to return to the beginning of the incident and to recount all the perceptual detail involved in it. He would be directed to recount the incident over and over again, until all the emotion involved in it was discharged.

The pre-clear would then be directed to another, it was hoped, earlier, incident, and the process would be repeated. Ideally, "basic-basic" would be located and erased and the pre-clear then progressively cleared of all subsequent engrams. Often, however, this would not occur, and it would therefore be necessary to end the session at some convenient point. That point was usually after the "reduction" of an engram, that is, when all the affect associated with a particular incident was discharged. A Dianetics session usually lasted for around two hours, but it might continue for much longer if the pre-clear was "stuck in an incident," that is, in an engram.

At the end of the session the pre-clear would be told to "come up to present time." The auditor might then question him as to the time, location, and so forth to ensure that he was "in present time." He would then say "Cancelled" and end the session. "(Work continues until the auditor has worked the patient enough for the period) . . . Come to present time. Are you in present time? (Yes) (Use canceller word.) When I count from five to one and snap my fingers you will feel alert. Five, four, three, two, one. (Snap)" (Hubbard 1968, p. 202).

Dianetics as a cure for illness

Hubbard viewed Dianetic therapy as a uniquely efficacious means of resolving psychosomatic illnesses (which he believed to include *at least* 70 percent of all known illnesses). "Arthritis, dermatitis, allergies, asthma, some coronary difficulties, eye trouble, bursitis, ulcers, sinusitis, etc. form a very small section of the psychosomatic catalogue. Bizarre aches and pains in various portions of the body are generally psychosomatic. Migraine headaches are psychosomatic, and with the others, are uniformly cured by dianetic therapy. (And the word *cured* is used in its fullest sense)" (Hubbard 1968, p. 92).

Even the common cold was psychosomatic, and those cleared by Dianetics, it was said, did not suffer from colds (Hubbard 1968, p. 92). Further, "a number of germ diseases are predisposed and perpetuated by engrams. Tuberculosis is one " (Hubbard 1968, p. 92). So, there are few ills to which mind or flesh are heir that could not be helped by Dianetics.

While Hubbard claimed only to be concerned with producing a theory of the mind and of illness formation on a functional basis and said that structural explanations for the phenomena generated by his practice, or for its success, would follow, he occasionally suggested structural hypotheses to guide future researchers: "Arthritis of the knee, for instance, is the accumulation of all knee injures in the past. The body confuses time and environment with the time and environment where the knee was injured and so keeps the pain there. The fluids of the body avoid the pain area. Hence a deposit which is called arthritis" (Hubbard 1952, p. 32).

Such matters, however, were of less concern to Hubbard than was producing a theory and method which worked.

Dianetics was initially presented as a practice which anyone of normal intelligence could successfully undertake. A thorough knowledge of the book *Dianetics: The Modern Science of Mental Health* was thought to be all that was necessary, and hence interested persons could "co-audit" each other on the basis of their knowledge of this work (Hubbard 1968, p. xxii). Professional courses were soon established, however, and those interested were encouraged to undertake four weeks of training at a fee of 500 dollars to secure certification as a Hubbard Dianetic Auditor.

Membership and motivation

While Hubbard's book drew many followers briefly to Dianetics, most abandoned it as rapidly as they had taken it up. A core of followers re-

mained, however, and concerning these there is available a certain amount of demographic and motivational data.[2] The most heavily represented age group in Dianetics was that from 27 to 40 years of age, with the average age estimated to be about 38 years. The sex distribution showed a clear male overrepresentation, and the occupations of followers displayed a marked clustering in white-collar occupational categories. Manual workers were not typical, and indeed the occupational data show a marked tendency for members to have had professional, semiprofessional, or other nonroutine white-collar occupations. As one would therefore expect, the educational level of Dianeticists was higher than that of the general population. Most had completed secondary education, and a large proportion had attended a college or university. Further, available data show that Dianeticists were predominantly consumers of science-fiction literature; they were largely Protestants or agnostics; and many had already acquainted themselves with at least one quasi-philosophical-psychological system, such as Count Alfred Korzybski's General Semantics. They tended also, almost without exception, to be white.

The divergence from the general pattern of followers of other marginal therapeutic movements in Western societies, in which females are typically overrepresented, can be accounted for by the broader scope of Dianetics. Although heavily promoted as a therapeutic system, it did not restrict its domain to the area of physical healing. Rather, its therapeutic capacity was seen as particularly relevant to psychosomatic illness and psychological improvement. Thus much of its appeal was as a lay psychotherapy and self-improvement system rather than merely as a means of curing chronic physical illness. Motivations for taking up Dianetics can be seen as falling into three analytically distinct categories.

1. *The problem solver.* Most individuals recruited to Dianetics were preoccupied with a concern for self-improvement, based upon an acute awareness of their failure to attain the standards of achievement in some areas of life that were approved and reinforced in the society around them. Their concern might be for improvement of a physical, psychological, or social kind. The search for therapeutic efficacy was a prominent source of motivation for many of those who were attracted to Dianetics. Dianeticists sought relief for themselves or other members of their families from

2. The demographic data derives primarily from a survey conducted by a Dianetic newsletter and reported in *The Dianews* 1, 23 (15 June 1952). The motivational data derive from my own documentary analysis, interviews, and questionnaires.

conditions as diverse as cancer, schizophrenia, and agoraphobia.

A large number of Dianeticists, although it is impossible to say what proportion of the total, had a strong sense of capacities latent within them which could be brought to the surface if only a method could be found. An English respondent, for example, indicated that he had hoped Dianetics would prove the solution to his "sense of frustration at not being able to fully use talents I possess." Others admitted to having a deep sense of inferiority or insecurity, which they hoped Dianetics would enable them to overcome.

2. *The truth seeker.* Many individuals were attracted to Dianetics when they came upon it at some point during a lifelong search for meaning and truth. During the course of this search they had often examined the literature of popular philosophy and psychology, of religion, metaphysics, and occultism. Science fiction, with its panoramic vision of man, time, and the cosmos, also provided many with insight into the meaning of life and human behavior. Dianetics, with its assertive claims to infallibility, offered to answer many of the questions which puzzled such individuals and offered a practical and easily operationalized technique to put into effect the truths which it had uncovered. Others came into the orbit of Dianetics because a member of an amateur psychology or philosophy discussion group with which they were associated drew their attention to it.

3. *The career oriented.* A small proportion of individuals were attracted to Dianetics as the source of an alternative career as a professional Dianeticist, or by the possiblity that Dianetics was a revolutionary new therapeutic tool which would greatly improve their current practice as therapists, orthodox or unorthodox.

Societal reaction

While the response of the book-buying public rapidly placed Hubbard's book on the best-seller list, it was not everywhere received with enthusiasm. Reviews by psychologists and psychiatrists were almost uniformly unfavorable. Objections were raised to its monocausal determinism and grandiose promises and to its claims to scientific exactitude and an extensive experimental background; and fears were voiced that the severely ill might fatally waste time in Dianetic therapy before seeing a doctor.[3] More sympathetic reviewers suggested that Dianetics was harmless enough and

3. Among the hostile reviewers were Rollo May (1950) and Martin Gumpert (1950).

might possibly even be of help to socially isolated individuals.[4] The benefits of the pre-clear's having a sympathetic listener while he ventilated his problems was recognized by some reviewers, who nevertheless remained concerned about the effects this might have in untrained hands in cases of severe mental disorder (Anonymous 1951a, p. 2). Although some of these reviews may have attracted people to Dianetics, it was the view of informed Dianeticists that the reviews in the larger-circulation periodicals and newspapers were generally so unfavorable that they led many to fall away.

Apart from numerous marginal, limited, and quasi-medical converts, Dianetics was received coldly by the medical, psychiatric, and psychological professions. Dr. Gregory Zilboorg publicly attacked Dianetics before a forum at the New York Academy of Medicine (Anonymous 1951b, p. 6), and a resolution of the American Psychological Association calling on psychologists not to employ Dianetic techniques in their therapy was widely reported (Anonymous 1950a, p. 2). Dr. Joseph Winter, medical director of the first Foundation, attempted to interest his professional colleagues in Dianetics, but with little success. And Dr. Morris Fishbein, a spokesman for the American Medical Association, castigated Dianetics as yet another "mind-healing cult" (Anonymous 1950b).

Some in the medical profession clearly held the view that there was a need for more-active steps to be taken to deal with what was seen by some doctors as a form of quackery. In January 1951 it was reported in a *Bulletin* of the Dianetic Foundation that the New Jersey Board of Medical Examiners had initiated an injunction, which was later vacated, against the Foundation, for conducting a school of medicine without a license. It was almost certainly as a result of the publicity given to this action that creditors of the Foundation began to demand settlement, which led to reorganization and centralization of the Foundation in Wichita, Kansas.

The decline of Dianetics

The major external source of the decline of Dianetics was the fall-off in numbers of new recruits, which led in turn to a financial crisis. The central organization of Dianetics was poorly administered. Hubbard was lecturing in various parts of the country and commuting between Los Angeles and New York during late 1950 and early 1951, and giving little direction to

4. More sympathetic was the review by Willard Beecher and Calder Willingham (1951).

either of these Foundations in their day-to-day administration. He progressively alienated other board members by his practice of initiating developments without consulting them and by what some of them viewed as his increasingly evident authoritarianism.

Large numbers of staff were recruited in the early months without adequate supervision. Foundation income was expended on the assumption that the Dianetics boom would long continue. However, by the beginning of 1951 applications for training and therapy began to drop off and income fell correspondingly. In part, as I have argued, the decline in numbers of new recruits to Foundation services was precipitated by attacks on Dianetics in the press by doctors and psychiatrists and by scathing reviews of Hubbard's book.

Recruitment may also, to some extent, have been affected by adverse publicity resulting from a divorce action in which Hubbard was involved. In particular, however, the decline in numbers was due to the failure of Dianetics to live up to its promise in the eyes of its early public. The state of "clear" that would, it had been promised, emerge after only 20 hours of auditing had not made its appearance, and many individuals who had been working at the technique found their "cases" had improved little or not at all and gave up.

Yet another reason for attrition was the presentation of Dianetics as a psychotherapy. Whatever their feelings about the state of "clear," many people had gone into Dianetics to solve relatively specific problems of illness or psychological handicap. Whether through spontaneous remission, the hope given them by Dianetics, the attention they received as preclears, or the therapeutic validity of the practice, a number *had* felt improved. Having secured what they had wanted from Dianetics, some discontinued their involvement.

The Foundation moved toward a financial crisis. Hubbard's co-directors began to resign and the creditors to press for payment. Don Purcell, a Wichita businessman and ardent Dianeticist, offered to assist the Foundation in getting out of its difficulties. The corporation was centralized and its assets moved to Wichita, Kansas, in April and May 1951, where Purcell made funds and a building available. Purcell became president of the Foundation and Hubbard its vice-president and chairman of the board of directors. The other branches were closed down and the number of staff drastically reduced. The New Jersey creditors, however, pressed for settlement of the original Foundation's debts, and a court decision declared the Kansas operation liable as its legal successor. A

receiver was appointed. A compromise settlement of the claims was negotiated, but new claims were filed by other creditors.

Hubbard broke with his colleagues at this point (they then declared the Foundation bankrupt) and moved to Phoenix, Arizona, where he publicized new developments in his theory and practice, which he called Scientology. Scientology, far from being a lay psychotherapy, developed progressively into a highly professionalized religious philosophy and was shortly incorporated as a church.

Self-improvement and healing cults

Dianetics has a place in a continuing tradition of self-improvement movements in the recent history of the United States. Enormously accelerated social mobility and a prevalent ideology of individual achievement led to a concern for infallible techniques that would ensure success for the mobility-oriented. The late nineteenth and early twentieth centuries saw the appearance of various movements and organizations offering access to advanced, occult, metaphysical, or otherwise esoteric knowledge, and some which, more cynically, merely offered certification that access to such knowledge or training had been obtained. The "diploma mill" became an established, if derogated, institution. Movements such as New Thought suggested that prosperity and success were available to everyone. The use of a few simple techniques would enable anyone to overcome the limitations which he believed held him back (Griswold 1934).

Dianetics also found a place in the continuing tradition of healing movements in the United States. Indeed, the two traditions overlapped to a large degree—movements within this domain offering both healing and self-improvement and certifying "professional" competence in the practices purveyed (Steiner 1945). The development of science, particularly medical science, during the nineteenth century led, John Lee has cogently argued, to increased expectations regarding physical health and comfort (Lee 1970, pp. 5-7). These expectations were in excess of what medicine could actually achieve.

The new healing movements such as Christian Science and New Thought offered a means of overcoming this gap between expectation and performance in the realm of physical healing. Medicine became increasingly specialized and compartmentalized, and allopathic medicine directed attention to the disease rather than the individual, which led to a depersonalization of the practitioner/client relationship. The new healing

movements, on the other hand, retained a personal orientation, a concern for the "whole man." Hence, it has been argued that the role of the practitioner in such movements is closer to that of the psychotherapist than to that of the medical practitioner.

As, during the early twentieth century, medical practice became more competent to deal with physical illness, expectations of health and well-being became increasingly centered on the psychological domain and the difficulties of interpersonal relations. Movements, like Christian Science and New Thought, that had claimed efficacy in handling physical illness lost ground, while others arose offering psychological well-being, release of mental and emotional tension, cures for psychosomatic and neurotic illness, techniques for releasing hidden inner abilities, and means of "making friends and influencing people." In such areas science has as yet made little concrete progress, and the market remains open to cultic groups offering knowledge and techniques produced by more mystical, occult, or pseudoscientific means. Whatever the source of such knowledge, the prestige of science has become such as to require that almost every new movement entering this field claim scientific legitimacy and authority, if by no other means than that of incorporating the word *science* in its title. [5]

Discussion

Dianetics struck a powerful resonance in the minds of many Americans in the post-World War II period. It offered a rationale for failure in social mobility and in social interaction. It provided an explanation for this failure in terms of traumatic incidents in which the individual had been unwittingly involved and thereby relieved him of responsibility for his failure. All the past mistakes, failures, and sources of guilt could be wiped out. But most important of all, it offered one a means of eradicating the persisting causes of failure, and thus of attaining the level of achievement to which he aspired. In a status-striving age it provided a means of improving the individual's chances of status mobility. The theory of Dianetics assured its adherent that his "true self," his conception of what he believed he was really capable of achieving, was indeed as he conceived it. It reaffirmed this idealization of self and promised a means of eliminating the barriers to its fulfillment, of eradicating the gap between his "true

5. For example, Science of Mind, Science of Creative Intelligence, Christian Science, *Dianetics: The Modern Science of Mental Health.*

self" and the identity that was typically confirmed in social interaction. Moreover, Dianetics provided a means for understanding not only oneself but also for understanding others, a way of categorizing and accounting for their behavior and a guide to appropriate responses.

Dianetics seems to have been seen as an acceptable and legitimate solution to the problems with which recruits were faced, for two reasons: either they had tried alternative systems of belief and practice and found them unsuccessful; or they had rejected such alternative systems as inappropriate to their situation.

Many of those whom I interviewed claimed an acquaintance with the literature of psychology and expressed dissatisfaction with it. Psychology, as far as they could see, in the 1940s was split between behaviorism and psychoanalysis. Behaviorist psychology seemed to them to have little or no relevance to man in general and no solution to their problems in particular. Psychoanalysis, while it addressed many of the problems which they faced and offered solutions, had two major drawbacks. First, analysis seemed an inordinately lengthy process, often lasting several years. Second, it was too expensive for most to consider it a practical proposition.

Those who were suffering physical ills or disabilities had generally tried medical means of overcoming them but found little satisfaction from medical professionals, few of whom recognized the essentially psychological or social basis of many of the complaints presented to them. Ill-equipped through lack of training to cope with the needs of such patients, they resorted to pharmacological or surgical treatments that, while successful in some cases, left others feeling the need for a treatment practice which took greater account of man as a whole. Other individuals, suffering chronic illnesses for which medical treatment had proved unsuccessful or from illnesses for which effective therapeutic interventions had not yet been discovered, had exhausted all the resources that orthodox medicine could offer. Individuals suffering both physical and mental problems had generally sought solutions in a variety of other therapeutic practices before they came in contact with Dianetics.

Bureaucratization and the scale of modern, urban society produce a context in which many individuals experience a lack of control over their destiny and environment, a sense of being moved and constrained by forces beyond their control. Many of those adherents of Dianetics who did not conceptualize their situation in medical or psychological terms experienced the world in which they lived as more or less unpredictable, chaotic, or meaningless. They sought some means of greater control over

their environment and their reaction to it. Related to this, a small proportion were engaged in therapeutic work of a limited or marginal kind and saw considerable limitations in the tools they had available. A further small proportion claimed a simple intellectual curiosity, which had earlier led them to other systems of self-improvement, metaphysical, or occult knowledge.

While science held great promise, having delivered technological "cargo," and having proved a powerful tool in the improvement of material conditions, it had done little to solve perennial and increasing problems of psychological well-being, to provide cures for certain forms of illness, or to equip man better to cope with his social environment. Dianetics followers tended to conceptualize appropriate solutions to such problems as being "scientific" in form. Their conception of science was, however, a lay conception. It was technological and instrumental in character. What constituted a science was a body of knowledge which appeared to explain some set of phenomena in a rational and consistent way and which provided a means of intervening in the processes involved so as to achieve successful or desired outcomes. Their test of the standing of any body of knowledge was: Does it work? That is, do interventions of the prescribed form issue in the desired outcomes? When after a Dianetics session they felt better than before, they concluded that it did work.

They tended to expect that new and important scientific developments would appear through media or institutions marginal to the scientific and orthodox medical communities. They conceived of these communities as elitist groups with vested interests in the promotion of particular theories and practices and unwilling to accommodate new ideas or even to give them a fair hearing. Hence the innovator would generally need to find a more marginal institutional base in order to get his revolutionary new thoughts heard.

Dianeticists apparently believed in the *immanence of knowledge*—that knowledge was freely available and that anyone who applied himself might expect to secure radically new or deeper insights into the nature of the world. They also believed in the *elitism of science*—that scientists were unwilling to permit any radical challenge to their own views. Since orthodox science was so conservative on this account, the intellectually curious might seek truth in less orthodox realms—in metaphysical or occult groups, marginal healing, philosophical or psychological movements, or science fiction. Science fiction provided all that science lacked, filling in the lacunae of scientific knowledge or competence with fictional or specu-

lative detail and blurring the distinction between the empirical and the conceivable. Converts to Dianetics were mobilized to accept an unorthodox system of belief and practice by the urgency of their need, which orthodox systems had been unable to meet, or by a conviction that radical developments in knowledge were to be anticipated outside the domain of the institutions of orthodoxy, which lacked the vision to generate them.

John W. Campbell, as editor of *Astounding Science Fiction,* was an influential figure in the science-fiction world and its environs. His readership saw him as a man of vision, willing to give any idea a hearing. When Campbell gave his support to Dianetics, interest in the movement was aroused because of his prestige and his enthusiastic acclaim of this new "science of the mind." With the publication of Hubbard's writings, the idea of "clear," like that of "flying saucer" a few years earlier, became a kind of Rorschach blot, a vague and amorphous image upon which any individual could impose his aspirations (Buckner 1965). Being clear, however Hubbard might define it, meant being able to do all those things which one currently could not do and to which one aspired so desperately.

Despite its initial impact, however, Dianetics foundered. It was to reemerge later as a subcomponent of Hubbard's much more successful and enduring movement, Scientology; but although it reached craze proportions in 1950, by 1952 it had effectively disappeared. Why had this happened?

