

Zetetic scholar

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EDITORIAL

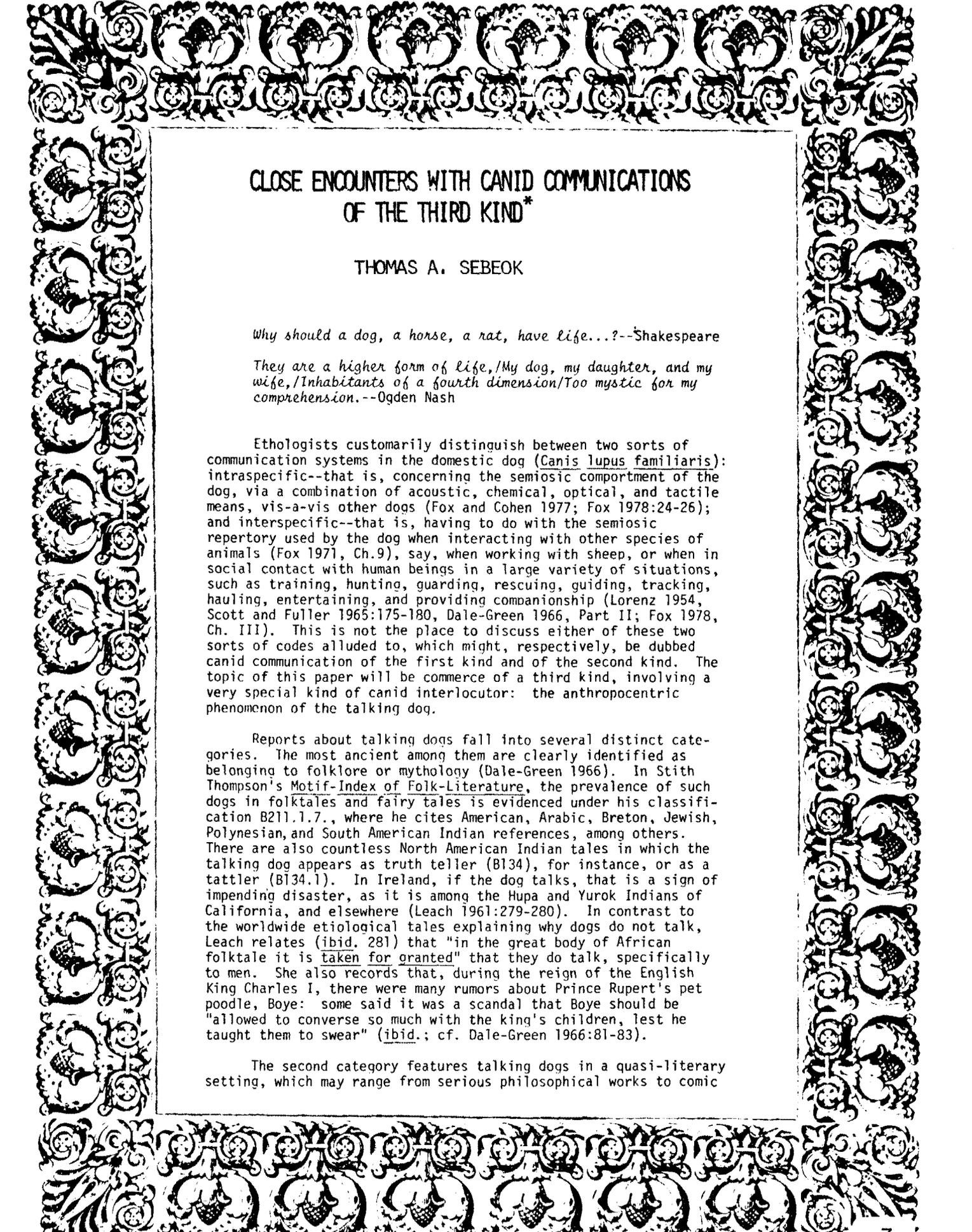
Statistics has long recognized that there are two types of error. Type I error consists of mistakenly thinking that something special is happening when it is not. But Type II error consists of thinking nothing special is happening when something actually is. Most scientists are concerned with avoiding Type I rather than Type II errors. Important exceptions exist when the search is made for something especially important, e.g., a cure for a disease. Then concern shifts to the fear of missing something that may prove quite significant. Similar reasoning exists throughout science but is of particular importance for any discussion of the paranormal. The critics, in general, do not want to make a Type I error and recognize a false anomaly. But most proponents think the anomalies are of such importance and theoretical consequence that they fear missing an important extraordinary event. ZETETIC SCHOLAR, unlike most critical journals dealing with the paranormal, will try to be equally concerned about avoiding both Type I and Type II errors. Science needs to be concerned with both. In fact, the difficulty of being concerned about both is probably at the heart of what Thomas S. Kuhn has called the "essential tension" present in science's need to be both conservative and open to change.

ZETETIC SCHOLAR continues to grow but very much needs your support. For most of you, this issue ends your subscription. Your renewal is urgently needed.

Because of increased printing and postal costs, ZS will now publish only two issues per year. However, these will be larger than those planned when I hoped to issue three times per year. So, subscribers should get as much as they expected. Since ZS remains essentially a one-man operation, less frequent mailings should also give me more time to put together symposia and initiate the dialogues that I hope will become the central feature of this journal.

This issue includes two substantial dialogues on the theories of Dr. Immanuel Velikovsky and on the scientific state of evidence for astrology. As with all materials in ZS, readers are invited to send in their views and reactions.

ZS remains a labor of love, and readers should realize that its attraction will only be for a relatively small group of those interested in responsible, nonsensationalistic, concern for serious scientific evaluation of claims of the paranormal. ZS does not represent either a crusade or an inquisition. Our subscribers will probably never number over a few hundred. So if you believe in what ZS represents, your support is badly needed. Since ZS loses money, its future must remain uncertain. We need about 500 subscribers to break even, so your renewals and your proselytizing on our behalf would be appreciated.



CLOSE ENCOUNTERS WITH CANID COMMUNICATIONS OF THE THIRD KIND*

THOMAS A. SEBEOK

Why should a dog, a horse, a rat, have life...?--Shakespeare

They are a higher form of life, / My dog, my daughter, and my wife, / Inhabitants of a fourth dimension / Too mystic for my comprehension.--Ogden Nash

Ethologists customarily distinguish between two sorts of communication systems in the domestic dog (*Canis lupus familiaris*): intraspecific--that is, concerning the semiotic comportment of the dog, via a combination of acoustic, chemical, optical, and tactile means, vis-a-vis other dogs (Fox and Cohen 1977; Fox 1978:24-26); and interspecific--that is, having to do with the semiotic repertory used by the dog when interacting with other species of animals (Fox 1971, Ch.9), say, when working with sheep, or when in social contact with human beings in a large variety of situations, such as training, hunting, guarding, rescuing, guiding, tracking, hauling, entertaining, and providing companionship (Lorenz 1954, Scott and Fuller 1965:175-180, Dale-Green 1966, Part II; Fox 1978, Ch. III). This is not the place to discuss either of these two sorts of codes alluded to, which might, respectively, be dubbed canid communication of the first kind and of the second kind. The topic of this paper will be commerce of a third kind, involving a very special kind of canid interlocutor: the anthropocentric phenomenon of the talking dog.

Reports about talking dogs fall into several distinct categories. The most ancient among them are clearly identified as belonging to folklore or mythology (Dale-Green 1966). In Stith Thompson's *Motif-Index of Folk-Literature*, the prevalence of such dogs in folktales and fairy tales is evidenced under his classification B211.1.7., where he cites American, Arabic, Breton, Jewish, Polynesian, and South American Indian references, among others. There are also countless North American Indian tales in which the talking dog appears as truth teller (B134), for instance, or as a tattler (B134.1). In Ireland, if the dog talks, that is a sign of impending disaster, as it is among the Hupa and Yurok Indians of California, and elsewhere (Leach 1961:279-280). In contrast to the worldwide etiological tales explaining why dogs do not talk, Leach relates (*ibid.* 281) that "in the great body of African folktale it is taken for granted" that they do talk, specifically to men. She also records that, during the reign of the English King Charles I, there were many rumors about Prince Rupert's pet poodle, Boye: some said it was a scandal that Boye should be "allowed to converse so much with the king's children, lest he taught them to swear" (*ibid.*; cf. Dale-Green 1966:81-83).

The second category features talking dogs in a quasi-literary setting, which may range from serious philosophical works to comic

strips. It was Plato who, in The Republic, characterized the dog as the "philosophical" animal par excellence; the most celebrated contemporary avatar of the philosophical dog is undoubtedly Charles Schulz's creation, Snoopy. There are many novels, short stories and fictive memoirs where all the action is narrated from the viewpoint of a dog hero: one thinks of Buck, in Jack London's The Call of the Wild, who converses with his fellows in "dog language," but is represented as thinking and dreaming in human terms; Virginia Woolf's Flush, who overhears "snatches of talk held in passing with the dogs of Wimpole Street" (1933-39), and could "read signs that nobody else could even see" (ibid. 59); and, above all, of Berganza and Cipión, the principal speakers in Miguel de Cervantes' delightful and widely imitated novella, amounting to an indictment of human conduct, El coloquio de los perros.

Nikolai Gogol, in his 1835 story, "The Diary of a Madman," introduced two dogs who not only talked but also exchanged letters. Gogol's narrator says: "With my own eyes I saw Madgie forming the words, 'I was, bow-wow, I was, bow-ow-ow, very sick.' Talk about a lap dog! I must say I was quite surprised to hear her talking. Later, however, when I had properly sized up the situation, I was no longer surprised. As a matter of fact, the world has seen many similar occurrences before. I've heard that, in England, a fish broke surface and uttered a couple of words in such an outlandish language that scholars have been trying to work out their meaning for three years--so far in vain. Then, too, I read in the newspapers about two cows who went into a store and asked for a pound of tea. But I'll confess that I was much more bewildered when Madgie said: 'I did write you Fidele. Perhaps Fido didn't give you my letter.' Now, I'd be willing to forfeit a month's pay if I've ever heard of a dog that could write" (Gogol 1960:9).