Paradoxically, one reason for its demise would seem to have been the very popularity of the idea. Unlike chiropractic and osteopathy, which maintained the necessity for specialized training, Dianetics was initially promoted as a lay psychotherapy which any two reasonably intelligent people could conduct on each other on the basis of Hubbard's book. Thousands bought the book, tried the practice, and then, just as readily, abandoned it. Those who remained were fiercely jealous of their independence, resisting control by the central organizations, and introducing new theories and techniques or eclectically combining Dianetics with other practices. Hence Dianetics did not spread on the basis of trained professionals with a commitment to the practice as it had been revealed to them carrying out their practice on an uninformed clientele to whom the "mystery" was unavailable. Early in its history, therefore, Dianetics was riven by competing schools and factions which challenged not only each other's authority but that of the movement's founder as well. Thus the financial crisis which led to the bankruptcy of the central organization and the crisis of authority resulting from the presentation of Dianetics as a lay

psychotherapy in which anyone could become an expert combined to cause the movement's demise.

Hubbard was to demonstrate his understanding of the lessons implicit in these developments when he established Scientology. Scientology was organized from the outset in a highly centralized and authoritarian fashion and was practiced on a professional basis. Its theory and method were only gradually revealed to those who displayed commitment to Hubbard and practiced its techniques in a pure and unalloyed fashion. A rigorous system of social control emerged, and it was made clear to all followers that Hubbard was the sole source of new knowledge and of interpretation of existing knowledge. It has therefore succeeded, up to the present, in avoiding the fissiparousness which had overtaken Dianetics.

REFERENCES

Anonymous 1950a. "Psychologists Act Against Dianetics." *New York Times,* September 9, p. 7.

Anonymous 1950b. "Poor Man's Psychoanalysis." *Newsweek,* October 16, pp. 58-59.

Anonymous 1951a. "Dianetics." *Consumer Reports,* August, pp. 378-80.

Anonymous 1951b. "Dr. Zilboorg Attacks Dianetics." *New York Times,* March 30, p. 15.

Beecher, Willard, and Calder Willingham 1951. "Boiled Engrams." *American Mercury,* no. 73 (August): 7.

Buckner, H. Taylor 1965. "The Flying Saucerians, a Lingering Cult." *New Society,* September 9.

Campbell, John W. 1949. "In Times to Come." *Astounding Science Fiction* 44, no. 4 (December): 80.

Griswold, A. W. 1934. "New Thought: A Cult of Success." *American Journal of Sociology* 40, no. 3: 308-18.

Gumpert, Martin 1950. "The Dianetics Craze." *New Republic,* no. 132, August 14, pp. 20-21.

Hubbard, L. Ron 1950. "Dianetics: The Evolution of a Science." *Astounding Science Fiction* 45, no. 3 (May): 43-87.

Hubbard, L. Ron 1953. *Self-Analysis in Dianetics.* London: Derricke Ridgway.

Hubbard, L. Ron 1968. *Dianetics: The Modern Science of Mental Health.* East Grimstead, Sussex: Hubbard College of Scientology. (Original edition published 1950 by Hermitage House, New York.)

Lee, John A. 1970. *Sectarian Healers and Hypnotherapy.* Toronto: Queen's Printer.

May, Rollo 1950. "How to Back Track and Get Ahead." *New York Times Book Review*, July 2.

Steiner, Lee R. 1945. *Where Do People Take Their Troubles?* Boston: Houghton Mifflin.

Wallis, Roy 1976a. *The Road to Total Freedom: A Sociological Analysis of Scientology.* London: Heinemann Educational Books.

Wallis, Roy 1976b. "Dianetics: A Marginal Psychotherapy." In Roy Wallis and Peter Morley, eds., *Marginal Medicine.* London: Peter Owen, Ltd.

Psychics, Clairvoyance, and the Real World: A Social-Psychological Analysis

Gary Alan Fine

The literature on parapsychology is filled with studies which seek to confirm or deny the existence of parapsychological phenomena (Rosenthal 1973; Murphy 1961; and Carrington n.d.). The ideal in these experiments has been to conduct the research under perfectly controlled conditions, so that no partisan can attack the results of the study as due to an artifact (Rosenthal and Rosnow 1969) or deception by the experimenter (Hansel 1966). This is essential for the proper determination of whether these effects—ESP, telepathy, clairvoyance, PK, and so on—exist and operate in the manner in which their practitioners claim.

A second, more "conventional" social-scientific approach has been to determine the nature of predictions of "normal" subjects. McGregor (1938), having collected 3500 predictions from 400 college students, noted that the most important element in accuracy of prediction is the *quality* of information that a person has available, and that both the amount of information an individual is aware of and his "sophistication" are less important. Hadley Cantril (1938), in expanding McGregor's approach, maintained that predictions give structure to current events and provide closure for unfinished events.

Little attention has been given to the systematic study of clairvoyance outside the laboratory. Many people claim that they have powers which are considered "psychic or extrasensory." Whether they claim that these powers derive from God (Dixon 1969), from nature, the devil, or elsewhere, it is appropriate to test their claims and to understand the nature of their social roles. However, a negative finding need not cast doubt upon the phenomenon itself, but only on those who are making the claims.

Of particular interest are individuals who make a career of clairvoyance, such as Jeane Dixon or Edgar Cayce. It should be possible to determine how well they predict the future compared to those who do not

profess any special abilities. No hypotheses were proposed as to which group would most consistently predict the future, but it was hypothesized that psychics would make qualitatively different predictions than non-psychics, and that raters could differentiate between the two sets of predictions.

Those who engage in prophecy (Agee 1969; and Dixon 1969) can point to specific events which "prove" their powers; cynics can point to equally specific events which make the prophet seem foolish or ludicrous. Some tests seem necessary to determine the accuracy of the claims and what such accuracy implies for the roles that psychics occupy.

Method

1. *Subjects.* Subjects for this study were ten well-known professional psychics and ten Harvard and Radcliffe upperclassmen. The psychics made predictions for the year 1973 in a national weekly tabloid (*National Enquirer,* 14 January 1973).[1] During early February 1973 predictions for 1973 were obtained from the undergraduates.

2. *Procedure.* The college students were asked to imagine: "You are a well-known psychic who believes in his or her power to predict the future. You are asked by a national magazine for predictions about the coming twelve month period. . . . What events do you expect to occur? Include approximately six predictions in your forecast."

They were also asked whether they believed that some people have the ability to predict the future.

3. *First coding.* It was determined that none of the psychics' predictions had proven accurate by 10 February 1973, so that date was used as the arbitrary starting point for the yearlong study. This gave the psychics over 13 months in which their predictions could come true, a slight bias in their favor. One hundred twenty predictions were collected—59 from the psychic group and 61 from the nonpsychic group.

Five students at Harvard were then asked to rate the 120 predictions according to the following instructions: "Listed on the other pages are a number of predictions—some made by psychics, some made by people who do not consider themselves psychic. On Sheet A for each prediction please score from 1 to 10 how likely you believe the prediction is to occur before 10 February 1974. (Ten is: extremely likely to occur; one is: extremely unlikely to occur.) Then go through the predictions again, and

1. In fairness it should be noted that Jeane Dixon was not in the sample.

mark on Sheet B how likely on a one to ten scale you believe it to be that the statement was made by a psychic. (Ten is: undoubtedly made by a psychic; one is: undoubtedly made by a nonpsychic.) Please answer for all predictions, even if some seem redundant or obvious."

4. *Second coding.* In February 1974 five other students at Harvard were asked to rate the predictions according to these instructions: "Listed on the other pages are a number of predictions—some made by psychics, some made by people who do not consider themselves psychic. On Sheet A for each prediction please score from one to ten how likely you believe it to be that the prediction occurred between 10 February 1973 and 9 February 1974. (Ten is: extremely likely that it occurred; one is: extremely unlikely that it occurred).

"Then go through the predictions again, and mark on Sheet B how likely on a one to ten scale you think it to be that the statement was made by a psychic. (Ten is: undoubtedly made by a psychic; one is: undoubtedly made by a nonpsychic.) Please answer for all predictions, even if some seem redundant or obvious."

Results

1. *Differences between psychics and nonpsychics.* T-tests (see Table 1) were run between the predictions of the professional psychics and the students for each of the four coded variables: (1) 1973 ratings of how likely it was that the event would occur (Pred73); (2) 1974 ratings of how likely it was that the event did occur (Pred74); (3) the 1973 rating of the likelihood that the prediction was made by a psychic (Psy73); and (4) the 1974 rating of the likelihood that the prediction was made by a psychic (Psy74).

All t-tests indicated a high level of significance. Psychics gave predictions that were originally considered less likely to occur and did occur less often than those of nonpsychics. In both cases coders were able to identify which predictions were provided by psychics.

2. *Differences between males and females.* A series of t-tests (see Table 2) produced results which were not different from chance on all four measures for males and females. Apparently the sex of the predictor is not an important factor in the accuracy or "psychicness" of the claim.

3. *Differences between believers and nonbelievers in psychic ability.* All nonpsychic subjects were asked whether they felt it was possible for people to predict the future. No significant differences were found on the four variables measured by t-tests between believers and nonbelievers in the possibility of clairvoyance. (See Table 3.)

Table 1
Effects of professed psychic ability

Group	Psychics	Nonpsychics
n	59	61
Pred73	3.08	4.58
SD	1.35	1.67
t	-5.32*	
Pred74	3.57	5.45
SD	2.43	2.93
t	-3.78*	
Psy73	7.29	4.97
SD	1.59	1.42
t	8.35*	
Psy74	6.10	4.35
SD	1.43	1.52
t	6.43*	

Table 2
Effects of sex

Group	Male	Female
n	54	66
Pred73	3.94	3.77
SD	1.90	1.50
t	.31	
Pred74	4.68	4.41
SD	2.86	2.85
t	.52	
Psy73	5.90	6.28
SD	1.92	1.86
t	-.35	
Psy74	5.11	5.29
SD	1.63	1.78
t	-.31	

Table 3
Effects of belief among nonpsychics

Group	Believers	Nonbelievers
n	29	32
Pred73	4.59	4.57
SD	1.77	1.57
t	.03	
Pred74	5.35	5.55
SD	2.96	2.90
t	-.25	
Psy73	4.86	5.07
SD	1.45	1.38
t	.57	
Psy74	4.36	4.33
SD	1.23	1.73
t	.07	

* p less than .001

4. *Correlations.* Correlations between the four measures indicate several interesting relationships. (See Table 4.) There was a strong positive correlation between ratings of how likely the prediction was to occur in 1973 and whether it was judged in 1974 as having occurred, which indicates that the 1973 judgments of likelihood of occurrence had validity. Judgments of whether the predictions were made by a psychic were highly

Table 4
Correlations

	Pred73	Pred74	Psy73	Psy74
Pred73	—			
Pred74	.572*	—		
Psy73	-.739*	-.444*	—	
Psy74	-.350*	-.155	.495*	

n = 120 for all r's
* p less than .001

positively correlated between the tests of 1973 and 1974. A negative correlation occurred between the 1973 likelihood-rating and the likelihood of the prediction's being made by a psychic—that is, those predictions that were judged in 1973 least likely to occur were judged to be those of the psychics. The eventual accuracy (Pred74) of a prediction and the 1973 ratings of whether the prediction came from the psychic (Psy73) were significantly negatively correlated, while the 1974 (Psy74) ratings were not.

5. *Spearman and Brown effective reliability measure.* Following the method described by Robert Rosenthal (1973), intercorrelations of the four variables were computed to determine the effective level of reliability between the judges. Effective reliabilities were: Pred73, R=0.84; Pred74, R=0.83; Psy73, R=0.81; Psy74, R=0.48. Reliabilities were considered acceptable for the first three variables, but sufficiently low for the fourth that some care needs to be exercised in interpreting that data.

6. *Regression analysis.* The results indicated that although psychics are less often correct, they tend to make predictions that have less original likelihood of coming true, that is, predictions that are more difficult. A multiple regression was performed, with prediction accuracy (Pred74) as

the dependent variable. (See Table 5.) The analysis indicated that the major source of variance was the original likelihood of the prediction (Pred73; unique variance: 22.7 percent). Whether the prediction was

Table 5
Regression analysis for prediction accuracy(Pred74)

Name	Coefficient	STD Coefficient	t-test	dF	Unique Variance
Pred73	.89	.53	6.32*	117	.227
Psychic (1=yes/2=no	.54	.09	1.14	117	.007
Constant	.27		R^2=.335		

* p less than .001

actually made by a psychic (yes/no) had an insignificant t-test associated with it (t=1.14; n.s.) and a small unique variance (0.7 percent). The judges' estimates of whether a prediction was made by a psychic (Psy73, Psy74) each increased the R^2 by less than 0.01. This indicates that when controlling for difficulty of prediction there are no significant differences between psychics and nonpsychics, though despite the controls, it seems nonpsychics are slightly better able to predict the future.

7. *Normal probability plot.* Because it could be argued that one reason no differences were found was that most of the psychics could not predict the future but that *some* could, two normal probability plots were performed. One looked at accuracy of predictions, and the other looked at the residuals of the regression equation. Neither plot indicated that any psychic or nonpsychic was predicting at a level significantly worse than the others.

Discussion

The evidence indicates that not all those who claim psychic powers have them to any noteworthy extent. Proving that someone does not have psychic ability, in this case clairvoyance, is like proving the null hypothesis. Those who believe in parapsychology freely admit (Koestler 1972)

that not all people have psychic ability. Indeed it is not my intention to argue that clairvoyance does not exist. Many laboratory studies have shown beyond a reasonable level of statistical significance that parapsychological processes are operating; however, systematic research on this as a *social* phenomenon has been minimal.

This study indicates that many self-described "psychics" may not be psychics, but perhaps entertainers, well-meaning believers in their own power, or frauds. It must alternatively be noted that perhaps these individuals were not giving their "psychic's" predictions in the newspaper—though this explanation does not seem consistent with their professed claims of psychic ability.

The results, coupled with the belief in psychics in many segments of our society (Truzzi 1972), lead to the realization that claims of psychic ability are a social phenomenon and should be studied as such. One need not conclude that psychics have less ability to predict the future than nonpsychics—though one could argue that university students might possibly be better informed about current events and thus in a better position to provide "reasonable" guesses about the future.

The evidence indicates that being a psychic involves learning how to make "psychic" predictions. It was discovered that predictions of psychics follow a predictable pattern. The dimensions in which their predictions differ from nonpsychic predictions are not clear—though one certainly seems to be their likelihood of occurring.

One element which may provide credibility for the psychic is his self-confidence in making unlikely predictions. Nonpsychics, when asked to make predictions, choose events which have a significantly greater likelihood of occurring, at least as perceived by others. The fact that predictions by nonpsychics seem more reasonable is an important element in being able to perceive accurately whether the statement was made by a psychic or nonpsychic. Other factors which should be tested are the "exoticness" of the predictions and their dramatic or "cataclysmic" nature—for example, predictions of great advances in technology and major tragedies, such as assassinations. Certain key phrases are used more often by psychics than by nonpsychics, for example, "X" will be in the "headlines." Psychics do not randomly choose their predictions but use, perhaps subconsciously, certain formulas and "tricks of the trade." Being a psychic is a "career" (Goffman 1961). Just as faith healers, stockbrokers, and political analysts must learn to make predictions which sound possible though not obvious, psychics must learn the proper level of credibility for which to strive.

Psychics serve a function in our culture. People have a desire to reduce the uncertainty in their lives—these predictions point up occurrences which are possible and otherwise unexpected. By noting events which might occur, they may serve to relieve tension. By making the future somewhat more "predictable and less problematic," anxiety about the unknown is reduced.

Summary

None of our psychics or nonpsychics predicted significantly better than any other psychic or nonpsychic—whether we were looking at actual predictions or at predictions controlling for difficulty and professed psychic ability.

There seems to be great consistency between the rated accuracy of predictions in 1973 and their judged accuracy in 1974. Similarly, those predictions judged in 1973 to have been made by a psychic were judged in 1974 to have been made by a psychic. The analysis indicates that there was considerable accuracy in these judgments.

The evidence indicates a need for further social-psychological work in several separate and relatively unexplored areas. First, more research should be devoted to applied parapsychology, in conjunction with laboratory research. Second, research should be conducted on the nature of professional psychics, *not* to determine if they are deviants, frauds, or hucksters, but to determine what is necessary for a successful public "career" in clairvoyance. Third, there is need for study of the structure and function of belief in parapsychology. The social psychology of parapsychology has been ignored.

REFERENCES

Agee, Doris 1969. *Edgar Cayce on ESP.* New York: Warner.

Cantril, Hadley 1938. "The Prediction of Social Events." *Journal of Abnormal and Social Psychology* 33: 364-89.

Carrington, Hereward n.d. *Laboratory Investigations into Psychic Phenomenon.* Philadelphia: David McKay.

Dick, W. and J. Kelley 1973. "Ten Leading Psychics Reveal Their Predictions for 1973." *National Enquirer,* January 14.

Dixon, Jeane 1969. *My Life and Prophecies.* New York: Bantam.

Goffman, Erving 1961. *Asylums*. New York: Anchor Books.

Hansel, C. E. M. 1966. *ESP: A Scientific Evaluation*. New York: Scribner.

Koestler. Arthur 1972. *The Roots of Coincidence*. New York: Vintage.

McGregor, D. 1972. "The Major Determinants of the Prediction of Social Events." *Journal of Abnormal and Social Psychology* 33: 179-204.

Murphy, Gardner 1970. *Challenge of Psychical Research: A Primer of Parapsychology*. New York: Harper & Row.

Rhine, J. B. 1964. *Extra-sensory Perception*. Boston: Bruce Humphries.

Rosenthal, Robert 1973. "Estimating Effective Reliabilities in Studies that Employ Judges' Ratings." *Journal of Clinical Psychology* 29: 342-45.

Rosenthal, Robert, and Ralph L. Rosnow, eds. 1969. *Artifact in Behavioral Research*. New York: Academic Press.

Truzzi, Marcello 1972. "The Occult Revival as Popular Culture: Some Random Observations on the Old and Nouveau Witch." *Sociological Quarterly* 13: 16-36.

Scientists as Experts: Observations on "Objections to Astrology"

Ron Westrum

When the scientist leaves his role as researcher and takes on the role of advocate, he finds the temptation to use his scientific credentials to give the statements the ring of authority difficult to resist. Nor is such use necessarily illegitimate if his credentials bear directly on the matter at hand. The question of relevant credentials must be raised, however, each time a scientist publicly advocates some point of view or course of action. Otherwise, the appearance of impartiality and, above all, of expertise that scientific opinions convey may tempt some of us to accept these opinions as the final word. This danger is heightened when the opinion is a collective one, with the common (and seemingly unanimous) support of dozens of names. So it is with the statement "Objections to Astrology: A Statement by 186 Leading Scientists," originally published in *The Humanist* magazine (Bok et al. 1975) and quickly picked up by the media.[1]

Most of the signers of this statement are astronomers, chemists, or physicists, with a smattering of biologists and scientists with other specialties included. Eighteen have won the Nobel Prize; many others are eminent. The statement argues that there is no scientific foundation for astrology's tenets; that the authors are disturbed by the wide dissemination of horoscopes and astrological charts and forecasts; that the widespread interest in astrology reflects the uncertainty of our times; and that the continued public support for astrology can only contribute to the growth of "irrationalism and obscurantism." They state that there is strong evidence to counter the claims of astrology, although they do not state what this evidence is.

1. An article entitled "Scientists Scan Astrology, Find a Universe of Hokum" (Rensberger 1975) appeared in the *New York Times* on the front page. Other media quickly followed. See Anonymous 1975.

A statement of this kind is interesting from a multitude of perspectives: as an example of the popularization of scientific findings, or at least of scientific attitudes; as the latest in a long history of scientific approaches to astrology (White 1923-1958); as the latest move in the "war against superstition"; and as yet another example of the pretentiousness of scientific orthodoxy (Sagan 1976). What I would like to do, however, is to look at the statement in the light of its implications for public debate on controversial scientific questions. Is this the form in which scientific opinions ought to be expressed? Are the authors of the statement indeed the experts in the case? How ought scientists to present controversial issues, and their opinions on them, to the public?

Some historical precedents

The publication of "Objections to Astrology" was not the first time that a group of scientists had done battle with a belief dear to the public, but which scientists regarded as superstition. The belief in witchcraft was far more pernicious in its social effects than is astrology; it was also more deeply rooted in European social structure and ideas. Its eventual defeat, which Hugh Trevor-Roper attributes to the rise of mechanistic ideas of nature, did not come until the seventeenth century (Trevor-Roper 1969). The witchcraft craze was ending just as modern science was beginning. It thus became the first in a series of popular superstitions against which scientists had to struggle. But science developed a powerful weapon for dealing with "irrational" beliefs: technical expertise.

The growth of science meant the growth of a body of technical experts to whom difficult questions might be referred. Unlike the Church, which had always attempted to deal with technical questions at the highest level, science promoted a division of labor: expertise became specialized. A question might be referred to the specialist in a given area and not simply to the most powerful authority in science. One of the uses to which the French Crown early put its Academy of Sciences was that of dealing with difficult technical questions (Hahn 1971). The questions were researched by small committees, which would issue reports that represented the formal opinion of the Academy. The eminence of the Academy's members, the emphasis on mutual criticism, and the formal nature of Academy proceedings were seen as a protection against the issuing of opinions that were casual or without a scientific basis.

One of the more famous controversies in which the Academy partici-

pated was its confrontation with Mesmerism. At the end of the eighteenth century, Mesmerism enjoyed a popularity that is comparable to that of astrology today (Darnton 1970). Friedrich Mesmer's treatments included the use of "magnetic fluid" to put people into trances, effect cures, and improve health. Hands and "magnetized" objects were passed over, or placed on, bodies to secure the effects desired. (Not surprisingly, the system was popular with ladies who were treated by Mesmer's male colleagues.) Nor was the system merely empirical; a well-developed, if erroneous, theory was promulgated to explain Mesmerism's effects, which we today would recognize as those of hypnotism or simple suggestion.

The Academy of Sciences, its sister institution the Royal Society of Medicine, and doctors from the faculty of medicine participated in two investigations of Mesmerism in 1784. The prestige of the investigating committees (which included Benjamin Franklin) could hardly have been greater. Through a series of experiences and controlled experiments, the savants found that Mesmerist methods did not necessarily lead to the claimed effects and that the effects could be produced without Mesmerist methods (Darnton 1970, pp. 62-64). These findings and their implications were hotly disputed by Mesmerists. And well they might be, for although Mesmer's theory had little merit, the phenomenon of hypnotism, which deserved study, went unnoticed by the committees.

Nearly two centuries later the American government found itself embroiled in the controversy over the existence of Unidentified Flying Objects, more commonly known as "flying saucers." Many witnesses reported seeing strange aerial craft performing odd maneuvers in the sky, landing on the ground, disgorging (temporarily) occupants, stopping cars on highways, chasing airplanes, and so forth (Jacobs 1975). After years of prodding, the government finally consented to commission an independent study and contracted with the University of Colorado to perform it. The commission, headed by Edward U. Condon, a well-known physicist, intensively investigated 59 cases of UFO sightings. These case studies included field investigations, photogrammetric analyses, magnetic sensors (used after the fact), and interrogation of the witnesses of the alleged sightings. Extensive mathematical calculations were used to process the evidence in some of the cases. When the commission issued its report in 1968, it stated that it had found little of scientific value in the study of UFOs: "Our general conclusion is that nothing has come from the study of UFOs in the past 21 years that has added to scientific knowledge. Careful consideration of the record as it is available to us leads us to conclude that

further extensive study of UFOs probably cannot be justified in the expectation that science will be advanced thereby" (Condon 1969, p. 1).

The report, even before its publication, was highly controversial and elicited considerable criticism from UFO advocates, some of whom were members of the scientific community and even of the commission itself (Saunders and Harkness 1968; Boffey 1968; Surrock 1974). Doubts were grave enough that the Science and Astronautics Committee of the American House of Representatives called its own experts to testify before it (U.S. House of Representatives Committee on Science and Astronautics 1968). These experts generally disagreed with the findings of Condon and his associates. Nonetheless, a committee appointed by the National Academy of Sciences reviewed the Condon Committee's work and found it "a very creditable effort to apply objectively the relevant techniques of science to the solution of the UFO problem" (Anonymous 1969).

The conflict between experts

The UFO controversy raises a very interesting question: What if the experts disagree? This happens often enough where mundane issues are concerned, but it is especially likely in matters which are on the frontiers of science. The National Academy of Sciences attempted to weigh the testimony of experts on both sides of the UFO controversy, but their report makes it obvious that they did not really grapple with the issue of scientific method.[2] There is one case, however, which shows that conflict between experts can occasionally produce important insights. The debate in the case involved the installation of a new American antiballistic-missile system, called Safeguard. The system would be designed to intercept and destroy incoming ballistic missiles. Its primary purpose was to protect American offensive missiles, our "second-strike capability," in the verbose jargon of military strategy. Such a question raises a host of issues, ranging from the ethics of nuclear war to the intentions of the Soviet Union. But the greatest conflict grew out of the fact that the experts could not agree even on the basic data.

The debate took place in many phases, but the one we are interested in took place in 1969. Presentations were made before congressional committees, letters were written to the *New York Times*, a book was pub-

2. According to one writer, such superficial jobs are not unusual for the Academy. See Boffey 1975.

lished by one side in the dispute, and many private statements were circulated.[3] One of the participants, Albert Wohlstetter, felt that the opponents of the system were not being fair in their presentation of the data. He thought many of their statements were misleading in a way that made their argument appear stronger than it was. So he took an unusual step. He asked the Operations Research Society of America (ORSA) to examine the methods he and his opponents used.

Operations research is the study of how operations can be made more efficient. Many of the issues in the Safeguard debate were clearly issues of operations rather than of basic science. It is important to recognize, also, that ORSA was being asked to examine only the *methods* used in the debate; it was not being asked whether Safeguard was a good idea or not. ORSA agreed to study the question and appointed a six-member, ad hoc committee, which included four past presidents of the society.[4]

The committee's findings gave Wohlstetter a resounding vindication and made his opponents look either very careless or very dishonest. The ORSA report stated:

> The evidence strongly supports the disturbing conclusion that when prominent experts outside the Administration supported their opinions on Safeguard deployment with arguments or results of an operations-research nature, these analyses were often inappropriate, misleading, or factually in error. Moreover, in many instances, elementary standards for proper presentation of results to permit verification and meaningful dialogue were not met. Failure to distinguish properly between the roles of the analyst and the advocate contributed to the lack of complete research to clarify the presentation. (Operations Research Society of America 1971, p. 1176)

As one might guess, these findings were received with but little grace by those whose reputations had been damaged. Several members of ORSA threatened to resign in protest. The report was criticized as being unfair, and the committee which had written it was accused of partiality (Operations Research Society of America 1971: Appendices IV-D, IV-H, IV-I; Wade 1971; Panofsky 1972).

---

3. See U.S. Senate Committee on Government Operations 1969. This pamphlet gives a sampling of some of the opinions in the debate along with some of the original documents.

4. See Operations Research Society of America 1971. The entire issue is devoted to an examination of the Safeguard debate. Documents regarding the establishment of the committee are contained in Appendix IV.

Thus, even calling in theoretically impartial experts to rule on the work of other experts did not produce an end to the conflict of expert opinions. It is therefore necessary to ask: How can one who is not an expert judge between experts when their opinions conflict? One interesting set of solutions to this problem has been evolved by our legal system, which often has to deal with the problem of the conflict of expert testimony. Let us examine how expert testimony is handled by the courts.

Expert opinion in court

The sociologist Max Weber considered rational procedure the hallmark of modern jurisprudence (Weber 1967, pp. 301-21), and indeed the laws of evidence attempt to make the decision-making processes of the jury as rational as possible by carefully controlling the evidence it receives.[5] Admittedly, the laws of evidence are based on commonsense assumptions (Bentham 1825); but in the specific area of expert testimony precedent has now built a strong tradition. Expert testimony in court is subject to strict rules, some of them even more strict than the requirements for publication in a scientific journal. There are three basic requirements for such testimony:

> (1) The expert may testify only about areas which depend on skills or knowledge beyond the competence of laymen.
>
> (2) An expert witness must possess special qualifications of background, training, and experience.
>
> (3) The expert may testify only to matters within his area of expertise. In doing so he can be required to use well-established principles to support his opinion. (Adapted from Waltz 1975)

That an expert must be qualified before his testimony can be accepted seems obvious, but what is not always so obvious is that his expertise must be in the *exact* area on which he will testify. An excellent example of this point can be seen in the famous Sacco and Vanzetti case. This international cause cèlébre concerned two young Italian anarchists accused of killing a paymaster and a guard in a 1920 robbery. One of the

5. Unfortunately, controlling the input of information is only one aspect of the jury's rationality; what they do with the information they get is another matter.

critical issues in the case was whether the fatal bullets had come from a gun found in the possession of one of them—Sacco—when he was arrested. This issue involved a delicate question of forensic ballistics, an area of expertise only recently opened up at the time the trial took place in 1921.[6] Their trial nicely demonstrates some of the pitfalls of expert testimony:

Several firearms experts appeared in behalf of both the defense and the prosecution. These experts contradicted each other on the question of whether the bullet had been fired from Sacco's gun and on many subsidiary issues involved in identifying the bullet. One of the "experts" was in fact a charlatan, a "Doctor" Albert Hamilton. The others included men with considerable knowledge about firearms but very little knowledge about the area of forensic ballistics. Thus the question of whether Sacco's gun had fired the fatal bullet was clouded by a conflict of testimonies that betrayed the experts' lack of qualifications.

In 1927 a man arrived on the scene who was capable of putting the confusion to rest. His name was Calvin Goddard. For several years he had been perfecting the techniques of bullet identification. He had joined the founders of the Bureau of Forensic Ballistics in New York—the first institute of its kind in the world. In a short time he was so adept at using the instruments the team had developed that he became the undisputed leader of the bureau. One of these instruments, the comparison microscope, was to play an important role in the Sacco and Vanzetti case.

Goddard offered his services to both the prosecution and defense in the case. The defense refused, but the prosecution accepted. Goddard set to work in an atmosphere of considerable tension, since the other "experts" recognized that he came equipped with tools they did not realize even existed. When Goddard examined the fatal bullet and a test bullet from Sacco's gun in the comparison microscope, one expert withdrew his previous testimony and admitted that Goddard's methods were far in advance of his own. Another expert also admitted his error. Goddard found that the fatal bullet and a shell casing found at the scene of the crime had both come from Sacco's gun. Sacco and Vanzetti died in the electric chair.

---

6. The following account is taken from Thorwald (1965, pp. 435-50). He deals only with the ballistics issue although other issues were equally, if not more, important in the trial.

The basis of expert testimony

Qualifications are, however, only half the story. The method by which the expert arrives at his decision, or what is called the "basis" of his testimony, is equally important. If the expert cannot show an unbroken line of necessary inferences from the evidence in the case to his conclusion, his conclusion can be impeached. Furthermore, the principles he uses in assessing the data have to be generally accepted in his technical specialty, and findings that he cites should be those that are also accepted. He is thus constrained by the state of his art. Going beyond the state of the art in his testimony is not permitted.

What can happen when experts rule on matters not well understood in their technical specialty is illustrated by a case from the history of science. Although the case was not tried in a court, it might well have been. It took place in 1772 and involved a piece of a stone said to have fallen from the sky. Three scientists (or *physiciens*, as they were then called), members of the French Academy of Sciences, were asked to examine the stone fragment and render a verdict on it: Could it really have fallen from the sky?

The eminence of the scientists was attested by their membership in the Academy, and one of them was Antoine Lavoisier, the father of modern chemistry. They were thus highly qualified. In their report on their examination of the "mineral" (Fougeroux et al. 1777) the three noted that stories of such "thunder stones" had always been looked upon by savants with suspicion and concluded that the fragment which they had examined had not fallen from the sky. It was, they suggested, most probably ferrous sandstone which had been struck by lightening. They based their conclusions on a mineralogical analysis which, although it would seem superficial now, was adequate for the period in which they lived. Their case would have done well in court.

Yet, their verdict was wrong. Stones do fall from the sky, and the stone which they had examined was most probably part of a meteorite. This case demonstrates the difficulty with expert testimony on questions on the frontiers of scientific knowledge. The person who is testifying may be the foremost researcher in his field, but the field itself may not be far enough advanced to include matters on which judgment is being made. The phenomenon of meteorites was still too little studied to render the verdict of the three academicians very useful; a mere 31 years later, in 1803, the whole scientific community would take a different view of the

matter. The changed opinion would be based on the chemical analysis of meteorites from a dozen falls and the close investigation of several cases.[7] But this information was unavailable in 1772, and a decision by scientists at that time was unlikely to go beyond the current state of science.

Expertise on astrology

The previous historical examples have been presented to set the stage for a look at "Objections to Astrology." The first point to consider is that of the relevant expertise for entering into such a debate. Ideally, expertise would come naturally from researchers active in the study of astrology through scientific experimentation, clinical observation, and statistical analysis. The qualifications of such a group to act as experts would seem obvious.

Alternatively, since the group of active researchers is rather small, one might accept a kind of derivative expertise. This secondary expertise might be gained through familiarizing oneself with the relevant studies, literature, and so forth. It frequently happens in science that those who have not done research in a particular field themselves are excellent critics of the methods and conclusions of others' research and can suggest refinements in methods or point out false inferences from collected data. Such expertise would be useful in the astrology debate.

However, it is not clear how many of the 186 signers of the protocol have either of these types of expertise. The eminence of many of these men and women does not allow us to dismiss their opinions lightly, even if they have not familiarized themselves with the relevant literature. Yet we can question whether they have the right to state that "there is no scientific foundation for [astrology's] tenets" without having done the necessary homework. This is especially true since much of the data that does exist on the question seems to indicate that the following statement is ill-considered: "It is simply a mistake to imagine that the forces exerted by stars and planets at the moment of birth can in any way shape our futures."[8] Whether these data will ultimately prove valuable or not is a secondary issue. ***The major issue is whether judgment should precede study.*** As Jean-Baptiste Biot (1774-1862) said in a paper on meteorites: "Always, in doubt-

7. There is not a good general account of the meteorite controversy. See Paneth 1949.

8. This statement appears in the second paragraph of "Objections" and is contradicted by data such as those presented by Gauquelin (1976).

ful questions, the ignorant simply believe, the half-savant decides, but the informed person examines: for he does not have the temerity to erect boundaries to the power of nature" (Biot 1803, p. 7).

It is certainly incorrect to assume that because a person is an astronomer he is necessarily an expert on astrology. Neither is a nuclear physicist nor a physiologist necessarily an expert in nuclear medicine, although the latter involves both of their specialties. As the Sacco and Vanzetti case clearly shows, being an expert in a specialty does not permit one to assume expertise in a related field in question. Astrology concerns celestial bodies and human beings, but this does not imply that an expert in either field is necessarily an expert on the correlation of events in the two spheres.[9]

How should expert opinion be presented?

Even if one assumes that all the scientists who signed the protocol possessed the requisite expertise, one might still ask how their opinion ought to have been presented to the public? We might contrast here the operant procedures of the mass media with those of the courts.

Quite often, when a controversial event takes place, the newspapers or wire services request the opinion of an "expert." This opinion is often solicited by interview or over the telephone and is often printed in excerpted form. This rather casual procedure, while perhaps better than no follow-up at all, has several defects. First, the choice of an expert is often dictated more by the demands of time and convenience than by concern for ultimate qualification; the lack of a cross-examination such as that which might take place in a court often encourages this lack of rigor. Second, the opinion is sought on the spur of the moment rather than with the careful deliberation necessary to prepare a court testimony. Finally, the printing of an abbreviated or summarized opinion, often in paraphrased form, is likely to do further violence to reason.

"Objections to Astrology" avoids many of these pitfalls but not all of them. How many of the signers would survive a cross-examination in court on their qualifications to serve as an expert witness on astrology?[10] But of course the general public, at whom the statement was aimed, would not be aware of this point.

9. This point was made to me very forcibly by my colleague Gordon Moss, an interdisciplinary researcher who studies the influence of human relations on illness.

10. For examples of such cross-examinations, see Waltz 1975 and Levine 1968.

Nor is the public encouraged to examine the evidence itself. A take-our-word-for-it stance pervades the entire statement. Even a well-educated person would hardly be placed in a position to make a better judgment by the statement since the evidence on which an independent decision could be made is scarcely presented.[11] The strong conclusions presented in the protocol might be appropriate as the final result of an extensive research program by a scientific commission. But in its present form it encourages the person reading it to accept a kind of proof by authority. Interestingly enough, this is exactly the same request made by newspaper astrologers. One has to recognize the historical reality that the judgments of scientists have often proved ephemeral, which is all the more reason to make clear the evidence and arguments on which their judgments are based.

What ought to be done?

The signers of the protocol might consider the example set by their French colleagues. In 1960 a book was published in France by Louis Pauwels and Jacques Bergier (1960) entitled *Le Matin des Magiciens* ("The Morning of the Magicians"). It dealt with secret societies, mysterious human powers, the Nazis, alchemy, and similar subjects. Its authors advocated what they called "fantastic realism." The public was quite enthusiastic about the book, and its success led to the publication of a well-illustrated periodical, *Planète*, which continued to serve up the same heady brew of mysticism, "fantastic science," anthropology, and speculation. Alarmed by the current of irrationalism and occultism that *Planète* seemed to evoke, the French Union Rationaliste issued a book of its own, *Le Crepuscule des Magiciens* ("The Twilight of the Magicians"), in which the approach, methods, and conclusions of Pauwels and Bergier were attacked.[12]

I would like to second the suggestion made by Carl Sagan that a scientific symposium on astrology, similar to those offered by the American Association for the Advancement of Science on UFOs (1969) and on Immanuel Velikovsky's ideas (1974) might be useful. Proponents of

11. The article by Lawrence E. Jerome (1975) is a step in the right direction, but an adversary process would clearly have given a fairer account, as Gauquelin's article shows.

12. Gilifret 1965. A copy of this book was kindly sent to me by J. Marchand, Secretary to the Union Rationaliste. Unfortunately this book seems to be out of print. The address of the Union Rationaliste is: 16 Rue de l'Ecole Polytechnique, Paris, France.

astrology, particularly those with numerical data, should confront critics of astrology. This procedure would seem closer to the scientific ideal and, if its results were published, would afford educated persons at least a chance to weigh both sides of the argument. The scientific support that this method might appear to lend to astrology must be weighed against the harm done by authoritative statements that encourage one to believe without proof that astrology is bunk. To encourage the public to believe in science without understanding it should never be the aim of scientists.

REFERENCES

Anonymous 1969. "UFO Study: Text of the Academy's Review." *News Report* (of the National Academy of Sciences) 19, no. 2 (February): 6-8.

Anonymous 1975. "Press Comment on 'Objections to Astrology.' "*The Humanist* 35, no. 6 (Nov.-Dec.): 21-23.

Bentham, Jeremy 1825. *A Treatise on Judicial Evidence*. London: J. W. Paget.

Biot, Jean-Baptiste 1803. *Relation d'un Voyage Fait dans le Departement de l'Orne, pour Constater la Realite d'un Meteore Observe a l'Aigle le 26 Floreal An ll.* Paris: Baudouin.