The last story Franz Kafka ever wrote (1924), his serene and tender "Investigations of a Dog," is a perfect manifestation of this genre in the 20th century. The dog informs the reader: "Apart from us dogs there are all sorts of creatures in the world, wretched, limited, dumb creatures who have no language but mechanical cries; many of us dogs study them, have given them names, tried to help them, educate them, uplift them, and so on" (ibid. 5). In this tale, dogs perform toward the dumb creatures the very part the human race ordinarily fulfills toward the dog world, while men are elevated into invisible though still operatively present entities, a rule we customarily ascribe to the incomprehensible powers of the divine.

One of the most remarkable examples of this fictional genre was Olaf Stapledon's fantastic novel about Sirius, a super-sheepdog, created by a scientist by means of certain hormones introduced into the foetus through the mother's blood-stream. This scientist, Thomas Trelone, had originally planned to work with apes, because they "offered the hope of more spectacular success. They were by nature better equipped than dogs. Their brains were bigger, their sight was more developed, and they had hands. Nevertheless from Trelone's point of view dogs had one overwhelming advantage. They were capable of a much greater freedom of movement in our society"

(1944:15). Sirius is brought up as a member of the family, along with a little girl, Plaxy, and achieves "true speech," which is "a sure sign of the fully human degree of intelligence. The baby chimpanzee that was brought up with a human baby kept level with his foster-sister until the little girl began to talk, but then dropped behind; for the ape never showed any sign of using words" (p.26). Sirius far outstrips the Kelloggs' Gua, to whom this indirect reference is made. We are further told that Sirius was so sensitive "to odour and to sound, that he found human speech quite inadequate to express the richness of these two universes" (p.36). Eventually, he was also taught to sing. His adventures at Cambridge were especially noteworthy, even though the famous academics he would converse with there suspected that Trelone "was playing a trick on them" (p.103). Sirius ultimately reverts to wolfhood and comes to a tragic end.

A special case allied to the foregoing category is Dog Toby, of the "Punch and Judy" show, a live dog trained to perform with the puppets in the booth. This dog was sometimes expected to vocalize, even to sing.

Another special case is the use of a dog in lieu of a hand puppet, a shadowgraph, or especially a doll-dummy, in staging a "near" ventriloquist act. In such entertainments, the dog must, of course, be trained to move its jaws in coordination with his master's ventriloquial voice.

Yet a third, quaint category was recently fabricated by the dogfood industry, which, in a blatant attempt to sell more of its product, "has populated our television screens with so many loquacious dogs" (Ziolkowski 1977:22).

In a fourth category belong reports dealing with real dogs that are reputed to actually talk (or, in some instances, sing, as a dog named Zopicus, referred to by Plutarch, and many others, mentioned with increasing frequency since 1650), and it is with these humanised creatures, belonging to the curious intermediate world Horace Walpole designated "dogmanity" (which is not the same as, but probably has common roots with, a more brutal transformation, lycanthropy), that this paper is chiefly concerned. Talking dogs in general exemplify the Clever Hans experimenter expectancy effects, which were named after an illustrious horse of Berlin early in the century, and which were thereafter also most thoroughly explored in horses, although many other so-called "clever animals" are mentioned in the literature, such as mice and rats, bears, cats, "learned pigs," a "goat of knowledge," sealions and even a walrus, innumerable birds, including geese and woodpeckers, the dolphins of 1960's, and three species of African apes in the 1970's--after all, as Emily, "his monkey wife" describes about her relationship with Mr. Fatigay (Collier 1969:14), the chimp is "Something better than his dog, a little dearer than his horse!" Now all talking dog cases of this kind fall into two broad classes: those involving intentional deception (hoax, fraud), and those affected by self-deception in

varying degree, exemplifying the Clever Hans fallacy proper. Those of the second class are much more instructive from a semi-otic point of view, but those of the first are not without points of interest (cf. Sebeok 1979a, Ch. 5).

Dogs can readily be trained to bark in response to cues, imperceptible to other human bystanders, emitted by their operator. Bernhard Grzimek (1975:12:225-226)--no mean observer of the nuances of animal behavior--witnessed one such performing dog barking "answers" to questions addressed to it by its master: "The dog carried out its routine several times without Grzimek noticing any cues being given by the dog's master. Afterward, the trainer told Grzimek that his dog began to bark when the man shifted his weight from one foot to the other, and the dog ceased barking when weight was shifted back to the original foot. The beginning and ending of barking series could be accurately controlled in this way. The trainer had also taught the dog other signs used to communicate commands."

The condensed transaction related by Grzimek suggests a series of questions of capital interest to the semiotician. Among these are:

1. What is the function of the source of the message, the man, as against the function of its destination, the dog? The answer, which comes from stage magic, stems from the principle of misdirection, the basis of which is that the audience will look wherever you artfully direct their attention. In performing a trick, beginners in magic are enjoined never to look at the opposite end of their effect. If you want to know how the illusion of the talking dog is created, keep a sharp eye on the operator, not, as he wants you to, on his subject alone.

2. What channel links the message source (the master) with its twofold destination: a) the subject (dog) and b) the audience (Grzimek)? The answer is by no means self-evident, and needs to be broken down into several parts.