Boffey, Philip 1968. "UFO Project: Trouble on the Ground." *Science* 161 (July 26).

Boffey, Philip 1975. *The Brain Bank of America: An Inquiry into the Politics of Science*, New York: McGraw-Hill.

Bok, Bart J. et. al. 1975. "Objections to Astrology: A Statement by 186 Leading Scientists." *The Humanist* 35, no. 5 (Sept.-Oct.): 4-6.

Condon, Edward U. 1969. "Conclusion and Recommendations" In *Scientific Study of Unidentified Flying Objects*. New York: Bantam Books.

Darnton, Robert 1970. *Mesmerism and the End of the Enlightenment in France*. New York: Schoken Books.

Fougeroux, Auguste-Denis, Louis-Claude Cadet, and Antoine Lavoisier 1777. "Rapport . . . d'une observation communiqué par l'Abbé Bachelay, sur une pierre qu'on pretent entre tombee du ciel pendant un orage." *Journal de Physique, de Chimie et d'Historie Naturelle* 2: 251-55.

Galifret, Yves, ed. 1965. *La Crepuscule des Magiciens: Le Realisme Fantastique Contre de Culture*. 2nd ed. Paris: Editions Rationalistes.

Gauquelin, Michel 1976. "The Influence of the Planets on Human Beings: Fact versus Fiction." *The Humanist* 36, no. 1 (Jan.-Feb.): 29-31.

Hahn, Roger 1971. *Anatomy of a Scientific Institution: The Paris Academy of Sciences 1666-1803.* Berkeley: University of California Press.

Jacobs, David 1975. *The UFO Controversy in America*. Bloomington: Indiana University Press.

Jerome, Lawrence E. 1975. "Astrology: Magic or Science?" *The Humanist* 35, no. 5 (Sept.-Oct.): 10-16.

Levine, Moe 1968. "Cross-Examination of Defendant's Medical Expert on Cosmetic Disfigurement Showing Expert's Lack of Qualification." In *Examination of Medical Experts.* New York: Matthew Bender. Pp. 298-307.

Operations Research Society of America 1971. "Guidelines for the Practice of Operations Research." *Operations Research* 19, no. 5 (September). The entire issue is dedicated to this subject.

Panofsky, Wolfgang 1972. "Letters." *Scientific American* 226, no. 1 (January): 6.

Paneth, F. A. 1949. "Science and Miracle." *Durham Science Journal* 41: 45-54.

Pauwels, Louis, and Jacques Bergier 1960. *Le Matin des Magiciens.* Paris: Gallimard.

Rensberger, Boyce 1975. "Scientists Scan Astrology, Find a Universe of Hokum." *New York Times,* September 3, p. 1.

Sagan, Carl 1976. "Readers Forum." *The Humanist* 36, no. 1 (Jan.-Feb.): 2.

Saunders, David R., and R. Roger Harkins 1968. *UFOs? Yes! Where the Condon Committee Went Wrong.* New York: Signet.

Sturrock, Peter A. "Evaluation of the Condon Report on the Colorado UFO Project." SUIPR-599. Stanford: Institute for Plasma Research.

Thorwald, Jurgen 1965. *The Century of the Detective.* New York: Harcourt, Brace, and World.

Trevor-Roper, Hugh 1969. *The European Witch-Craze of the Sixteenth and Seventeenth Centuries and Other Essays.* New York: Harper & Row.

U.S. Senate Committee on Government Operations 1969. "Planning-Programming-Budgeting: Defense Analysis: Two Examples." Washington, D.C.: Government Printing Office.

U.S. House of Representatives Committee on Science and Astronautics 1968. *Symposium on Unidentified Flying Objects.* Washington, D.C.: Government Printing Office.

Wade, Nicholas 1971. "ABM Debate: Learned Society Split by Old Grievance." *Science* 174 (October 15): 276-277.

Waltz, Jon R. 1975. *Criminal Evidence.* Chicago: Nelson-Hall.

Weber, Max 1967. "The Formal Qualities of Modern Law." In Max Rheinstein, ed., *Max Weber on Law in Economy and Society.* New York: Simon and Schuster.

White, Lynn 1923-1958. *A History of Magic and Experimental Science.* 8 vols. New York: Columbia University Press.

A REPLY TO RON WESTRUM

Are Astronomers and Astrophysicists Qualified to Criticize Astrology?

Paul Kurtz and Lee Nisbet

I

Of course we should reject as illegitimate simple appeals to authority without supporting evidence. But did Professor Westrum study the September/October 1976 issue of *The Humanist* in which "Objections to Astrology" first appeared? Or does he base his objections to the statement primarily on press accounts? For it is puzzling that he virtually ignores the fact (aside from a passing reference to Lawrence Jerome in a footnote) that the statement was accompanied by two lengthy articles of over 12,000 words by Bart J. Bok and Lawrence E. Jerome, each giving evidence and documentation for the case against astrology. Professor Westrum maintains that the scientists "state that there is strong evidence to counter the claims of astrology, although they do not state what this evidence is." Yet, in his supporting article Professor Bok, a lifelong student of astrology and former president of the American Astronomical Society and drafter of the statement, discusses the origins of astrology, the preparation and interpretation of horoscopes, and gives evidence from the scientific view as to why he thinks astrology is mistaken. And in his article Lawrence Jerome, who has done extensive research on astrology, reviews both the historical development and the contemporary literature of astrology; he discusses in turn horoscopes, cosmic clocks, statistical astrology, and humanistic astrology.

The point is that "Objections to Astrology" was published along with the two supporting articles, and most newspapers referred to both articles in reporting on it. Thus the case offered against astrology was not based simply on an appeal to authority but on evidence (or the lack of it) for astrology.

Incidentally, the first news story issued was by Alton Blakeslee of the Associated Press, released nationwide (2 September 1976), and he quoted from both the Bok and Jerome articles. The article in the *New York Times* (September 3) was entitled "186 Top Scientists Dismiss Astrologers as Charlatans" and not "Scientists Scan Astrology, Find a Universe of Hokum." So much for careless scholarship.

II

Nevertheless, Professor Westrum does raise some serious questions that bear analysis. He asks, What are the "relevant credentials" that qualify a scientist (or scholar) for commenting on astrology? Are astronomers and astrophysicists qualified to say anything at all about the claims of astrology? Well over 90 percent of those who endorsed "Objections to Astrology" were astronomers or astrophysicists, the others being physicists, chemists, behaviorists, and statisticians. Surely astrologers are talking about the planets and solar system and their alleged influence on earth, and astronomers and astrophysicists have spent more time and effort in the study of these phenomena than virtually any other scientists. Astrology and astronomy share a common origin of several thousand years and many of the most basic assumptions and conclusions of astrology presuppose or contradict modern astronomical principles (such as the precise positions of the planets in space, the precession of the equinoxes, etc.). Who is better qualified than astronomers and astrophysicists to say something about such astrological claims? It is a libel to say that those who endorsed the statement have never read or studied the claims of astrologers and that they lack the competence to do so. As a matter of fact, any number of them—Bart Bok, George Abell, Richard Berendzen, Owen Gingerich, N. Wyman Storer, and Charles R. Tolbert, to mention only a few of the signers—have written on astrology and debated with astrologers. Indeed, in the public mind astrology is often confused with astronomy; all the more reason why so many astronomers thought it important to disentangle the subjects and to show that there is no evidence from the physical sciences for astrological claims. Astrologers make other claims, but their fundamental assumptions have astronomic import and can be scrutinized by astronomers.

Professor Westrum also questions whether it was appropriate for the scientists who signed the statement to leave the role of researcher and take on the role of advocate. Had he read carefully the subsequent issues of *The*

Humanist since the statement was issued, he might have gained some idea of the reasons for the statement. Bart Bok states, "As far as I am concerned our primary purposes have been achieved. They were: (1) to warn young people against accepting astrological predictions without question; and (2) to provide them with a clearly written statement and two articles showing that astrology totally lacks a scientific foundation" (January/February 1976, p. 28). Bok goes on to say that from 1930 to 1974 he regularly taught beginning courses in astronomy, and that he became quite disturbed by the many questions from his students regarding the astronomical foundations for astrology (or lack of them).

Surely Professor Westrum does not believe that scientists should withdraw from society and not try to influence social policy or public attitudes. Experts will disagree and scientists may be mistaken. The scientific testimony of one day may be overthrown or disconfirmed the next. But this does not mean, therefore, that scientists should withdraw into a sanctuary and that only unsubstantiated claims should rule in the arena of public thought and discussion. It does not mean that astrologers may speak out and that those scientists who disagree with them should not be allowed to criticize them.

III

Apparently Professor Westrum would permit a scientist to take on the role of critic or advocate, but only if he is an "expert witness." An expert witness is one whose expertise is in "the exact area on which he will testify." Sound logic must be employed in his testimony and "furthermore, the principles he uses in assessing the data have to be generally accepted in his technical specialty . . ."

These criteria suggest that astronomers and sociologists, for example, can only be expert witnesses concerning astronomy and sociology. They cannot, according to Professor Westrum's logic be expert witnesses relative to astrology. Their "exact area" is not astrology and they decidedly *do not* use the same principles of data evaluation as technical specialists in astrology (astrologers) use. Is the conclusion to be drawn, using Professor Westrum's criteria, that only astrologers are expert witnesses concerning astrology? If so, Professor Westrum as a sociologist would not himself be competent to judge astrology, since he is not an astrologer, nor could he judge its relationship to astronomy, since he is not an astronomer.

One may ask, Are Scientologists the only experts qualified to comment publicly on Scientology, exorcists on exorcism, and UFOlogists on UFOs? Are those who are skeptical of the claims in these areas not to be permitted to comment? Then Professor Westrum agrees with the astrologers who have indeed maintained that *they* alone are the authorities on astrological questions and that any external criticism is ill founded. But if one were to reason in this manner, then the so-called experts in any field are permitted to so circumscribe it that anyone outside of it and not certified by its credentials—such as by having received a degree from an astrology association—is not qualified to say anything about it.

Interestingly enough, even Professor Westrum repudiates (implicitly) these restrictive criteria. He admits that the number of "active researchers," that is scientific researchers, doing work on astrology is "rather small." He implies but does not say, then, that the number of astrologers doing *scientific* research in astrology is small—almost nonexistent.[1] Since the number of astrologers doing nonscientific work in astrology is large, this implies that few *astrologers* are expert witnesses. There must be, then, more important criteria than being in the "exact area," or subscribing to the principles of the technical specialty, to qualify as an "expert witness."

Professor Westrum develops another category of scientists who perhaps could be qualified to comment on astrology—those with "derivative expertise." "Derivative expertise" is a "secondary expertise," which "might be gained through familiarizing oneself with the relevant studies, literature and so forth." Professor Westrum concludes however that it is doubtful that many of the 186 signers of the astrology statement have even this "secondary" expertise. His evidence? None is given, other than that the signers agreed with this statement: "It is simply a mistake to imagine that the forces exerted by the stars and planets at the moment of birth can in any way shape our futures." He claims Gauquelin's statistical study

1. Robert Hand, an astrologer, in his essay "On Creating a Science of Astrology," found in the *Journal of Geocosmic Research* (Vol. 2, no. 2, pp. 77-78), points out: "It is unfortunate that most astrologers are out of touch with the basic-research aspect of astrology, and of those who are in touch with it, many feel threatened by it. The serious journals of astrology contain a mixture of articles relating to the craft of astrology and a very few relating to astrology truly as a science . . . Astrologers are only just now learning to test their own practices. Most of us have not had the proper training to do so. So far, testing of astrology has been done by a few dedicated amateurs with no funding."

thus far refutes this assertion.[2] Professor Westrum believes that if the signers were familiar with Gauquelin's research they would never have supported the statement. (He again fails to mention that Jerome's article devotes some attention to Gauquelin's work.) Signing the astrology statement proves in Professor Westrum's mind that the 186 scientists lack even derivative expertise and that, therefore, their status as experts of any kind relative to astrology is negligible.

Professor Westrum's reasoning warrants close attention. He admits that no one knows yet what the meaning and scientific value of Gauquelin's data is. What he fails to say, however, is that Gauquelin's research has little to do with astrology. Gauquelin himself explicitly *denies* that his work supports classical astrology. Indeed, he considers himself a leading critic of astrology. Gauquelin does not say that the forces exerted by stars and planets in any way "shape our futures." What he attempts to do is statistically correlate professions and personality characteristics with the positions of certain planets at the hour, day, and place of birth (he offers no data on the moment). It may very well be that Gauquelin's research is warranted. We are not ourselves prejudging that. *The Humanist* magazine has devoted the better part of a year to discussing Gauquelin's work and to setting up conditions to test his hypothesis. There have been in its pages nine articles by or about Gauquelin! Of course, whether his hypothesis is correct can be verified only by the scientific evidence. *The Humanist* is sponsoring research in this regard in cooperation with two astronomers (George Abell and Owen Gingerich) and two statisticians (Elizabeth Scott and Marvin Zelen) in order to test Gauquelin's claims.

Professor Westrum should not assume prior to confirmation that Gauquelin's hypothesis is warranted. Has he himself engaged in testing that would warrant his conclusions? Are his judgments grounded in some scientific expertise that he possesses, or is his expertise "derivative"? If Professor Westrum thinks that he has evidence for the truth of popular or classical astrology, then he should tell us what his evidence is. He certainly does not in his article give us the benefit of his knowledge.

---

2. He says: "We can question whether they [the scientists] have the right to state that 'there is no scientific foundation for [astrology's] tenets, without having done the necessary homework. This is especially true since much of the data that does exist on the question seems to indicate that the following statement is ill considered: 'It is simply a mistake to imagine that the forces exerted by stars and planets at the moment of birth can in any way shape our futures.'" And in a footnote he adds that this statement "is contradicted by data such as those presented by Gauquelin."

IV

In our view, and in the view of many scientists who issued the statement, astrology has little, if any, scientific foundation. Astrology is several thousand years old. It is not a "frontier" science, but is still rooted in Ptolemy's cosmology. It has no clearly worked out or coherent scientific theory. The astronomical assumptions underlying it are questionable. Astrologers differ widely in their views about the nature of astrology, of how to interpret horoscopes, and even of what constitutes an adequate test for prognostications. There have been few efforts by astrologers to find supporting data for their claims. Many explicitly reject the idea that astrology is a science, and talk instead about "occult" forces or correlations influencing human destiny. For these reasons, it seemed to us a useful public service to have 186 scientists present their objections to astrology (i.e., that it simply is not a science); far from being condemned, it should be welcomed.

What is surprising to us about Professor Westrum's paper is that he devotes the lion's share of his attention to criticizing the scientists who issued the statement, but almost none to the growing tide of irrational belief systems, which astrology represents. Nor does he say anything about the spurious claims being made by astrologers who sell their wares to an unsuspecting public with impunity.

Biorhythms and Sports Performance

A. James Fix

Several popular articles (e.g., Wallenstein and Roberts 1973; and MacKenzie 1973) and at least one book (Thommen 1973) have appeared during the past few years describing the "biorhythm" or "biocurve" theory of human performance. This theory alleges that the ebb and flow of physical, intellectual, and emotional energy follows 28-, 33-, and 38-day cycles. The concept has been derived generally from certain psychoanalysts of the nineteenth century, most notably Wilhelm Fleiss, a German physician who at times shared a close relationship with Freud. The ability of the biorhythm theory to predict human performance is apparently "confirmed" by the tendencies of Hank Aaron and Babe Ruth to hit more home runs on "up" days than on "down" days and by reports of accident rates by certain industrial firms in Japan and Sweden. The firms found that accident rates were cut sharply when they warned their employees of impending "critical" or "accident-prone" days, according to employees' individual biocurve charts. Some American firms now sell charts and other equipment to help people keep daily records of their own biorhythms (e.g., Unique Products Co., Hanover, Pennsylvania).

The theoretical problems with the biorhythm concept are large enough that few serious researchers have subjected it to scientific scrutiny. These problems include: (1) the belief that each person has exactly the *same* cycles of 28, 33, and 38 days—it is unknown in science for two genetically distinct organisms to carry any trait or characteristic with essential identity, whether it be liver weight, red-blood count, diurnal cycle, or menstrual flow and cycle; (2) the necessary assumption that the cycles are totally inflexible and invariant, maintaining their patterns regardless of age, sex, illnesses, or life events; (3) the difficulty in stating where the

cycles begin—the assumed starting point is the day of birth, regardless of prematurity or other events surrounding the delivery; (4) the assumption that all the systems in all humans begin an "up" pattern (or increase of energy) from the day of birth.

There is a simple way to put the biorhythm claims to objective test: we can measure the actual productivity of people at various points in their cycles. One group of people whose productivity is publicly available are professional athletes. For the present study it was predicted that major-league baseball hitters would perform better on their "up" days than on days low in their cycles or midway between these extremes.

Method

During the first week of the 1975 major-league baseball season, three players were selected by a random process from the first four players in the batting order of each of the major-league teams. Selections were confined to the first four batsmen since an early position in the batting order would suggest that a major part of that player's responsibility is hitting.

A "biocurve" was made for each of the selected players for the months April through September based on birth dates recorded in *Baseball Digest*. Batting performance was checked on "up" days (when all three curves were within 1.5 days of their peak), "down" days (all three curves were within 1.5 days of their nadir), and "triple zero" (TZ) days (all three curves were within 1.5 days of the "neutral" point, neither high nor low). These latter days have been called "critical," and an increased risk of accidents is thought to exist for them. Since none of the biological systems are either up or down on these days, performance is expected to be sporadic, listless, uneven.

The "up," "down," or "triple zero" days as defined here are rather rare, one or another occurring on an average of only once every four months. Therefore a number of the selected players had none of the specified "days" during the designated months and were dropped from the study. When this occurred, the remaining of the first four hitters was taken as a replacement. Other losses occurred to the final selected group. During the season some players disappeared from the lineups because of injury, replacement, or release from the team. The study continued with the players who remained active. A total of 85 players was watched, with 70 actually coming to bat on one of the designated days.

Results

"Triple-zero days occur about twice as often as "up" and "down" days combined. This means that players would be expected to bat about four times as often on TZ days as on either "up" or "down" days. In this study the TZ batting appearances held an even larger numerical superiority. This would be true for any observation period of less than one year; "up" or "down" days can occur at most once per baseball season per player, while TZ days can occur twice. TZ days also occur with players who experience neither of the other type of days. Thus TZ days were somewhat overrepresented in the sample.

Table 1 contains the batting records of the players on the designated days. The pattern does not follow the hypothesized superiority of "up" days over the others. Two-tailed z-tests for the proportion of hits were nonsignificant in all comparisons. Thus players in general showed no significant tendency to bat more effectively on any certain type of day.

Table 1
Batting Performance of Players on High, Low, and Middle Biocurve Days

	At Bats	Hits	Average
"Up" Days	34	7	.206
"Down" Days	32	8	.250
"Triple Zero" Days	192	53	.276

Two-tailed z-tests: "Up" vs. "Down"—.60; "Up" vs. "TZ"—1.31; "Down" vs. "TZ"—0.45. None approach 0.05 level of significance.

The ideal study is one in which subjects serve as their own controls. This occurred only rarely in the present study, since it is an uncommon occurrence for a person to have more than one designated day in a single six-month period. "Up" and "down" days are always widely separated, so no player had both of these during the season. Table 2 contains the batting performance of those players who could serve as their own controls by batting on an "up" or "down" day and also on a TZ day. It will be noted that once again the two-tailed z-tests failed to reach significance.

Table 2
Batting Performance of Players Serving as Own Controls

	At Bats	Hits	Average
"Up" Days	24	6	.250
"Triple Zero" Days	21	5	.238
		Two-tailed z = 0.13 (NS)	
"Down" Days	15	3	.200
"Triple Zero" Days	20	6	.300
		Two-tailed z = 0.97 (NS)	

Discussion

Wherever theories and belief systems are used to sell products or services to the public, people have a right to know the scientific evidence for the ideas being promoted. In this case there was no evidence that the biocurve theory is helpful in providing personally useful predictions for individual athletic performance. Professional baseball hitters appear to perform no better on "up" days than on "down" or TZ days.

As far as I am able to determine, the present study is the only one available that has applied objective methods to assessing the biocurve hypotheses. Certainly, a better study would be one in which individuals could be used consistently as their own controls, and skills other than physical ability could be assessed. However, until such studies have been completed and have found clearly positive results, it would seem best to restrain all claims of personal benefit from the use of biocurve information. In the present study the participants were probably unaware of their biocurve positions on any given day. It would be interesting to find out whether giving information to people concerning their biocurve standing on certain days could reduce injuries or other problems, or even improve performance, simply on the belief that the information is important to their functioning. The placebo, or expectancy, effect is widely known in medicine and in psychological therapies and might be the basis for some of the claims made by biocurve proponents.

One criticism of these results may be that, while biorhythm effects are difficult to demonstrate with group averages, individual predictions might still be useful. Aside from the argument's being statistically untenable, on a practical basis it is difficult to see how the knowledge that 27 July 1975 was an "up" day for Carlton Hendrick of the Cleveland Indians could console his 0-for-4 effort in hitting that day; nor should it bother Daryl Evans of the Boston Red Sox that August 22 was a "triple zero" day. He doubled and homered in three official attempts that day.

REFERENCES

Mackenzie, Jean 1973. "How Biorhythms Affect Your Life." *Science Digest* 74 (August): 18-22.

Thommen, G. S. 1973. *Is This Your Day?* New York: Crown.

Wallenstein, M. R., and Nancy Lee Roberts 1973. "All Together on the Bio-curve." *Human Behavior* 4 (April): 8-15.

Von Däniken's Chariots: A Primer in the Art of Cooked Science

John T. Omohundro

> "I am not a scientific man, and if I had written a scientific book, it would have been calm and sober and nobody would talk about it."
>
> —Erich von Däniken

> *Playboy:* Are you, as one writer suggested, "the most brilliant satirist in German literature for a century"?
> *Von Däniken:* The answer is yes and no. . . . In some part, I mean what I say seriously. In other ways, I mean to make people laugh.

Were it not for the fact that Erich von Däniken has millions of otherwise intelligent people discussing his book and theories seriously, I would prefer to write a parody of his style. But I fear his readership might believe me too. I ignored his books for four years, but now I cannot teach my students or talk to my academic colleagues without his name souring my day. It is out of his hands, now, this chariot thing. It has reached the people, and for reasons that are their own they have made von Däniken a prophet (profit?) and me a defender of the Establishment.

Why is this book so popular? Von Däniken, it seems, has written one of the scriptures of a new cult. What he says, people obviously want to hear.

Throughout history, cultures subjected to stressful situations have responded with cataclysmic religious reformations, often as a substitute for or supplement to political rebellion. The Zulu Uprising in Africa, the Sepoy Rebellion in India, the New Guinea Cargo Cults, the Ghost Dance of the Plains Indians, the Taiping Rebellion in China, and the Luddites and Anabaptists in Europe are some of the famous examples. Anthropologists call them revitalization movements, messianic cults, and so forth,

and take them quite seriously. Though they vary greatly, they have certain characteristics in common: a humorless fanaticism, prophets, a new world view, and a stiff distaste for the Establishment. Most of these movements are rooted in obvious and serious crises, and frequently are part of a religious and political change in the culture.

The entire von Däniken affair, even much of the UFO interest associated with it, is, I think, very much like these movements. Only hindsight will give a good perspective on this point in American history, but the "we are not alone" attitude has become an important element of our culture's religious cosmology. A frustration with science's not having delivered all that it promised, a distaste for the specialization of scientific research, and a continuing need to believe in an intelligence beyond our own are the main characteristics of this antiscience mysticism. It does not take much imagination to see that science has been for many in our culture the New Religion, with its white-frocked priests talking in strange tongues about a universe we couldn't even understand. (Try to grasp the idea of a boundless universe doubling back on itself, a la Einstein.) The priests' accomplishments in a few areas like technology and medicine were enough to satisfy the faithful. But as a religion science didn't stand the test of time. The contrast between what we could do in space with what we could do for ourselves on earth was like watching a priest celebrate mass with his zipper down. Science is rather stale as a religion, and it cannot substitute for one. The man-in-the-street prefers a richer religion than that.

If von Däniken's thesis is part of your religious cosmology, so be it. I don't argue religion; I try to study it and see how it relates to human life. But if von Däniken seems like science to you, shame on you.

What follows is an attempt to lay open von Däniken's approach as a warped parody of reasoning, argumentation, as well as a vigorous exercise in selective quotation, misrepresentation, and error based on ignorance (presumably, if it is not intentional fibbing). For students his work does serve two valuable purposes: first, it raises their interest in the cultures and myths which he so badly mishandles; and second, like Lewis Carroll's *Alice in Wonderland, Chariots of the Gods?* is a challenge to study and determine all that is wrong with it. So it is by no means a complete waste of one's time (either his, yours, or mine).

Briefly stated, *Chariots of the Gods?* proposes that scientists have overlooked or refused to inform the world of the many pieces of evidence which suggest that we have been visited, probably several times, by intelligences from other planets. Von Däniken argues that an open-minded

approach to the ruins of past cultures and their art and myths raises many unanswered questions which can best be answered by accepting the hypothesis of extraterrestrial visitors. Data from Incan, Mayan, Sumerian, Egyptian, and many other cultures which suggest the hypothesis include cave painting, architectural and technological accomplishments, and mythological events of great similarity around the world. Von Däniken says that the explanations given by scientists of these data are too smug, and that now that space travel is possible for us, we must at least admit that his hypothesis is as viable as anyone else's.

Some of my professors used to tell me that hypotheses are a dime a dozen; people make them up all the time. Making an hypothesis is not science; it's what you *do* with an hypothesis that more or less is science and is to be judged by others. Von Däniken is entitled to his hypothesis. But what does he *do* with it?

The straw horse, red herring, and other ruses

Argumentation is an art which can easily be perverted. One technique to make yourself sound good is the straw horse: misrepresent the thing you wish to argue against. Von Däniken's characterizations of archaeology and anthropology—fields which focus on precisely the kind of data he studies—are abysmal.

> . . . in the future, archaeology can no longer be simply a matter of excavation. The mere collection and classification of finds is no longer adequate. Other branches of science will have to be consulted and made use of if a reliable picture of our past is to be drawn. (p. 14. This and future references from von Däniken are to *Chariots of the Gods?*)

> Let us say that someone decides to become an anthropologist and he reads and learns a lot about anthropology, about bones and apes and all those details. (Ferris, p. 58)

By denying the breadth of these fields and the wealth of data in them he has left somewhat of a vacuum into which to float his own ideas, which I hope to show are clearly not based on any background in archeology or anthropology. These are not the only disciplines he chops. His critics are nearly unanimous in accusing him of misrepresenting or failing to understand even the rudiments of geology, mythology, psychology, chemistry, astronomy, and physics (Ostriker 1973, p. 239).

His technique is successful in part because there are many presumably educated people who don't understand these fields, or even the ways of scientists in general. He has played to the prejudices and stereotypes of those who are not "scientists" (priests of the old religion). The tone of "you and I, dear reader" places him and his readership in an underdog position against the monolithic Establishment of picky pedants who represent the scholars.

Another technique that works well for misleading the mind is the red herring. The object is to confuse the reader by introducing an extraneous issue so that he will not catch you on your main point. Politicians might introduce Motherhood and Apple Pie, but von Däniken has his reflections on truth, atomic war, and propagandizing for space research. His comments in these red herrings seem startlingly in contrast to his arguments.

> We owe it to our self-respect to be rational and objective . . . (p. 5)
>
> We may be as religious as our fathers, but we are certainly less credulous. (p. 37)
>
> Anyone who really seeks the truth cannot and ought not to seek it under the aegis and within the confines of his own religion. (p. 53)
>
> It is unworthy of a scientific investigator to deny something when it upsets his working hypothesis and accept it when it supports his theory. (p. 66)
>
> It is depressing what many people—and sometimes whole occult societies—make out of their ostensible observations. They only blur our view of reality and deter serious scholars from dealing with verified phenomena . . . (p. 120)

These comments are quite irrelevant to his arguments and serve only to glaze the reader's critical judgment.

One final technique that is useful in argumentation is to warn the reader in advance about the criticisms which will be leveled by one's opponents. This is not the same thing as dealing with those criticisms, but neatly puts the critic on the defense when listeners say, "Aha! Von Däniken said you would say that!" thus somehow scoring a point for the home team. For example: "Impossible? Ridiculous? It is mostly those people who feel that they are absolutely bound by laws of nature who make the most stupid objections" (p. 84).

Our ancestors, the dummies

What most depresses my fellow anthropologists and me is the way people accept von Däniken's unnecessarily anthropocentric and ethnocentric views of other people in the world and in history. Anthropocentricism is the assumption that other living, sentient, or intelligent creatures must feel and think or evolve as humans do. Ethnocentricism is the even more narrow assumption that other people must think, behave, or evolve as we do. Further, there is usually a heavy flavor of cultural superiority in such assumptions.

Chariots of the Gods? plays upon most people's inability to break out of these assumptions. It implies that up until the last thousand years or so the world was filled with primitives, heathens, savages, dummies. Their intelligence matched their simple technologies; their languages were simple, their cultures were primitive, they were brutes. If they seem to have come up with something quite fantastic by our standards, someone smarter than them must have given it to them. They then proceeded to garble it up in their ingenuousness; they certainly didn't do those things for the same reason that we would have.

> (Caucasoid-like figurines of Summer): . . . very difficult to fit into the schematic system of thought and its concept of primitive peoples. (p. 27) [*The reasoning: primitives can't be Caucasians.*]
>
> Since we are not prepared to admit or accept that there was a higher culture or an equally perfect technology before our own, all that is left is the hypothesis of a visit from space! (p. 28) [*A sillygysm! See the next section.*]
>
> (The Yahweh of the Semites): It is . . . difficult for enlightened children of this age to think of an infinitively good Father who gives preference to "favorite children" such as Lot's family. (p. 37) [*The reasoning: You modern Christians aren't going to believe all this Biblical stuff about a harsh God.*]
>
> . . . descriptions of extraordinary things that could not have been made up by any intelligence living at the time the tablets were written, any more than they could have been devised by the translators and copyists who manhandled the epic over the centuries. (p. 49) [*The reasoning: Only moderns have enough intelligence to be imaginative.*]
>
> Since the question of space travel did not arise 100 years ago, our fathers and godfathers could not reasonably have had thoughts about whether our ancestors had visits from the universe. (p. 4) [*Just plain wrong.*]

The Mayans were intelligent; they had a highly developed culture. . . . it is difficult to believe that it originated from a jungle people. (p. 55) [*The reasoning: Jungle people are somehow dumber than most. Just look at Africa.*]

(The Egyptian, Chinese, and Incan civilizations): Who put the idea of rebirth into the heads of these heathen peoples? (p. 63) [*The reasoning: Heathen heads are empty.*]

(Egyptians): How did such a highly developed civilization arise at such an early date? . . . Who gave them their incredible knowledge of math and a readymade writing? (p. 65) [*Anyone that was civilized before us cheated. What ever happened to the Honor Code?*]

(Ancient storytellers had strong imaginations): So it must be that the ancient storytellers had a store of things already seen, known, and experienced at hand to spark their [*otherwise dull?*] imaginations. (p. 65)

How on earth could people in the dim past arrive at different perceptions of one and the same thing, when the horizon was very limited? (p. 66)

(Egyptian mummification): Who put the idea of corporeal rebirth into the heads of the heathen? (p. 81)

If the stone age cavemen were primitive and savage, they could not have produced the astounding paintings on the cave walls. (p. 87)

(The Mexican flying serpent Quetzalcoatl): How could anyone worship this repulsive creature as a god, and why could it fly as well? (p. 104) [*Answer: government subsidy.*]

These are really just a handful of examples which reveal ethnocentricism. Von Däniken's reasoning, conservatively stated, is: there are some real mysteries in the past because it is obvious that people who lived then are not solely responsible for those remarkable things. There are indeed real mysteries in the past, but they are usually not the ones von Däniken sees. When one consciously puts aside the prejudices of his own culture and examines the cultures of the peoples mentioned in *Chariots of the Gods?* one begins to see the way myth, art, architecture, politics, kinship, and technology relate to one another, reflect and react to one another. The "fit" of many of these seemingly bizarre practices in the rest of their culture is often in itself a wonder to behold.

The sillygism and cooked science

Von Däniken's book is a virtual goldmine of logical fallacies, implications by innuendo and rhetorical questions, and failures to apply "Occum's Razor." Alicia Ostriker, who interviewed von Däniken for *Esquire*, wrote, "So what if the fallacies fly by in flocks like mallards heading south?" She was captivated by the man's enthusiasm and chose to overlook his "gee-whiz style fit only for kiddies." She chose to overlook his flaws—but many other people don't see them.

A non sequitur, or logical fallacy, makes a conclusion which does not follow from the premise. The book starts out with a few non sequiturs. On page vii von Däniken argues that if you ignore his book, then you are a layman who refuses to face the adventurous and mysterious past. On page 2 he says that if one accepts the possibility of developed life elsewhere in the universe, then it must have been a civilization. Here is an example, phrasing the main thesis of the book: "Since we are not prepared to admit or accept that there was a higher culture or an equally perfect technology before our own, all that is left is the hypothesis of a visit from space!" (p. 28).

A rhetorical question places the entire burden of proof on the reader, who either acquiesces because of the generally bewildering style of the argument or passes the burden of proof on to the "scholars." When contemplating the ruins of Tiahaunaco, in Bolivia, von Däniken writes: "Had our forefathers nothing better to do than spend years—without tools—fashioning water conduits of such precision?" (p. 21)

Applying Occum's Razor means that when two explanations for one set of facts are possible, one adopts the simplest explanation, that is, the one that assumes the least number of "ifs." Von Däniken has argued (Ferris 1974) that space travel is a simple explanation, since it is now possible by us. However, it is not the possibility of space travel or of extra-terrestrial intelligence that is questionable. The thesis of *Chariots of the Gods?* fails by Occum's Razor because it constructs a gigantic house of cards, each card requiring a new "if." The "ifs" are held together by faith alone and patently contradict most of the principles which "science" had begun to see as a rather unified system. Look, for example, at von Däniken's thesis that modern humans are the act of deliberate breeding by extraterrestrial intelligences. The fossil record of humanlike creatures and the culture they possessed stretches back more than a million years.

Through the millennia, by rather gradual steps, we see the body approaching modern shape and the brain approaching modern size. Cultural developments like fire, sophisticated stone tools, burials, tailored clothing, and so forth appear long before modern Homo sapiens. To see ourselves as a continual development of those trends, moving and adapting to the changing climates, creatures, and contours of the land, is much tidier than introducing some undefined, undated appearance of superior "breeders."

Von Däniken plays heavily on the reader's readiness to conclude that a long string of random possibilities equals a certainty. By the same reasoning, it is a virtual certainty that you will get six heads in six coin tosses, since there is a real possibility (50 percent, to be exact) that one toss will come up heads.

Last, and perhaps most disturbing, is von Däniken's misrepresentation of the very process of "doing science." He does not exhibit, nor does he anticipate in the reader, any real facility in the nature of a "fact," an hypothesis, developing a theory, and proof (or more accurately, demonstration). At one point von Däniken disclaims that he is compiling a sequence of proofs of prehistoric space travelers: "that is not what I am doing. I am simply referring to passages in very ancient texts that have no place in the working hypothesis in use up to the present" (p. 66).

The author doesn't know what a working hypothesis is, nor is he embarrassed to stamp "Q.E.D." on an enormous gaggle of tautologies (assume something, create an hypothesis, test, claim to have proved your assumption). He avoids ever stating anyone else's explanation in reasonable terms. He is loose with his concept of proof, with which he bludgeons unidentified others for not producing. More than any other characteristic, it is this blithely ignorant toying with the method of scientific reasoning which marks the book's shabbiness.

Just plain wrong

A review of *Chariots of the Gods?* in *Book World* says, "To check his 'facts' would take months of research, since he never cites his authorities." His highly selective choice of what to introduce as data follows absolutely no discernable criteria. His translations make critics howl (with glee if they have a sense of humor, with rage if they do not). Many of the "facts" which von Däniken presents have been checked out. A few of these are presented below.

The Piri Reis maps (p. 14). (Amazing maps, but far from accurate.)

The Tiahuanaco culture of Bolivia (p. 20ff). (Cf. Lanning to remove a few of the mysteries von Däniken sees here.)

The Sumerians (p. 24). (Braidwood and Adams among others have quite fine ideas about where the Sumerians came from.)

"Isn't there something rather absurd about worshipping a 'god' whom one also slaughters and eats?" (p. 33). (No. The world has a number of people who do so: Australian Aboriginies, Mesopotamians, Ainu, and others.)

The copper furnaces at Ezion Geber (p. 44). (The dating given is wrong; also the source has withdrawn his speculation: the rooms are storage rooms.)

The breeding experiments of space travelers on prehumans (p. 52). (The *Esquire* interviewer points out that von Däniken doesn't even believe this stuff himself. Then why say it? It defies all the principles of genetics and evolution. Ironically, he calmed down in *Gods from Outer Space*, and the book didn't sell.)

The "suddenness of Egypt" according to Egyptologists (p. 74). (This leads you to suspect someone put it there—bingo. In fact it developed out of a Neolithic farmer culture a thousand years after the civilizations began in Mesopotamia.)

The Cheops pyramid: the height formula, *pi* formula, and wood hypothesis. (Cf. Wilson. Even von Daniken's math is bad.)

"Did the Egyptians learn the possibility of mummification from nature? If that were the case, there ought to have been a cult of butterflies or cockchafers . . . there is nothing of the kind" (p. 84). (Worship of the scarab beetle was widespread.)

The Chinese tomb with 41 dead without violence (p. 86). (How about disease or starvation, a common threat in Chinese civilization?)

Terra cotta heads in Jericho ten thousand years ago: "That, too, is astonishing, for ostensibly this people did not know techniques of pottery making" (p. 87). (Wrong on several counts, one being that terra cotta had been made into statues for over ten thousand years before this.)

"I would suggest, on tolerably good grounds, placing the incident I am concerned with in the Early Paleolithic Age—between 10,000 and 40,000 (p.88). (The early paleolithic ended about 200,000 years before this. What he is describing is called the Upper Paleolithic and Mesolithic.)

The "Chinese" jade necklace in Guatemala (p. 93). (This one threw me for a minute, but Wilson says jade is indigenous to Central America.)

" . . . in Christ's day the concept of a heaven with fixed stars taking into account the rotation of the earth did not exist" (p. 105). (Let me quote Ostriker again: "What the average reader of von Däniken probably doesn't know is that the idea of life on other worlds is not exactly a new one." She further points out that before Ptolemy's geocentric world view came along, a number of cultures were not far off the view we hold now.)

Outline drawings of animals which simply did not exist in South America ten thousand years ago, namely camels and lions (p. 106). (Perhaps they are llamas and pumas, native to the area.)

"There are artificially produced markings, as yet unexplained, on extremely inaccessible rock faces in Australia, Peru, and Upper Italy" (p. 106). (Speaking just for Australia, the aborigines have been seen to make the same markings in their totemic rituals).