Of course, Professor Grzimek was told that the cue the trainer purposefully emitted consisted of a slight deflection in his own posture. The dog was thus supposedly informed via the optical channel to either commence or cease vocalizing. A whole series of subsidiary questions is immediately spawned: Was the optical channel the only one engaged in triggering the dog's response? If not, what other, secondary channels came into play--the acoustic perhaps, e.g., activated by an alteration in the operator's breathing rate (Johnson 1912:9)? Changes in the operator's muscle tension can likewise be detected by other than the visual avenue, as Johnson's tests with blind dogs has proved; both normal and blind dogs "in ordinary situations rely greatly on kinaesthetic and muscular sense-processes in making their adjustments" (*ibid.* 78), and consideration of the experimental literature on the visual capacity of dogs indicates that "the average dog has far more faulty vision than most dog-lovers suppose" (Warden and Warner 1928:2). If a galaxy of channels was

indeed involved, which of the strands were indispensable, which redundant? Was the operator fully aware of what he was doing and, if so, was his explication veridical? Which channel was under his voluntary control, or employed wittingly, which was not, or out-of-awareness?

If the dog perceived his master's sign behavior, why was the selfsame display imperceptible to an expert human spectator? The last question at least is easily answered by invoking Carl Stumpf's principle of minimale unabsichtliche Bewegungen, enunciated by that eminent scholar, in 1907, in his capacity as the Chairman of the Wissenschaftliche Kommission set up to investigate the horse now known as Clever Hans (Sebeok 1977:1068). Hediger (1974:27-28), in his fascinating excursus into the field of sense organs in the animal kingdom, rightly emphasizes that, as between animals and men, "the animal is frequently the considerably better observer of the two, or is more sensitive than man; it can evaluate signals that remain hidden to man." The range of the channels utilized continues, however, to be unknown, as does the degree of deliberation the trainer may have exercised. Startling as these assertions must seem, they are true even in very thorough experiments conducted by eminent psychologists in which no other animal was implicated, but only "The Control of Another Person by Obscure Signs" (Stratton 1921; this refers to the case of Eugen de Rubini, which I discuss in more detail in Sebeok 1979a, Ch. 4).

3. The account cited leaves no doubt that the dog barked. Why, then, was this animal designated a "talking" dog? The answer is both complicated and intriguing. I propose to return to it at the end of this paper, after some observations about dressage of dogs to perform in the circus, and a brief historical digression about talking dogs in general.

The most perceptive and useful exposition of the techniques for training dogs to perform in the circus--best understood when reread in the light of Bouissac's insightful semiotic approach (1976)--is still the manual by Hachet-Souplet (1897:35-77). This shrewd observer of circus life, especially of animal acts, describes various types of Clever Dogs, including Munito, the "plus connu de tous les chiens savants," who flourished during "la grande vogue des chiens calculateurs..." i.e., the early 19th century (ibid. 36). Munito answered questions pertaining to botany, natural history, and geography, and, at the urging of his master, a certain Signor Castelli, would pick up lettered cards between his teeth to spell out the answers. In 1750, the craft of one Clever Bitch was billed thus: "Chienne savante. L'on espère que les curieux voudront bien honorer par leur présence une chienne qui sait lire et compter par le moyen de cartes topographiques, et qui répond par le même moyen aux demandes que l'on lui fait sur les métamorphoses d'Ovide, la géographie, l'histoire romaine...Elle compte les personnes qui sont dans une assemblée, elle écrit tous les noms propres. Elle démontre les quatre règles de l'arithmétique. Elle désigne l'heure, les couleurs, etc...." (ibid. 36-37).

Hachet-Souplet (*ibid.* 43) quickly disposes of the legend of the Clever Dog, represented as "pouvant tout faire de lui-même, raisonnant comme une personne." The dressage of the performing dog is in fact accomplished by a compromise between persuasion ("par la parole et le geste") and coercion ("par la ficelle et tels carcans appropriés") (*ibid.* 47), according to instructions he sets forth in ample detail, explaining, among many others, the stunts of the great Munito, the likes of his distinguished predecessor, Don Carlos, "The Double-Sighted Dog," who gave a command performance before King William and the royal family at Brighton during the 1830's, and of his many epigones who followed. Especially noteworthy is Hachet-Souplet's instructive explication of the accomplishments of Singing Dogs, which goes as follows (*ibid.* 74-75). Imagine playing a tune on a piano placed side by side with another piano, which is shut and from which the dampers have been removed. A person who puts his ear against the closed piano will hear the vibration of certain chords, whenever the corresponding chords are activated on piano No. 1. A comparable resonance is set off in the vocal cords of the dog whenever the trainer articulates sounds; "et, s'il se trouve que la bête est extrêmement nerveuse, elle perçoit, au gosier, des espèces de petits chatouillements qui la forcent pour ainsi dire à articuler des mots à son tour. Elle a d'abord l'air de s'étrangler, puis se met à hurler en proférant des sons variés se rapprochant de la voix humaine; on peut alors diriger ses lèvres avec les doigts, pour l'habituer à perfectionner son jeu" (*ibid.* 74).² Here Hachet-Souplet properly underlines that it is understood that the vocalizations emitted by the dog "n'ont pour elle aucune signification," although by further patient education, if correctly carried out, he thinks it may be possible to forge in the dog's central nervous system a bond linking the sign to the object--feasible in theory perhaps, but in practice very difficult to achieve. Thirty years later, Warden and Warner (1928) have explored, with an attitude of healthy skepticism, this very proposition in great detail with Fellow, a canine star of stage and screen. Was this dog's éclat for understanding human language justly warranted? The investigators concluded that "there would seem to be no doubt that scores of associations between verbal stimuli and definite responses have been well fixated" (*ibid.* 26) by the patient teaching, over several years, of Mr. Jacob Herbert, this dog's operator. The evidence, on the other hand, for associations between verbal stimuli (signs) and objects or places could never be determined because of the uncertainty, due to the meagerness and inconclusiveness of the data, with respect to the dog's capacity for making a delayed reaction, that is, to disengage from the immediate context. As Bronowski (1974:2548) convincingly argued many years afterwards the time-lag is precisely the most important and basic among the four behavioral criteria postulated by him for distinguishing speech from other animal communication systems.