Engravings of cylindrical rocketlike machines in Kunming, China (p. 107). (They are! The Chinese invented gunpowder and shot rockets.)

These are some of the items I caught. Others are pointed out in the articles mentioned in the bibliography. Rustless iron columns in India, the Easter Island stones, and so forth are not quite the mystery von Däniken claims.

This review has been aimed at those readers of von Däniken who feel that in the interests of science and reasonableness we should consider his argument. I have sketched some of the reasons why, when one considers his argument, one discovers no science or reasonableness in it. The mass popularity of *Chariots of the Gods?* doesn't derive ultimately from any interest in science or reasonableness but, as I have suggested, stems from a reaction against it. There is some justification for such a reaction; I even advocate a dose of insanity in everyone's life. Von Däniken's book is a good read if you need a dose of enthusiastic delirium. But I do not mix my insanity and my science.

REFERENCES

Ferris, Timothy 1974 "Playboy Interview: Erich von Däniken." *Playboy.* August. Pp. 51 ff.

Ostriker, Alicia 1973. "What If We're *Still* Scared, Bored and Broke?" *Esquire,* December. Pp. 238 ff.

BIBLIOGRAPHY

If your interest has been stimulated by the controversy surrounding *Chariots of the Gods?,* I recommend the following works as just as interesting but more sound.

Braidwood, Robert. *Prehistoric Men.* 7th ed. Glenview, Ill.: Scott, Foresman, 1967. Through the stone age and into the civilizations of Mesopotamia.

Deetz, James. *An Invitation to Archaeology.* Garden City, N.Y.: Doubleday, Natural History Press, 1967. Scientific archaeology.

Eiseley, Loren. *The Immense Journey.* New York: Random House, 1957. Human evolution.

Frankfort, Henri, et al. *Before Philosophy.* Baltimore: Penguin, 1966. An excellent exposition on an old myth.

Lanning, Edward. *Peru Before the Incas.* Englewood Cliffs, N.J.: Prentice-Hall, 1967. One of the few general books.

Sanders, William T., and Barbara J. Price. *Mesoamerica: The Evolution of a Civilization.* New York, Random House, 1968.

Shklovskii, I. S., and Carl Sagan. *Intelligent Life in the Universe.* New York: Dell, 1966. Exobiology by experts.

Wallace, Anthony. *Religion: An Anthropological View.* New York: Random House, 1966. A way of looking at non-Western religions.

Wilson, Clifford. *Crash Go the Chariots.* New York: Lancer Books, 1972.

Book Reviews

REVIEW SYMPOSIUM

The UFO Controversy in America. By David M. Jacobs. Indiana University Press, Bloomington, Ind., 1976. 362 pp. $12.50.

DANIEL COHEN:

David Michael Jacobs, an assistant professor of history at Temple University, has written what should have been an important book. He has reviewed the entire UFO controversy from 1947 (with a brief look at pre-1947 sightings) to 1974. This is a heroic task, for there is an enormous amount of material on the subject, most of it perfectly awful. While Prof. Jacobs has not read everything, he has clearly read more than most ordinary mortals could bear. He has attempted to do a decent job with an unwieldy subject.

And yet, to one who lived through, and in a small way participated in, the UFO controversy, Jacobs' book is disappointing. The whole fuss over UFOs was a lot nuttier than he makes it sound.

There is space for only a couple of examples of what I mean. Jacobs makes only one offhand mention of science-fiction and occult editor and publisher Ray Palmer. Yet noted astronomer Donald Menzel gave Palmer major credit for starting the whole UFO controversy. Palmer sure got me into UFOs. I was a SF reading teen-ager in Chicago when UFOs, or flying saucers as they were called then, first came to public attention. Along with my fellow SF addicts, I got hooked on flying saucers through Palmer-edited publications. I lost my faith in flying saucers when I saw Palmer get smashed in a debate on the subject with Willy Ley at a SF fan convention.

Palmer printed the personal account of Kenneth Arnold, the man who saw the first flying saucers in 1947. He hired Arnold to investigate another celebrated sighting. He beat the drums for flying saucers in his popular science-fiction magazines. He was cofounder of *Fate,* today the largest circulation occult-type magazine in the United States, and one which has consistently printed UFO stories for over a quarter century. After he left science fiction and *Fate* Palmer edited what was then the largest-circulation magazine devoted exclusively to flying saucers. He was the first to propose the idea of an Air Force conspiracy of silence. He was the first to suggest that the flying saucers might have bases on earth and that they might be hostile. He was the first to propose the idea that flying saucers might have come from inside the earth. Perhaps Menzel and I exaggerate Palmer's importance in the growth of the controversy, but surely he deserves more than the brief mention he receives from Jacobs.

Jacobs also slights Frank Edwards. Yet Edwards' books had a far wider readership than any book written by Donald Keyhoe, who dominates *The UFO Controversy*. Edwards, who also did hundreds of radio and television interviews, was an accomplished performer and by far the most eloquent and persuasive spokesman of the pro-UFO camp. Keyhoe, on the other hand, always sounded like he was raving, even when he wasn't.

Both Palmer and Edwards were wild men. They floated nutty theories like the one about the UFOs originating in a hollow earth. Their interest in accuracy was marginal at best, and perhaps they didn't believe most of what they wrote. While they didn't contribute much of significance to the problem of whether UFOs are "real," they were extremely influential in the growth of the controversy. They helped to keep up massive public interest, and without that interest the controversy would have dried up and blown away long ago. And it is the controversy about UFOs, and not their reality, that Jacobs' book is supposed to be about.

Clearly Jacobs believes, along with J. Allen Hynek (who wrote the introduction to this book) and others, that there is "something"—probably something extraterrestrial—to UFOs. But he never quite comes out and says this, which is a shame. Even historians have a right to an opinion, and readers have a right to know what it is. Like many other pro-UFO writers before him, Jacobs tries to make the whole subject sound very respectable. People like Palmer and Edwards are ignored, and the "contactees" are treated as some unimportant aberration. Writers like Jacobs try to set themselves up as the sane but open-minded middle between the crazy contactees and the pigheaded skeptics. That is an acceptable partisan position, but poor history.

This book has its heroes and its villains. The number-one hero is the late James MacDonald, a pro-UFO scientist. We are told how MacDonald suffered scorn and ridicule and paid his own way to investigate sightings. Fine, but critics took a lot of flack too. I used to have a special box in which I stored abusive mail from UFO buffs. Most of it came not from the contactees but from the "respectable" buffs. I'm not complaining. I enjoyed the fight, and I'm sorry it's over. But there were plenty of others who were unwilling to come out and say what they thought—that the whole UFO business was stupid—because they didn't want to put up with the abuse.

Opponents also took money out of their own pockets to investigate sightings. There was certainly no money to be made in knocking UFOs—quite the reverse.

One of Jacobs' villains is the late Edward U. Condon. Jacobs writes, "In addition, he [Condon] made a special trip to New York City in June 1967 to appear at a meeting of the contactee-oriented Congress of Scientific Ufologists where Howard Menger [a celebrated and particularly silly contactee] was the guest speaker. Condon took a bow in the audience."

True enough, Condon was there. Menger was there too, but he was not *the* speaker. It was a three-day conference, not a meeting, and there were many speakers. It was a pretty looney gathering, but it was also the largest gathering of its type up to that time, and anyone who was supposed to be studying the UFO phenomena, as Condon was, should have been there. To say "he took a bow in the audience" is to leave the wrong impression. Condon was introduced (as were many others). He stood up and acknowledged the introduction. What was he to do—

wear false whiskers, or dive under the chair when his name was mentioned?

Jacobs got most of his information from pro-UFO people. According to his own listing of interviews, he talked almost exclusively to them. The result is an unbalanced book.

Then there is Jacobs' conclusion that a new era in UFO studies has dawned. "Free from the debates of previous years, researchers for the first time focused on identification and confronted head-on the mystery of unidentified flying objects." On what does he base this conclusion—the fact that Hynek and a few associates are still collecting reports, and the British journal *Flying Saucer Review* is still being published? UFO buffs have been coming to this sort of conclusion for 20 years. Parapsychologists, faith healers, and even astrologers have been saying the same thing about their pursuits. Such conclusions are pure boiler plate.

K. P. JULIAN:

Some day psychologists will have a field day in their studies of why so many millions of people around the globe, including a handful of scientists with impressive academic degrees, could so intensely believe that large numbers of extraterrestrial spaceships were visiting earth, despite the lack of a single authentic artifact to substantiate that belief.

When that time comes *The UFO Controversy in America*, by David M. Jacobs, should prove a useful reference. This book is the first to detail the origins of the UFO issue, and the U.S. Air Force's bungling efforts to resolve it, through the first several decades of its existence.

But Jacobs' book provides deeper insights than the author intended or realizes. It reveals that even a trained historian can develop so intense a desire to believe in alien visitations that he becomes an advocate rather than a neutral observer.

For example, in the "Foreword" the book is characterized as a "most admirable work," and its treatment of the subject is described as "fairly presented." But this introduction was written by J. Allen Hynek, currently the spiritual leader of the "UFO-believer" movement. It is as if a book purporting to objectively chronicle the facts behind the Watergate scandal had a glowing introduction written by Richard M. Nixon.

Jacobs has done a more thorough job of digging into USAF archival records than any previous researcher. Hence he is able to offer useful insights into the sometimes ambivalent feelings among USAF officers in the early days about whether UFO reports might indeed involve extraterrestrial visitations. This is the most useful portion of the book.

But the author fails to recognize the most crucial issue in the UFO mystery: that there can be a vast gap between the account of a person submitting a UFO report and what actually occurred. For example, consider Jacobs' account of the famous 1964 Socorro case, in which a lone policeman claimed that he saw a UFO

by any other eyewitnesses. Jacobs writes that the policeman "saw a shiny, aluminum-like object. . . . He noticed two people in white coveralls standing next to the object."

All that Jacobs, or anyone, knows with certainty is that the policeman *claimed* that these events had occurred, so the author ought to have written that the policeman "*reported* that he saw . . . " (This case has been characterized as a hoax, intended to provide a much-needed tourist attraction for an economically depressed town, by Philip J. Klass in his recent book *UFOs Explained.*)

Jacobs need not have been so naïve and trusting had he tried to fully inform himself by interviews with current experts on *both sides* of the controversy. But it is clear from his extensive footnotes, as well as from the book's contents, that he chose to concentrate on current experts who are "believers." For example, while Jacobs interviewed Hynek on at least three separate occasions, he did not talk with Donald Menzel, world-famous astronomer and UFO debunker. Nor did he contact Klass, whom Jacobs' book characterizes as "the new leader of the anti-UFO forces" (p. 221).

Despite these important shortcomings, the book is one of a tiny handful (out of more than a hundred on the subject) that deserves reading by serious researchers in the field.

Such researchers will learn of the bitter internecine battles between the leaders of the different UFO groups, who depend for financial support on annual membership dues. While these leaders proclaim that they are only interested in "solving the UFO mystery," perceptive readers may conclude that their motivations are less noble.

In reality, the last thing in the world that the movement's leaders want is a solution to the UFO mystery—even if it fitted their extraterrestrial hypothesis. If an authentic extraterrestrial spaceship were to land in Washington, or Ypsilanti, tomorrow and its occupants were to seek formal contact, the UFO movement would be deprived of its cause célèbre and its raison d'être. The movement's leaders would return to an earlier humdrum existence, without the celebrity status they now enjoy with the news media.

DAVID STUPPLE:

In this informative and well-written book David Jacobs has provided us with the first full-length history of the UFO phenomenon. By exposing attempts by the Air Force and the CIA to cover up some amazing events, Jacobs encourages the reader to believe that UFOs are genuine anomalies that demand the careful attention of both physical and behavioral scientists.

The civilian UFO research organizations (with which Jacobs is personally allied) are the heroes of this drama. Jacobs calls them the "serious" researchers. These no-nonsense, down-to-earth types battle the Air Force and the CIA on the right and the contactees (the "lunatic fringe") on the left of UFO-dom. Most of *The UFO Controversy in America* is devoted to the war with the Air Force, and there, in my estimation, Jacobs succeeds splendidly. His discussion of the contac-

While acknowledging seemingly authentic cases where bewildered chance observers sight a flying-saucer occupant and experience physical and mental effects, Jacobs is more concerned with contactees who claim to be on friendly terms with the space brothers. He suggests that these people (and there are thousands of them) are either mentally ill or frauds. To make his case Jacobs details the careers of five major contactees who became prominent in the 1950s: George Adamski, Orfeo Angelucci, Daniel Fry, Truman Bethurum, and Howard Menger. Thus Jacobs, who defends the civilian researchers against debunkers, turns debunker himself. Curiously he avoids a third explanation—one that he accepts for the *followers* of the contactees—that the contactees simply share a contemporary mythic frame of reference for interpreting their experience. The contactees, we are to understand, are frauds, while their followers are merely naive.

My own research indicates that while some contactees are frauds, others have an experience that they believe to be real. Not all contactees go public, but those who do face both ridicule and encouragement. Often they elaborate their original stories by borrowing from the themes available in the flying-saucer subculture. They may, for instance, ratify their importance by alleging a visit from the Men In Black. Whether frauds or not, the pressure of maintaining an exotic belief system seems to lead some contactees to believe their own stories.

Jacobs understates the interest that "serious" researchers have shown in the contactees. Many researchers feel that aliens are purposely misleading the contactees in order to make the "serious" researchers look ridiculous. And significantly, these same researchers have adopted the fourth-dimension explanation of the origin of the aliens. This "new" theory, of course, has long been part of occult lore and is a standard explanation used by the contactees themselves.

But Jacobs shouldn't be criticized harshly for not adequately discussing the flying-saucer movement when his topic is the UFO phenomenon. *The UFO Controversy in America* deserves the critical acclaim and commercial success it has received. It will soon be out in paperback. Buy a copy.

The Geller Papers: Scientific Observations on the Paranormal Powers of Uri Geller. Edited by Charles Panati. Houghton Mifflin, Boston, 1976. 327 pp. $10.00.

Reviewed by Ray Hyman

Charles Panati has assembled 23 contributions by several scientists, four magicians, and a photographer which constitute, presumably, the best possible case for the reality of Uri Geller's supernatural powers. In addition to making these papers available, Panati has attempted to integrate and interpret their import by means of a preface, a long introduction, and an epilogue. In his preface he anticipates the various responses. "Geller-advocates who read this collection of firsthand observations may feel confirmed in their present opinion of him. His critics will dissect these papers and will find large loopholes and countless faults with the experiments and descriptions they contain, for all of the evidence presented here is cer-

and impressive information—from responsible scientists and professional magicians alike. Their observations taken as a whole are hard to dismiss on the grounds of simple fraud or mass delusion."

Panati asks that "the thoughtful reader . . . give these reports a careful review before drawing final conclusions on the phenomena associated with Uri Geller." Let's assume that our "thoughtful reader" is imbued with the rationalistic and naturalistic viewpoints that take it for granted that all phenomena, both animate and inanimate, obey the fundamental laws of natural science as we currently understand them. Certainly his first perusal of this book is bound to be unsettling. He will discover that scientists, many of them with impressive credentials, report that they have observed Uri Geller "deform solid steel rods without touching them, cause part of an exotic crystal to vanish from within a sealed container, alter the memory of a rare metal alloy, erase information from computer tapes, set Geiger counters ticking with only his thoughts, and read the thoughts of others while he is sealed in a room that blocks out all types of radio waves."

But perhaps scientists are no match for a clever illusionist? Our thoughtful reader might protest that it takes a deceiver to catch a deceiver. But even this loophole is seemingly blocked. Artur Zorka, a professional magician, placed his own fork into Uri's outstretched hand. "His fingers curled around it, and in moments, without the fork's leaving my sight for even an instant, it literally exploded, sending fragments of the handle across the room." Danish conjuror Leo Leslie took a nickel-plated, enameled key that Uri had just lightly stroked. Leslie writes that "while I sat looking at the key the enamel suddenly started to crack, and a second later strips of the nickel plating curled up like small banana peels, while the key actually started to bend in my hand." Both Zorka and Leslie claim that they have no explanation for how Uri accomplished these feats.

The thoughtful reader, it is easy to imagine, may find these observations disturbing, to say the least. But Panati has urged a "careful review," and any argument for a proposition—especially one so revolutionary in import—claiming scientific status requires careful scrutiny. Such careful scrutiny, it turns out, produces results that I am sure were not those intended by Panati. The careful scrutiny yields disturbing feelings. But these disturbing feelings are no longer the ones associated with the possibility that Uri is able to bend the laws of nature. Rather, they derive from the possibility that Geller is able to bend the judgments of otherwise competent scientists.

The first inkling that something is not as it seems comes from a simple classification of the types of reports on Uri's accomplishments. Of the 23 contributions we find 18 separate observational accounts or "studies" of Geller. Many of these studies were carried out under conditions that both Panati and their authors admit were informal, uncontrolled, or otherwise badly flawed from a scientific viewpoint. In fact, of the 18 separate studies ten can be so classified. Therefore, even by the editor's standards, over half of the evidence has no scientific status.

What about the eight remaining candidates for scientific respectability? Our careful scrutiny again raises serious doubts. The prime candidate for scientific respectability is obviously the paper by Harold Puthoff and Russell Targ, "Information Transmission Under Conditions of Sensory Shielding," which was pub-

lished in *Nature* on 18 October 1974. This is the only study in the entire collection that has made it through the usual scientific procedure of peer review and ultimate publication in an accredited scientific journal. What Panati and many others seem to overlook is that the paper was not published in *Nature* because of its scientific qualities but in spite of the lack of such qualities. An editorial that accompanied the paper when first published, and which Panati includes here, makes it clear that the editors and the referees judged the study to be weak and inadequate to a point that ordinarily "could be grounds for rejection of the paper." The editorial lists five reasons for printing the paper—none of them having anything to do with its scientific merits—"despite its shortcomings." If it had been a paper on a less controversial subject and one which had not been accompanied by such publicity and widespread rumors, it clearly would not have been accepted for publication.

The strongest of the remaining seven candidates for scientific status would seem to be Eldon Byrd's "Uri Geller's Influence on the Metal Alloy Nitinol." Byrd is an engineer at the Naval Surface Weapons Center in Maryland. He is also a psychologist who has published papers on the alleged ability of plants to telepathically sense harmful intent toward themselves. Nitinol is an alloy of nickel and titanium which has "a physical memory for the shape in which it is formed at the time of manufacture." Byrd reports that on three separate occasions Uri was able to produce a kink or bend in pieces of nitinol wire that resulted in permanent change in the wire's memory. One of the occasions took place at what is called the Isis Center (and not at the Naval Surface Weapons Center, as Panati mistakenly reports), a now-defunct parapsychological organization. As Byrd points out, the claim to "control" in these experiments rests not on careful observation or recording of Uri's behavior, nor on any search of or other constraints placed upon him, but rather on the assumed properties of the testing material. Byrd's case for natural powers on Uri's part depends on two assumptions: (1) that at the time he tested Uri "nitinol was generally not available to the public" and (2) that to effect the sort of change produced by Uri normally requires that one heat the wire to 900 degrees Fahrenheit. Both these assumptions, it now turns out, are questionable. Charles Kalish experimented with nitinol in 1972 (a year before the first experiment with Uri) and developed a magic trick based on its use; the trick was actually marketed by an English magic company. And, separately, the memory of nitinol can be changed by strong mechanical pressures without high temperatures. (Both of these pieces of information come from Martin Gardner). Here, as with the other papers in this collection, review by peers and appropriate experts would have helped.