It is of more than passing interest to note that the dog's intelligence--including "the arithmetical condition of the dog's mind"--was scientifically studied, as early as the 1880's, by Lord Avebury (Lubbock 1866), who experimented with his black

poodle, Van. Avebury was among the first to suggest "that some such system as that followed with deaf mutes" be adapted to further two-way communication with animals (a technique many people imagine was first invented by psychologists only in this decade to enable them to communicate with the apes). Avebury was also keenly aware of what came decades later to be called the Clever Hans Fallacy, which he further, quickly and correctly, connected with the mentalist illusion commonly dubbed "thought-reading." In discussing how a dog may learn to count, he relates an episode, where the operator, a Mr. Huggins, "did not consciously give the dog any sign, yet so quick was the dog in seizing the slightest indication, that he was able to give the correct answer" by barking when he came to the card on the ground with the correct number.

In view of Avebury's prescient sagacity, it is disheartening to read about Chris, the pet dog who flourished in the 1950's in the home of G.H. Wood, in Warwick, Rhode Island, and who "was reported to be able to answer any kind of question put to him" (Pratt 1977:223). Chris made remarkable scores at symbol-card guessing. He indicated his choice by pawing once for a circle, twice for cross, three times for wavy lines, four times for square, and five times for star. Pratt, one of the two associates of J.B. Rhine who observed Chris, although never under proper laboratory conditions--the dog died in 1963, just when "the trained psi research worker" was about to find the opportunity "to investigate further along similar lines" (ibid. 235)--offered three possible explanations for Chris's extraordinary performance. The first, which he favored, was the exercise of the dog's own ESP. The second was "the possibility that the successful agents were unconsciously giving sensory information of what the cards were to the person [*viz.*, Mr. Wood himself] working with Chris" (ibid. 234). The third was the intrusion of "some honest mistake in interpreting the rules they were supposed to follow" thus nullifying the safeguards (ibid.). The latter two, Pratt concluded however, "are hardly within the bounds of reason" (ibid.). The fourth possibility, the one foreseen and incisively delineated by Avebury, and amply proved by others, seems not to have occurred to the committed parapsychologist.

Gould (1978:504) has recently referred to "finagling, doctoring, and massaging"--that is, the unconscious manipulation of data by professionals or, *a fortiori*, by the laity, innocent marks that they mostly are. Consider Leibniz (Ritter 1911:1). In a letter to Grimarest, he had written: "Ich habe soeben einen Brief des Kaiserlichen Prinz-Regenten empfangen, wo Se. Hoheit mir bemerkt, dass er in diesem Frühjahr zweimal auf der letzten Leipziger Messe einen Hund, der spricht, gesehen und sorgfältig geprüft habe. Dieser Hund hat deutlich mehr als 30 Worte ausgesprochen, sogar ziemlich sinngemäss seinem Herrn geantwortet. Er hat auch das ganze Alphabet ausgesprochen, mit Ausnahme der Buchstaben m, n, x." Leibniz also sent a letter, through the intermediary of the mathematician Pierre Varignon, in 1715, to the Abbé de St. Pierre, where the report caused a sensation at the Académie Royale des Sciences (Observations 1718:3-4). As Varignon had already written

to Leibniz (1962:194) on August 9, 1713, "L'histoire du chien parlant a cause ici [Paris] d'autant plus de surprise qu'elle seroit incroyable si vous n'asseuriez l'avoir aprise d'un Prince qui l'a entendu parler dans une Foire, où une infinité d'autres personnes en doivent avoir été temoins: sans doute que le maitre de ce chien ne manquera pas de le promener par toute l'Europe: s'il vient ici, il en remportera seurement beaucoup d'argent, quoyque ce chien ne parle qu'Allemand que peu de gens de ce pais-ci entendent, lui suffisant pour la curiosité dont on est ici, que son chien y prononce les lettres de l'Alphabet que vous me dites qu'il scait prononcer." Leibniz (*ibid.* 199), in an undated missive, then amplified further: "Je suis maintenant témoin oculaire et auriculaire du chien parlant; entre autres mots il a bien prononcé Thé, Caffé, Chocolat...."

It was naturally very convenient that this native German speaking dog could scarcely commune with a Francophone audience; Varignon's stricture prompts me, however, to repeat my wonderment already expressed in another place (Sebeok 1979, Ch. 4): how was the con perpetrated on an intellectual eminence of the stature of Leibniz? Before I attempt to explain this, I might adduce the case of Rolf, the astounding Airdale terrier of Mannheim (Larguier des Bancels and Claparède 1915, MacKenzie 1913), whose wondrous reputation persists to this very day (e.g., Jutzler-Kindermann 1954:39-53, Borgese 1968:9, Rowdon 1978, *passim*). Probably the most telling incident about Rolf was that he suddenly became ill shortly after the arrival of the Swiss psychologist, Professor Claparède, the scientist who was to have subjected his ability to a series of critical tests (Warden and Warner 1928:14). This little detail is almost never mentioned in the many colorful yarns about Rolf; (it reminds me of nothing so much as the authenticated [Randi 1978:28] refusal of the Israeli "psychic," Uri Geller, to go on stage in Birmingham after receiving word backstage that the front row was packed with magicians, making up, instead, a story about a bomb threat, thus forcing the cancellation of his show).

The Clever Hans affair gave renewed impetus to a dialectic launched by Descartes and others in the 17th century, then carried far forward by Julien Offray de la Mettrie, the 18th century physician and forerunner of the behaviorist position, with his sharp rejection of Cartesian dualism. The issue centered on the question whether language and the cognitive structure assumed to underlie it was the critical feature of *Homo sapiens* which separated him from the speechless creatures. The thorough but amateurish book of Krall (1912) and the equally thorough but far more expert response of Maday (1914) well represented opposing points of view on this topic, respectively. Griffin's recent (1976) monograph is a most important contribution to this protracted argument, which shows no signs of abating (Shepard 1978). The center ring of contention has shifted (at least in the United States) from horses to marine mammals in the 1960's (Wood 1973, Ch. 5), but is now solidly occupied by chimpanzees (*Pan troglodytes* and an occasional *Pan paniscus*), along with a handful of Bay Area gorillas (Sebeok 1979b). The side shows continue to feature the most varied sorts

of animal species, among which "educated horses" (Bouissac 1976:52) still appear with reliable regularity (Blake 1975, 1977), while birds such as the Gallic Greater Spotted Woodpecker whose communicative bond with man is alleged to be "analogous to that found by the Gardners and Prenack [sic]" in apes in America (Chauvin-Muckensturm 1974:185) and whose "bec est au moins l'égal de la main du chimpanzé" (ibid. 207), remain perennial favorites.

Amidst the long but inconclusive disputation, always emotionally charged and often acrimonious, about the uniqueness of language and/or mind, and the attendant literature dealing with language learning in infrahumans, dogs are continually being brought forth for incidental consideration. On occasion, talking dogs are exalted to the front of the stage. Thus Krall (1912: 211-224) describes in some detail (and with photographic illustrations) the conversational ability of Don, a German setter belonging to the royal game warden, Hermann Ebers, at Theerhutte, in Gardelegen--"Ich hatte wiederholt Gelegenheit, Don, den berühmten Hund... 'sprechen' zu hören," he records (ibid. 215); I shall return to this case below; see also Sebeok 1979a, Ch. 4). He gives quite a few other instances, concluding (ibid. 220): "Aus all diesen Veröffentlichungen durfte wohl zur Genüge hervorgehen, dass die Sprechbegabung des Hundes nicht gar so selten auftritt, wie wir bisher annehmen mussten..." His acute critic, Maday (1914:228-229), refers to talking dogs only in passing. He correctly refocuses the problem on two more specific questions: 1. does the dog speak imitatively; and 2. does the dog conjoin a string of sounds, or signifiers, with a particular object or goal? He points out that experts, like Oskar Pfungst--the solver of a Clever Hans condundrum--and Paul Kammerer (whose own troubles with allegations of scientific fraud lay far in the future--see Koestler 1973, Sebeok 1979a, Chs. 4, 5), both answered the first question decidedly in the negative, but held that, even if the answer turned out to be in the affirmative, dogs would merely have achieved the level of what parrots do. The second question, Maday acknowledges, is "zumeist zugegeben," but we saw earlier that Warden and Warner remained dubious about this concession even a decade and half later.

In the interwar period, some four score and more dogs were "educated," and the interest of scientists, writers (such as Maurice Maeterlinck), animal trainers and circus folk, as well as numberless dilettantes, converged on thinking, talking, counting canines. (Inexplicably, it turns out that 66% of these anthropoid dogs were instructed by women!) In 1954, Henny Jutzler-Kindermann, who identified herself as an agronomist, brought together experiences and observations, ranging from 1890 to 1953, about 16 horses, one cat, and no less than 88 dogs. Her sedulous collection--obviously a labor of love--accentuates the positive. Actually, it is a gallimaufry of unreliable lore in which rare nuggets of useful information are embedded here and there. Graham Greene, in his amusing essay, "Great Dog of Weimar," poked gentle fun at "Lola Kindermann, the airedale, and her father Rolf Meckel [read Rolf Moeckel], of Mannheim"; (for the case history of Lola,

see Jutzler-Kindermann 1954:56-76; when Lola was asked to explain how dogs knew so much, she replied that all dogs have taken an oath--like honorable stage magicians--not to reveal this secret--cf. Jastrow's devastating, if short, analysis of this case, 1935: 213-214). Greene remarks (1969:323) that he has "always suspected dogs: solid, well-meaning, reliable, they seem to possess all the least attractive human virtues. What bores, I have sometimes thought, if they could speak, and now my most appalling conjectures have been confirmed." A more sinister concern with Mrs. Kindermann's Clever Dogs was evinced by Hermann Goering, who proposed that their talents for communication be put to serve the State's interest, but there is no indication that they were thus actually used by the Nazis. (A similar role was later envisioned for the possible naval utilization of small whales; this program is discussed, under the heading "Kamikaze Porpoises," by Wood [1973:209-220]).