The case for the remaining six claimants to some sort of control of the observations and conditions is even weaker. Wilbur Franklin's two reports on metal fractures, for example, depend on observations made at Stanford Research Institute. The physicists at SRI, however, explicitly admit that these observations of metal bending were made under conditions that could not exclude trickery. The reports by John Taylor, Albert Ducrocq, and the scientists at Birbeck College, University of London, all suffer from insufficient details and records of Uri's behavior during the alleged psychokinetic happenings. Like Byrd, these scientists

seem to believe that they can compensate for inadequate controls and recording of Uri's behavior by scientific analyses of the deformed or altered materials he leaves behind.

The final contribution that claims some sort of control is that of magician Artur Zorka. Zorka writes that he and another magician, Abb Dickson, "were able to meet with Uri Geller privately for a personal interview and some *controlled* experiments. I italicize controlled because the type of control put on by a magician is different from that of any other investigator. It is a control designed by those who are trained for a profession in the art of deception, to prevent fraud." It is difficult for me, who also once worked as a professional magician and mentalist, to know what Zorka is talking about. Neither magicians nor mentalists have ever devised any standard set of procedures either to prevent fraud or to evaluate psychic powers. At any rate it is clear that, whatever Zorka means by "control," he did not obtain observations on Uri Geller by any means that would approximate scientific controls—objective recording, constraints on Uri's behavior and so on. Moreover, I have talked with two different individuals who have interviewed Abb Dickson about his observation of what took place and, if my informants are correct, Dickson's account differs enough from Zorka's on several key matters to place an entirely different light on the evidence.

So the first careful scrutiny suggests that not one of these reports constitutes "hard" evidence, in the scientific sense, for the reality of Geller's powers. This conclusion comes from considering the papers in isolation from one another. The situation becomes worse when we try to integrate them. The most extensive series of observations of Uri Geller took place at Stanford Research Institute in late 1972 and in the summer of 1973. In all, Uri put in several weeks of performance at SRI, whereas his appearances at other laboratories amounted to one or two visits of a few hours. Indeed, the time that he put in at all the other laboratories and observational settings combined probably does not add up to the amount of time he spent at SRI alone. Yet, in his attempt to provide us with an overview of the case for Uri Geller, Panati seems to completely overlook the astounding fact that, despite all their dedicated efforts, the SRI researchers were unable to come up with any evidence to justify the claim that Uri can bend metal or perform other psychokinetic phenomena by paranormal means. They are willing to grant Uri's paranormal powers only in the realm of perceptual phenomena—telepathy and clairvoyance.

What makes this startling is that the reports from all the other laboratories emphasize Uri's psychokinetic powers. As already mentioned, Franklin's contribution, with its elaborate theoretical analysis, entirely depends on the genuineness of the metal deformation he observed at SRI—a genuineness that the SRI researchers are unwilling to acknowledge.

In his Introduction, which he acknowledges serves as general review for the layman who might otherwise get bogged down in the technicalities of the individual reports, Panati fails to point out other inconsistencies in his case for Geller. For example, he supplies the text of the SRI film on Uri, which describes the experiments on divining for an object hidden in one of a number of cans, influencing a scale, moving the hands of a compass, and affecting a magnetometer. But he does

not discuss the interesting fact that when the SRI physicists came to the point of presenting their case to their scientific colleagues they omitted these experiments from their report. Nor does he or any of his contributors adequately define or discuss the elusive concept of "control" that some of them insist applies to their observations. In the context of these papers it becomes a vague term only remotely related to its usage in specific scientific contexts. For the physical scientists it seems to indicate a fine-grained analysis of the physical properties of objects that have been deformed by Uri. Sometimes it seems to be used in the sense of, "I took adequate precautions to rule out fraud, trickery, or other artifacts." Just what these precautions in fact were and precisely what forms of possible trickery were ruled out is never spelled out. All the studies commit the cardinal sin of failing to report adequately the conditions under which Uri performed and exactly what he did and did not do before, during, and immediately after a supposedly paranormal occurrence. Indeed it is usually unclear both what sort of records were made during the "experiments" and when the record was finally written out. No checks for reliability of observation are mentioned. In a footnote to William E. Cox's contribution we are told that his account was written down two days after the event!

Up to this point my comments have been based on an internal analysis of what Panati has included in his book. The case for Uri Geller becomes even more suspect when we consider what Panati has left out. I would guess that it will be difficult for Panati's "thoughtful reader" to find a clear and unambiguous statement of Panati's criteria for including a paper. My initial impression from the dust jacket and preface was that Panati implies, if he does not actually claim, that book covers all the scientific evidence on Uri Geller. In his preface, in fact, Panati writes that, "The book is written—through papers, reports, diary entries, and letters—by the scientists and professionals who, in various ways, have scrutinized Geller's talents, and feel that Geller is an individual who deserves further scientific attention." It was not until I read further into the book and noted what Panati wrote in his introductions to separate papers that I finally understood what he meant by the preceding quotation. For example, in the introduction to Thomas P. Coohill's report, Panati candidly admits: "The tests reported in the following pages were not executed with the rigor essential for a scientific investigation. They are presented here as anecdotes because Dr. Coohill, a respected scientist, believes that the events he and his colleagues witnessed that day—and two days after Geller's visit—were paranormal in nature." Or, again, in his introduction to the contribution by Lawrence Fried, Panati tells us: "Because of the impromptu nature of the 'thought photography' session between Lawrence Fried and Uri Geller, the following report cannot be taken as positive proof of the occurrence of a paranormal event. . . . Fried recounts all of these things in his brief report, which is included in this book because of Fried's unimpeachable professionalism and expertise with a camera."

In other words, Panati is telling us that for an author to contribute to his book the following must be true: (1) the person must have had a first-hand experience with Uri Geller; (2) he must have status as an accredited scientist or professional; and (3) he must have been persuaded of the reality of Uri's powers. This possibly explains the striking failure to even mention Yael Joel's revealing report "Uri

Through the Lens Cap," which appeared in the June 1974 issue of *Popular Photography* and was later reprinted in Martin Ebon's collection, *The Amazing Uri Geller.* Yael Joel can also be characterized by his "unimpeachable professionalism and expertise with a camera." And, like Lawrence Fried, he too was dumbfounded and had no answers when he discovered Uri's features on the negative in a roll of film that had been snapped with the camera lens covered. It was only when the print was made, and because the camera Uri used happened to have a "fish eye" lens, that Joel caught on to what must have happened and was able to reconstruct the events that enabled Uri to pull off his swindle. But Joel, unlike Fried, does not qualify for even a mention in Panati's case for Uri. This is because Joel ended up by not being persuaded of Uri's paranormal powers.

Panati's emphasis on the case *for* Uri Geller explains why he omits even mentioning the first-hand reports of scientists and professionals who were not persuaded of Geller's authenticity. These include psychologists such as David Marks, Richard Kammann, George Lawrence, Charles Rebert, and myself, among others. They include magicians such as Charlie Reynolds, James Randi, and myself (I am also an experienced magician). Some of the most interesting cases are those involving individuals who at first were persuaded by Geller's performance but who later became disenchanted through reconstruction of the original incident or new information. Among these are medical doctor Andrew Weil and physicists Jack Sarfatti, J. Hanlon, and J. W. Juritz. (Hanlon is mentioned only in connection with his suggestion that Uri might have had a miniature receiver implanted in his tooth. His more plausible criticisms of the Geller evidence are not even mentioned.)

Such omissions are regrettable for many reasons. An attempt seriously to come to grips with questions raised by the observers who have not been persuaded of, or who have changed their minds about, Uri's powers would have lent more credibility to Panati's book as well as enabling the reader to put much of what is presented into a better perspective. For example, Jack Sarfatti was a witness at two of the four sessions with Uri Geller reported in the contribution by John Hasted et al. from Birbeck College, University of London. In a report to *Science News* (20 July 1974) Sarfatti concluded: "My personal professional judgment as a Ph.D. physicist is that Geller demonstrated genuine psycho-energetic ability at Birbeck, which is beyond the doubt of any reasonable man, under relatively well controlled and repeatable experimental conditions." Over a year later Sarfatti wrote to *Science News* (6 December 1975): "On the basis of further experience in the art of conjuring I wish to publicly retract my endorsement of Uri Geller's psychoenergetic authenticity. . . . I have witnessed The Amazing Randi fracture metal and move the hands of a watch in a way that is indistinguishable from my observation of Geller's 'psychokinetic' demonstrations. Also, I am advised of Randi's demonstration of causing bursts in a Geiger counter and of deflecting a compass needle as reported in a letter from Kings College, University of London." Certainly this information from one of the scientific witnesses is part of the total picture that a "thoughtful reader" ought to have in trying to assess the reports from Birbeck and Kings College which are included in Panati's book.

The letter to *Nature* on spoon-bending experiments with six children by Dr. Pamplin and Mr. Collins of Bath University (4 September 1975) would certainly

help to put Taylor's experiments with children and the Geller effect (i.e., the ability of viewers of and listeners to a Geller performance to get their broken watches to run and to bend metal) in a new light. Presumably their experiment is not mentioned because they were able to catch each child on videotape in flagrant acts of cheating when the min-Geller thought the observer was not watching. And the paper by Richard Kammann and David Marks (read at the November 1975 meeting of the Psychonomic Society and subsequently given widespread coverage in the media) takes away much of the apparent mystery in the long paper by E. Alan Price on "The Uri Geller Effect." By ignoring this rapidly accumulating series of reports of first-hand observations that seemingly bring Geller's effects back into the realm of normality Panati has seriously deprived his "thoughtful reader" of the opportunity to reach a balanced conclusion based on the total body of evidence.

Unfortunately Panati misleads his reader not only by what he has left out but also by some of the things he actually says. For example, he strongly hints in the preface and again in the epilogue that "respected scientific journals do not publish the results of well-conducted psychical investigations" because of prejudice against the area of parapsychology. The implication is that many of the papers in this book were rejected for publication on such grounds. I find this misleading on two grounds. It implies that many of these papers meet standards of scientific acceptability when, in fact, not one of them does. And, second, as I have already mentioned, the one paper that did get published made it because the editors leaned over backward and ignored their conventional standards just because they wanted to publish the best case for Uri Geller.

Nor do I think Panati helps the "thoughtful reader" by telling him in the preface that, in investigating Geller, "to prevent fraud the scientists have searched Geller for metals that might be hidden under his fingernails and magnets sewn to his clothing, x-rayed his teeth for evidence of minute electronic devices, bound his hands, blindfolded his eyes, all but stripped him naked." Such a statement does little to dispel the impression that Panati has produced a public relations piece of puffery rather than a serious attempt to evaluate the case for Uri Geller. Other than one or two indications that Uri was probed with a magnetometer and a geiger counter, the cases presented in this book are notable for the lack of any mention of serious attempts to search Geller or to place him under any such constraints as are suggested by the statement.

What, in the final analysis, does this collection of arguments for the paranormal powers of Uri Geller really demonstrate? As I indicated, a first reading might be highly persuasive and unsettling. But the book and its argument falls apart the more closely we examine it. We see that even the subtitle misrepresents the situation. Instead of "Scientific Observations on the Paranormal Powers of Uri Geller" it should read, "Nonscientific Observations That Have Persuaded Some Scientists of Geller's Paranormal Powers." But the book does more than fail to make its case. When we realize that Panati has assembled the best possible scientific case for Geller's powers, we see something that I am sure Panati did not intend. We discover that after almost four years of "cooperating" with scientists in 17 different laboratories in eight different countries, not one systematic,

and repeatable series of observations to document those powers has been obtained. Indeed, in the one laboratory that has had by far the most opportunity to study Geller at first hand, it was impossible to gain any scientific evidence to support his metal-bending and psychokinetic powers. What Panati's book does, then, is demonstrate that scientists are wasting their time in trying to build a case for paranormal forces on the basis of "tests" of Uri Geller. It is Geller, not the scientists, who ultimately "controls" the conditions under which he will operate. In describing their "approach to experimentation" with Uri Geller the scientists at Birbeck College inadvertently reveal the reasons that Geller will either always have his way or will not cooperate with the investigators (see pages 190-96). The one predictable thing about Uri Geller is that, despite promises to the contrary, he will never try to produce his phenomena under conditions that would meet scientific standards of control.

But it would be wrong, in my judgment, to quickly forget about Uri Geller just because the attempt to construct a scientific case for his paranormal powers has failed miserably. This is not the first time that scientists have become involved in a battle of wits with an alleged psychic. Indeed, almost since the beginning of modern science, we can find cases in which outstanding scientists were apparently outwitted by clever tricksters. In the last century alone we have cases like Zoellner and Slade, Crookes and Florence Cook, Lombroso and Palladino, Alfred Russel Wallace and a host of spiritualistic mediums. And, just as in the present case, magicians also came into the picture to maintain that they alone were capable of separating truth from trickery. And, just as in the present situation, there were magicians who were, at least in the first instance, completely baffled by the alleged psychic (e.g. Harry Kellar was originally baffled by Eglinton and John Nevil Maskelyne was completely taken in by Henry Slade).

This raises a host of issues about scientists, magicians, and the qualifications necessary to detect fraud. This is not the place to discuss these issues. But the case of Uri Geller gives us a golden opportunity to examine these issues and see what lessons can be derived from them. If we simply let the Uri Geller Affair fade into oblivion without extracting from it the lessons it has to teach us about the limitations of scientific competence, then we are merely setting ourselves up for a repeat of it in the near future.

The Sphinx and the Megaliths. By John Ivimy. Turnstone Books, London, 1974. 198 pp. $2.95.

Reviewed by Bob Brier

The thesis of this book—that there is a connection between the pyramids of Egypt and Stonehenge in England—is not new to occultists. However, John Ivimy is the first to attempt a carefully researched defense of the theory.

Mystery, most of it misplaced, has traditionally been attached to both the pyramids and Stonehenge. There is surprisingly little about the pyramids that is

not known. The owners of most have been established, plausible building techniques (not involving "little green men") have been suggested, and the uses of the pyramids are almost certainly known. It is true that there is still much to be learned about them, but nothing of the magnitude that is sometimes popularly claimed.

About Stonehenge there is indeed a mystery, but again, not the one popularly claimed. The real puzzle is by whom and how was Stonehenge built? The Druids are about the only people whom archaeologists are certain *did not* build the monument. Stonehenge was ancient by the time the Druids first entered England. Archaeological evidence points to the extraordinarily early date of 2700 B.C. for the first construction of Stonehenge. At that time England was in the neolithic period, and the people in the area of the Salisbury Plain were primitive farmers with no metal tools and no system of writing. The construction and precision of Stonehenge seems to require capabilities well beyond these illiterate people. Recently several archaeologists have suggested ingenious, though not technologically advanced, methods by which such people could have constructed Stonehenge. The author of *The Sphinx & the Megaliths* chooses another approach to the mystery. He suggests that because a monument is erected in an area where primitive people live, it does not follow that these people erected that monument. Specifically, Ivimy believes that a colony of Egyptians is responsible for Stonehenge.

To support this claim he discusses numerous similarities between Stonehenge and the pyramids. For example: (1) at the time of the construction of Stonehenge only the Egyptians knew how to work in stone (actually, if the date for Stonehenge is correct, the Egyptians had not yet built in stone, since the step pyramid of King Zoser is the first structure in stone); (2) there is evidence that Stonehenge could have been used as an astronomical observatory, and the Egyptians were certainly stargazers; (3) both Stonehenge and the pyramids required a similar organization of a large segment of the working populations of each country. When covering the similarities Ivimy's summaries of existing theories are concise and clear; indeed, the book's greatest contribution is probably the large number of references to little-known sources, both classical and modern. They will delight those interested in the subject.

It will be clear to the reader of this book that the author is neither an Egyptologist nor archaeologist. But, in a way, this is in his favor. He is not bogged down by all the dogmas of the field and approaches the question with a fresh viewpoint. I should point out, however, that there are times when the speculative portions of the book get out of hand. "Seventy years may seem like an eternity in the 20th century, but to 'megalithic man' with his distant view of the universe it was surely more 'like an evening gone'" (p. 122). Ivimy thinks that because of certain similarities the Mormons are reincarnated Stonehengers and also that the riddle of the sphinx was used as "an effective means of selecting the brightest youths for entry into the trainee grade of the Greek priesthood" (p. 16)—all of which seems to this reviewer tenuous, at best. Also, the author believes that the ancient Egyptians were reincarnationists, which they were not; they were resurrectionists. But the weakness of some of the speculations and such inaccuracies are a small price to pay for an original work such as this. The originality extends even to the numerous illustra-

tions, most of which are not commonly found in books on Stonehenge or the pyramids. More-speculative occultists will love *The Sphinx & the Megaliths*; conservative readers will, I hope, appreciate it.

Capsule Reviews

Sectarianism: Analyses of Religious and Non-Religious Sects. Edited by Roy Wallis. John Wiley & Sons, Halsted Press, New York, 1975. 212 pp. $13.95.

These essays include studies of the Aetherius Society, Krishna Consciousness, Scientology, the Jehovah's Witnesses, the Unified Family, and a modern communitarian movement that is contrasted with the Shakers. These movements are contrasted with secular cults, including the Concept Houses for drug addiction, Maoists and De Leonists in the political sphere, and two ex-patient therapeutic groups—Neurotics Nomine and Recovery Inc. In addition, there is an interesting piece by Wallis on the definition of cults in sociology, in which he argues that content of doctrine is definitionally irrelevant to distinguishing cults from sects. Wallis argues that what is more important is "the conception of access to the truth or salvation incorporated in the belief-system, i.e., whether it is seen as uniquely or pluralistically legitimate." In addition to their excellent ethnographic content, these studies make a number of important theoretical statements and the volume is a significant addition to the growing literature on deviant social organizations. —M.T.

Keeping Score on Our Modern Prophets. By Kurt Saxon. Atlan Formularies, Eureka, Calif. (P. O. Box 438), 1974. 141 pp. $2.95.

This is an off-beat little book published by a house that seems to specialize in somewhat bizarre books, mostly dealing with mayhem, murder, and espionage methods. Though Saxon is a believer in psi, most of the book is a debunking effort. The chapter titled "Predictions on File" is an inventory of the many failures in prophecy of modern psychics like Jeane Dixon, "Doc" Anderson, Marc Reymont, Carroll Righter, and many others whose predictions have appeared in such places as the *National Enquirer*. The chapter on "Bible Bandits" deals with the common use of biblical quotations to "promote nonscriptural ideas." The chapter on Jeane Dixon is especially interesting in its claim that she was introduced to her occupation as a psychic by a gypsy fortune-teller Saxon tracked down. In addition, Saxon investigates the alleged meetings of Dixon with President Roosevelt and presents strong evidence that they never occurred. Since Jeane Dixon's background and methods have still never been fully examined (though Mary Bringle's *Jeane Dixon:*

Prophet or Fraud?, James Bjornstad's *Twentieth Century Prophecy,* and a fascinating section on her charity organization in Harvey Katz's *Give! Who Gets Your Charity Dollar?* together represent an excellent beginning for her would-be debunkers), Saxon's documentation is highly welcome. —M.T.

Entertaining with "ESP." By Tony "Doc" Shiels. David & Charles, North Pomfret, Vt., 1974. 109 pp. $5.95.

Tony Shiels is well known in magic circles as one of the exponents of what is called "bizarre magick," a highly dramatic form of presentation of conjuring and mentalist effects in a setting of supernaturalism and psychic powers. This sort of magic is the speciality of the periodical *Invocation*, edited quarterly by Tony Raven, and of Shiels' past magic publications like *Daemons, Darklings and Doppelgangers; Something Strange;* and *13!!!* This brand of magic is usually presented as authentic powers. For example Shiels' advertisements for his magicians-only publication, *The Shiels Effect*, states that it will tell the reader "how to bend metal in the Geller style . . . to teleport and levitate . . . to become a successful witch . . . to raise ghosts and poltergeists . . . to become invisible . . . to travel in time . . . to contact monsters . . . AND MORE . . . MUCH MORE!!!"