The Jutzler-Kindermann omnibus by no means satisfied the specialized clientele attracted to this sort of Museum of Canine Wonders. There followed the book of Mrs. Borgese (1968, Ch. 2), with her saga of some English setters, notably one Arli, whom she claimed to have taught to read, as well as, beginning in 1963, to type--including spontaneous "concretist" poetry--on an electric Olivetti. Mrs. Borgese believed that, at least in instances where his motivation was high, Arli did associate a string of letters with a particular signified (*ibid.* 53). Since typing is almost always an automatic process--for human secretaries as much as for dogs--the implied resemblance is discomposing. Emily Hahn (1978:35) noted, not altogether reassuringly, "Sometimes Mrs. Borgese forgot ...that Arli couldn't really read or write.... She found it necessary now and then to remind herself of this, otherwise she would be getting false notions." In fact, Mrs. Borgese's style of writing is so effusive that her interpretations of her dogs' feats are, to put it in the kindest way, ambiguous.

The latest--but doubtless not the last--contribution to this flourishing genre is by Rowdon (1978). His book, that rather reads like artless fiction, reveals a naivete about unconscious sensory cueing which is hard to endure, and is embogged in every imaginable trap in which the Clever Hans Fallacy can inveigle an unwary mark, as well as implicated in a few novel perversions. The operator Hilde Heilmaier, of Berchtesgaden, and Dorothy Meyer, a teacher in her employ, seemingly convinced this writer that, in witnessing two dogs tapping a la Hans, he was privileged to be in the presence of what he repeatedly refers to by a catch phrase, once suggested by Krall, as "a second Copernican revolution"(e.g., in the title of Ch. 2). These dogs, Belam, a saluki, and Elke, a poodle, were not just language-endowed but became bilingual. Since they were slated to appear on television in Anglophone areas, "Mr. Heilmaier now felt that the dogs should learn a little English" (*ibid.* 156). It apparently never entered Rowdon's mind that any tapping dog "knows" every natural language equally well, since the gesture is initiated and terminated by a verbal message--minimale Bewegungen--wittingly given (as in a con), or unwittingly given off (as in instances of self-delusion) by the operator. Rowdon ends his book,

speculating about "our future," on a note of unkempt Oriental mysticism, which will do the dog family no good at all; (it is instructive to contrast his treatment in this respect with the splendid study by Rensberger [1977], especially as to the wisdom of the new conservation ethic that the latter calls for).

"Talking dogs" is a popular cover term for a variety of inculcated canid semiotic comportments, tapping being one prominent form that such transmissions take. The tapping behavior of dogs and, of course, of horses, is analogous to the drumming out of messages by a woodpecker to its human feeder (Chauvin-Muckensturm 1974); to the transfer of information by means of certain noises from dolphins to their handlers (nota bene, there being "little reason to believe that they have a means of communication that can be considered a true language" [Wood 1973:118]); as well as, in some of its most salient aspects, to silent gestural message transmittance between literally enthralled apes and their otherwise enthralled trainers (Sebeok 1979b). Space limitations won't permit me to pursue the ins and outs of this postulated semblance here. I should like, instead, to take a closer look at another sort of "talking dog." Don, the dog that, by all objective accounts, barks, yet is widely avouched to have spoken, will serve as the prototype.

First, it is appropriate to call attention in this connection to the admittedly fuzzy notion of "presupposition," in the extended, pragmatic sense. Karttunen (1973:169) says that "To presuppose something as a speaker is to take its truth for granted and to assume that the audience does the same." In other words, according to this conception, presuppositions inhere in communicating organisms; they are not a property of the messages flowing between interlocutors. In the framework of the case under discussion, and all others like it, three or four parties are assumed to be co-present: 1. the barking dog, or message source; 2. the listening and often eliciting human, or message destination; 3. the operator; and 4. the optional but customary accomplice. For brevity, as well as to properly frame the situation, I rename 1. the subject, 2. the mark, 3. the con, and 4. the skill.

The con must see to it, to begin with, that the wanted presuppositions get planted in the mark, or, according to another terminology, that an appropriate "semiotic key" (Bouissac 1976: 190) is turned on. This is usually accomplished with the assistance of a skill, who is, in an ordinary confidence game, the decoy whom the mark sees winning--the "fourth business" (to introduce a theatrical figure), who lurks, so to speak, in the wings. The skill is used to impart an aura of legitimacy to the proceedings, increasing the probability that the mark's expectations will be fulfilled. Skill-induced presuppositions work much in the manner of medical ritual trappings that tend to assure that the placebo being administered will effectively relieve some complaint, or as does the Cyrano-like audience perspective that furthers the enactment of the dramatic episode commonly known as the hypnotic scene (as is evident from Theodore R. Sarbin's beautiful dramaturgical

metaphor of hypnosis, also supported by Ernest R. Hilgard's allied concept of the "hidden observer"--see also Sebeok 1979a, Chs. 10 and 5).