In *Entertaining with ESP* Shiels' effects are far more modest, but that is to be expected in a book by a magician published for the general public. As such, this is an excellent starting volume for the amateur interested in developing some party skills that will entertain others. The book also has a nice annotated bibliography that should lead the novice to the finer texts of conjuring (like Tony Corinda's *Thirteen Steps to Mentalism*). Shiels, like many mentalist performers who specialize in deception, apparently believes in the reality of ESP, and he devotes his final chapter to the topic. But even there Shiels concentrates on entertainment and avoids the kind of total commitment to psi expected of the reader by other writers of such books, e.g., David Hoy in *Psychic and Other ESP Party Games.* —M.T.

Random Bibliography of Interest

Scott, Gini. "Social Structure and the Occult: A Sociological Analysis and Comparison of the Social Organization, Behavior Patterns, and Beliefs of Two Occult Groups: A Spiritual Growth Group and a Witchcraft Group." Ph.D. dissertation, University of California, Berkeley, 1976.

O'Neil, Carl W. "An Investigation of Reported 'Fright' as a Factor in the Etiology of *Susto*, 'Magical Fright.'" *Ethos* 3 (1975): 41-63.

Robins, Edward. "Magic in Non-Western Societies." *Wisconsin Sociologist* 12 (1975): 22-31.

Cunningham, Raymond J. "From Holiness to Healing: The Faith Cure in America, 1872-1892." *Church History* 43 (1974): 499-513.

Woodrum, Eric Marc. "The Development of the Transcendental Meditation Movement." Paper delivered at the annual meeting of the Southwest Sociological Association, 1976.

Miller, David L. "A Critical Examination of the Social Contagion Image of Collective Behavior: The Case of the Enfield Monster." Paper delivered at the annual meeting of the Midwest Sociological Society, 1976.

Norman, William H. "Transcendental Meditation: Mysticism as Asceticism." Paper delivered at the annual meeting of the Midwest Sociological Society, 1976.

Stewart, James R. "Cattle Mutilations: An Episode of Collective Delusion." Paper delivered at the annual meeting of the Midwest Sociological Society, 1976.

Levin, Ira, Rose Lesniak, and Demetra Joyce. "An Astrological Theory of Introversion-Extroversion." *CAO* (Congress of Astrological Organizations) *Times* 1, no. 4 (1975): 3-7. (A pilot study sponsored by the Midwest Astrological Association.)

Rudwin, Maximilian. "Diabolism." In E. R. A. Seligman, ed., *Encyclopedia of the Social Sciences* (New York: Macmillan, 1935).

Connor, John W. "The Social and Psychological Reality of European Witchcraft Beliefs." *Psychiatry* 38 (1975): 366-80.

Wright, Peter. "Astrology and Science in Seventeenth-Century England." *Social Studies of Science* 5 (1975): 399-422.

Adorno, Theodor W. "Theses Against Occultism." *Telos*, No. 19 (1974): 7-12.

Batson, Daniel. "Moon Madness: Creed or Greed?" *APA* (American Psychological Association) *Monitor* 7, no. 6 (June 1976): 1. (On Rev. Moon and his followers.)

Chavez, John F., and T. X. Barber. "Hypnotic Procedures and Surgery: A Critical Analysis with Applications to Acupuncture Analgesia." *American Journal of Clinical Hypnosis* 18 (1976): 217-36.

Coporael, L. P. "Ergotism: The Satan Loosed in Salem?" *Science*, 2 April 1976, pp. 21-26.

Medawar, P. B. "The Strange Case of the Spotted Mice." *New York Review of Books,* 15 April 1976, pp. 6-11. (Review of J. Hixson's *The Patchwork Mouse.*)

Vicker, R. "How Can You Tell a Quack Witch Doctor from the Real McCoy?" *Wall Street Journal,* 10 March 1976.

Risse, Guenter B. "Vocational Guidance During the Depression: Phrenology Versus Applied Psychology." *Journal of the History of the Behavioral Sciences* 12 (1976): 130-40.

Rines, R. H., et al. "Search for the Loch Ness Monster." *Technology Review* 78 (March/April 1976): 25-40.

Michtom, Madeleine. "Becoming a Medium: The Role of Trance in Puerto Rican Spiritism as an Avenue to Mazeway Resynthesis." Ph.D. dissertation, New York University, 1975.

Moore, Thomas William. "A Round of Gods: Healing Rhythms of Psyche in the Astrological Symbolism of Marsilo Ficino." Ph.D. dissertation, Syracuse University, 1975.

Reis, John Patrick. "'God Is Not Dead, He Has Simply Changed Clothes . . . '" A Study of the International Society for Krsna Consciousness." Ph.D. dissertation, University of Wisconsin at Madison, 1975.

Yeaklely, Flavil Ray, Jr. "Persuasion in Religious Conversions." Ph.D. dissertation, University of Illinois at Champaign-Urbana, 1975.

Barozzi, Ronald Lee. "The Influence of a Meditation Technique and an Eastern Ideology on the Value Orientations of College Students." Ph.D. dissertation, Utah State University, 1975.

Weisz, George. "Scientists and Sectarians: The Case of Psychoanalysis." *Journal of the History of the Behavioral Sciences* 11 (1975): 350-64.

McBurney, Donald H. "ESP in the Psychology Curriculum." *Teaching of Psychology* 3, no. 2 (1976): 66-69.

Wedow, Suzanne. "The Strangeness of Astrology: An Ethnography of Credibility Processes." In W. Arens and S. P. Montague, eds., *The American Dimension: Cultural Myths and Social Realities* (Port Washington, N.Y.: Alfred Publishing Co., 1976).

Garfield, Eugene. "When Citation Analysis Strikes Ball Lightning." *Current Contents,* no. 20 (17 May 1976): pp. 5-16.

Eliade, Mircea. "The Occult and the Modern World." *University of Chicago Magazine,* Summer 1976, pp. 18-21.

Szumski, Richard. "A New Look at Kirlian Photography: Has It Lost Its Halo?" *Fate,* January 1976, pp. 30-38; and February 1976, pp. 73-85.

Evans, E. P. "Superstition and Crime." *Popular Science Monthly* 54 (December 1898): 206-21.

Skinner, B. F. "Card Guessing Experiments." *American Scientist* 36, no. 3 (July 1948): 456-62.

Tart, Charles T. "Models for the Explanation of Extrasensory Perception." *International Journal of Neuropsychiatry* 2, no. 5 (1966): 488-504.

Petersen, Pam. "Mesmerism, Popular Science, and Poe." Paper delivered at the annual meeting of the Popular Culture Association, Chicago, 1976.

Quarantelli, E. L., and Verta Taylor. "Exorcising Data from *The Exorcist* Film." Paper delivered at the annual meeting of the Popular Culture Association, Chicago, 1976.

Simms, L. Moody, Jr., "From Atlantis to UFOs: Today's Search for Alternate Explanations and Possibilities Regarding Mankind's Past and Present." Paper delivered at the annual meeting of the Popular Culture Association, Chicago, 1976.

Sanders, Clinton R. "Video-Possession: Satantism, Witchcraft, and Demonology in Contemporary Cinema." Paper delivered at the annual meeting of the Popular Culture Association, Chicago, 1976.

Report on the "Rationalist Press Association Annual Conference, 1975: Science and the Paranormal." (Including synopses of papers by Antony Flew, John Taylor, C. E. M. Hansel, Trevor H. Hall, E. J. Dingwall, and David Berglas.) *New Humanist* 91, no. 7 (November 1975): 175-85.

Marks, John D. "From Korea with Love." (On Rev. Moon and his followers.) *Washington Monthly,* February 1974, pp. 55-61.

Gilbert, Robert A. "A. E. Waite and the Later Workings of the Golden Dawn." *Spectrum* 1, no. 2 (December 1974): 3-7.

Gilbert, Robert A. "Arthur Machen and A. E. Waite—A Forgotten Collaboration." *Antiquarian Book Monthly Review* 2, no. 4 (April 1975): 7-8.

Gilbert, Robert A. "Baphomat and Son: A Little Known Chapter in the Life of 666." *Spectrum* 1, no. 5 (May 1975): 3-8; and *Spectrum* 1, no. 6 (July 1975): 9-20.

Books Received

Hewes, Hayes, and Brad Steiger, eds. *UFO Missionaries Extraordinary.* New York: Pocket Books, 1976. 175 pp. $1.75. Paperback.

Lame Deer, John (Fire), and Richard Erdoes. *Lame Deer: Seeker of Visions.* New York: Pocket Books, 1976. 277 pp. $1.95. Paperback.

Leonardi, Dell. *The Reincarnation of John Wilkes Booth: A Case Study in Hypnotic Regression.* Old Greenwich, Conn.: Devin-Adair Co., 1975. 180 pp. $6.95.

Ott, John. *Health and Light: The Effects of Natural and Artificial Light on Man and Other Living Things.* New York: Pocket Books, 1976. 222 pp. $1.95. Paperback.

Oyle, Irving. *The Healing Mind: You Can Cure Yourself Without Drugs.* New York: Pocket Books, 1976. 191 pp. $1.95. Paperback.

Panati, Charles, ed. *The Geller Papers: Scientific Observations on the Paranormal Powers of Uri Geller.* Boston: Houghton Mifflin Co., 1976. 317+xiii pp. $10.00.

Roberts, Jane. *The Coming of Seth* (originally titled *How To Develop Your ESP Power*). New York: Pocket Books, 1976. 252 pp. $1.95. Paperback.

Shiels, Tony "Doc." *Entertaining with ESP.* North Pomfret, Vt.: David & Charles, 1974. 109 pp. $5.95.

Steinhäuser, Gerhard. *Jesus Christ: Heir to the Astronauts.* New York: Pocket Books, 1976. 176 pp. $1.75. Paperback.

Sutphen, Dick. *You Were Born Again To Be Together.* New York: Pocket Books, 1976. 253 pp. $1.75. Paperback.

A Last Word

Our article "Scientists as Experts," by Ron Westrum, takes issue with the 1975 "Objections to Astrology" statement signed by 186 prominent scientists. This statement received widespread public attention, including criticism not only by the astrological community (which was to be expected) but also by numerous scientists. His argument for using the judicial model of expert witness and rules of evidence is foreshadowed in the more general argument along those lines presented by philosopher Stephen Toulmin in *The Uses of Argument* (1958). The debate on the statement has continued in the pages of *The Humanist*, through both articles and letters, and interested readers should seek further information there.

Readers interested in considering the problems in doing the kind of research involved in Roy Wallis' retrospective view of the Dianetics movement which later transformed into the new religion of Scientology might wish to examine Dr. Wallis' "Religious Sects and the Fear of Publicity," which appeared in *New Society*, 7 June 1973.

After John Omohundro's "Von Däniken's Chariots" was received by us, announcement came of a soon-to-be-published, detailed critical study of Erich von Däniken's claims. It is Ronald Story's *The Space-Gods Revealed* (Harper & Row), which contains a foreword by Carl Sagan. This is an excellent addition to a growing list of critical works which already includes *The Chariots Still Crash* (1975), by Clifford Wilson, and *Some Trust in Chariots* (1972), edited by Barry Thiering and Edgar Castle.

Following Ray Hyman's completion of his review of *The Geller Papers,* John L. Wilhelm's in many ways revelatory book *The Search for Superman* (Pocket Books) hit the newsstands with further insights into the parapsychological "experiments" conducted at the Stanford Research Institute to "test" the alleged psychic powers of Uri Geller. Wilhelm's descriptions of the tests are quite shocking in revealing the incredibly sloppy conditions surrounding some of the so-called scientific efforts there. If Wilhelm's facts are correct (and there is little reason to doubt them since Wilhelm seems to favor conducting such research), they are damning indeed. Many of the "paraphysicists" have clearly been making the same mistakes that the parapsychologists (of whom the paraphysicists are critical) were making twenty years ago and have long since corrected! But it appears that far more federal (taxpayer) funding has gone into these new investigations than the regular parapsychologists were ever able to obtain, at least publicly. (Readers interested in knowing more about such studies by physicists should consult Martin Gardner's "Magic and Paraphysics" in the June 1976 issue of *Technology Review.*)

Committee News Notes

On 11 June 1976 an invitation was issued to alleged psychic Uri Geller to participate in carefully controlled experiments to test his claims of paranormal powers. The invitation was issued by a subcommittee of the Fellows of the Committee for the Scientific Investigation of Claims of the Paranormal and appeared in full in the July/August issue of *The Humanist.* It ended with these words: "Should Mr. Geller choose not to answer this offer within 45 days, we will deem that decision to be a refusal to appear before the committee and will so inform the news media." We have heard nothing from Mr. Geller or his agent, so must presume his refusal to cooperate with us. We continue to hope that tests of Mr. Geller's claimed abilities "under reasonable scientific protocol as defined by and in the presence of those designated by the committee" may yet be conducted.

****

The Committee has joined with the American Humanist Association in sponsoring two critical tests being conducted upon the astrological/astrobiological claims of Michel Gauquelin. These tests are being conducted under the direction of Professors George Abell, Marvin Zelen, and Paul Kurtz, with the full cooperation of Mr. Gauquelin. The results will be published in a future issue of *The Humanist.*

****

James Randi informs us that the "god-men" of the Indian subcontinent are being taken into the court of public opinion during the Third All-India Divine Miracle Exposure Campaign, led by the well-known rationalist and psychiatrist Dr. Abraham T. Kovoor, president of the Sri Lanka Rationalist Association. At the age of 80 Dr. Kovoor is travelling the subcontinent denouncing the hundreds of mystics in that area as humbugs and criminals.

Dr. Kovoor is willing to put his money where his mouth is, too. He has offered the "god-men" the sum of 100,000 rupees ($12,000) to demonstrate any one of a long list of supposed miracles in his presence, under satisfactorily controlled conditions. He requires that 1,000 rupees be deposited by the claimant before the test, the amount to be forfeited upon failure to perform. So far only one man has come forward, offering one Sri Krishna from Pandavapura as a miracle worker, but he forfeited the deposit without a contest. Another, who has yet to come up with the deposit, says he can stand 30 seconds on hot coals without blistering his feet; the outcome of that is eagerly awaited.

"These miracle men are utter frauds," says Dr. Kovoor, "and resort to philosophical jargon, claptrap, and jugglery to achieve popularity. It is a vast racket, and there is big money in it." The exposure campaign was scheduled from May 18 through June 28, with some fifty lectures and many demonstrations involved.

Dr. Christopher Evans is currently initiating a British branch of the Committee for the Investigation of Claims of the Paranormal. Dr. Evans is well known for his his psychological work on the brain and sleep and for his book on pseudoscience, *Cults of Unreason.*

****

Those interested in the mystery of the Loch Ness and its alleged "monster" should soon enjoy the results of a careful look at the loch by staff of the *National Geographic* magazine currently conducting research there. Staff photographer Emory Kristof told us that their survey had already revealed some geographic surprises and that they would shortly start conducting some standard baiting experiments. In addition to the *National Geographic* report we can also look forward to a forthcoming NBC television documentary special on the loch now being filmed.

Contributors to this Issue

Bob Brier is an associate professor of philosophy at C. W. Post College and an Egyptologist. He has worked as an experimental parapsychologist with J. B. Rhine at the Duke Parapsychology Laboratory.

Daniel Cohen is the author of many books dealing with the paranormal, including *Myths of the Space Age.*

Gary Alan Fine is an assistant professor of sociology at the University of Minnesota.

A. James Fix is an assistant professor of medical psychology at the Nebraska Psychiatric Institute of the University of Nebraska College of Medicine.

Ray Hyman is a professor of psychology at the University of Oregon. His many publications include (coauthored with E. Z. Vogt) *Water Witching U.S.A.*

Paul Kurtz is a professor of philosophy at the State University of New York at Buffalo and editor of *The Humanist* magazine.

Lee Nisbet is executive editor of *The Humanist* magazine.

John T. Omohundro is an assistant professor of anthropology at the State University of New York College at Potsdam.

David Stupple is an assistant professor of sociology at Eastern Michigan University.

Marcello Truzzi is a professor of sociology at Eastern Michigan University.

Roy Wallis is a lecturer in sociology at the University of Stirling (Scotland), Morris Ginsberg Fellow at the London School of Economics and Political Science, and the author of *The Road to Total Freedom: A Sociological Analysis of Scientology.*

Ron Westrum is an assistant professor of sociology at Eastern Michigan University.

NOTES

NOTES

NOTES

NOTES

NOTES

The Committee for the Scientific Investigation of Claims of the Paranormal

Initially sponsored and supported by the American Humanist Association, and first announced in the May/June issue of *The Humanist* magazine, the Committee for the Scientific Investigation of Claims of the Paranormal stated the following objectives:

*To establish a network of people interested in examining claims of the paranormal.
*To prepare bibliographies of published materials that carefully examine such claims.
*To encourage and commission research by objective and impartial inquirers in areas where it is needed.
*To convene conferences and meetings.
*To publish articles, monographs, and books that examine claims of the paranormal.
*To not reject on a priori grounds, antecedent to inquiry, any or all such claims, but rather to examine them openly, completely, objectively, and carefully.

Publication of THE ZETETIC as the official journal of the Committee is a first step in these directions.

Paul Kurtz, Co-Chairman, Philosopher, SUNY at Buffalo,
Editor, *The Humanist*
Marcello Truzzi, Co-Chairman, Sociologist, Eastern Michigan Univ.,
Editor, THE ZETETIC
Lee Nisbet, Executive Director, Executive Director, *The Humanist*

Fellows of the Committee:
George Abell, Astronomer, UCLA; **James E. Alcock,** Psychologist, York Univ., Canada; **Isaac Asimov,** Chemist, Author; **T. X. Barber,** Psychologist, Medfield Foundation; **Richard Berendzen,** Provost, American Univ.; **Brand Blanshard,** Philosopher, Yale; **Bart J. Bok,** Astronomer, Steward Observatory; **Bette Chambers,** President, American Humanist Association; **Milbourne Christopher,** Magician, Author; **Daniel Cohen,** Author; **L. Sprague de Camp,** Author, Engineer; **Persi Diaconis,** Statistician, Stanford Univ.; **Eric J. Dingwall,** Anthropologist, Author; **Christopher Evans,** Psychologist, National Physical Lab., U.K.; **Charles Fair,** Author; **Antony Flew,** Philosopher, Reading Univ., U.K.; **Martin Gardner,** Author, *Scientific American;* **C. E. M. Hansel,** Psychologist, Univ. of Wales; **Sidney Hook,** Prof. Emeritus of Philosophy, NYU; **Richard Hull,** Philosopher, SUNY at Buffalo; **Ray Hyman,** Psychologist, Univ. of Oregon; **Leon Jaroff,** Senior Editor, *Time* Magazine; **Lawrence Jerome,** Science Writer, Engineer; **Phillip J. Klass,** Science Writer, Engineer; **Marvin Kohl,** Philosopher, SUNY at Fredonia; **Lawrence Kusche,** Science Writer; **Ernest Nagel,** Prof. Emeritus of Philosophy, NYU; **James Prescott,** Psychologist, HEW; **W. V. Quine,** Philosopher, Harvard Univ.; **James Randi,** Magician, Author; **Dennis Rawlins,** Science Writer, Astronomer; **Carl Sagan,** Astonomer, Cornell Univ.; **B. F. Skinner,** Psychologist, Harvard Univ.; **Marvin Zelen,** Director, Statistical Laboratory, SUNY at Buffalo; **Marvin Zimmerman,** Philosopher, SUNY at Buffalo.

Institutions given for affiliation only.