In the circus world, the keying begins with carefully planted advance publicity, reinforced by the circus poster, the unique semiotic status of which Bouissac (*ibid.*, Ch. 10) has described so elegantly. The media, as it were, are placed in the role of a shill. In the matter of Don, for instance, the press served as a quasi-metonymic shill, much as the semi-private epistles of Leibniz to Paris did two centuries earlier: their function was to create the atmosphere of confidence, or the felicitous sincerity conditions (Karttunen 1973:170), initially required for playing the illusion out. It couldn't help affecting Leibniz's own sensibility that no less a personage than the Imperial Prince Regent himself had told him that he had twice seen and scrupulously tested the dog at the Leipzig fair (see above). Just so, the French recipients of Leibniz's letters in turn placed their full reliance on the distinguished philosopher's report, even though, up to that time, Leibniz had not yet himself witnessed the dog's act. It is no coincidence that "Extensive comment has been made in the German and even in the American daily press on the reported conversational ability of 'Don,'" and that "Numerous observers reported that he had a vocabulary consisting of eight words...." (Johnson 1212:749). The requisite presuppositions had thus been put in place by the journalists, and the rumor mill ground out its curtain raising work of metasignification. This sour view of publicity is in line with previous research suggesting "that the impact of newspaper stories may be at once more general and more grave than was previously suspected" (Phillips 1978:749). The show could now begin.

What the main act consisted of was Don speaking (not tapping or typing), "if food were held before him and the following questions propounded: 'Was heisst du?' 'Don.' 'Was hast du?' 'Hunger.' 'Was willst du?' 'Haben haben.' 'Was ist das?' 'Kuchen.' 'Was bitterst du dir aus?' 'Ruhe.' Moreover, he was set to answer categorical questions by 'Ja' and 'Nein'; and in reply to another question to speak the name, 'Haberland'" (*ibid.*). Like the horse Hans, Don could be questioned in any language. Unlike Hans, however, he replied not in a sign language but in spoken German. One of Don's interlocutors chanced to be Oskar Pfungst, the same gifted psychologist who had figured out the correct solution to the Clever Hans problem, and who, of course, by no means shared the presuppositions of the general public. Pfungst thoroughly investigated, in part on the basis of a number of phonographic documents that he had recorded, the dog's behavior. His conclusions, first briefly published in a newspaper supplement, on April 27, 1911, were subsequently summarized in *Science* by Johnson (1912), himself an expert on the acoustic behavior of dogs.

In the process of recording, a curious acoustic transformation occurred--or so it seemed. In place of the real Don's spoken German, the recorded Don produced only disyllabic and monosyllabic

noises which, to disinterested hearers, i.e., those whose presumptions had not been doctored, sounded like nothing so much as ordinary barking. Pfunst quickly established two facts:

- a) that the dog invariably answered all questions with answers, from "Don" to "Ruhe," ordered in the same sequence--if the arrangement of the elements in the questioning changed, his responses turned out to be inappropriate or "ungrammatical"; and
- b) that Don learned nothing by observation and imitation.

Pfunst then decided, correctly, "that the speech of Don is therefore to be regarded properly as the production of vocal sounds which produce illusions in the hearer" (*ibid.* 750). The explanation for this susceptibility derives from the circumstance that "the uncritical do not make the effort to discriminate between what is actually given in perception and what is merely associated imagery, which otherwise gives to the perception a meaning wholly unwarranted; and they habitually ignore the important part which suggestion always plays in ordinary situations" (*ibid.* 751).

The riddle of the talking dog thus stands fully elucidated: when the effect is not altogether chimerical, the underlying mechanism must either be intentional cueing (deception) or unintentional cueing (self-deception). Perhaps it is as well to recall here a story told of Dr. Samuel Johnson, in whose presence a fatuous lady made her dog perform tricks. The Doctor was unimpressed. The lady exclaimed, "But, Dr. Johnson, you don't know how difficult it is for the dog!"³ Dr. Johnson replied, "Difficult, Madam? Would it were impossible."

A singular feature of quantum mechanics that has emerged from a recent series of experiments appears to be the possibility that the observer's knowledge or ignorance has some influence on the state of the particle observed. In semiotics, it is an established fact that, in searching for an understanding of the nature of dyadic communication, we have often been misled to search for in the message source what should have been sought in the destination (Sebeok 1979a, Ch. 5). Our brief but close encounters with canid communication of the third kind have once again underscored this common error: the secret is concealed not in the dog, but in the man. Accordingly, this paper was meant as a reminder that the actor's influence is decisive, and of the correlated fact that presuppositions (or, more generally, context) are ignored at one's peril.⁴

NOTES

*An abbreviated version of this article is appearing in Wechselbeziehungen Diachroner und Synchroner Sprachwissenschaft: Festschrift für Oswald Szemerényi zum 65. Geburtstag (Amsterdam: John Benjamins, 1979).

¹Martin Gardner (personal communication) recently wrote me:
"Years ago a magician told me he once worked a show with a mind-

reading dog act, and the owner had a 'stooge' in the audience who did all the cueing simply by moving a thumbnail to make a slight snap sound. I can't vouch for the truth of this, but the fingernail method seems to be a simple way to cue a dog. Even the trainer, on the stage, could do it with his hands behind his back."

²Heini Hediger (personal communication) calls my attention to Caro, a "singing" fox, who could range over almost two octaves; see also Schmid 1938:108-109.

³Gardner's favorite talking dog joke, and mine as well, is the oldie about the man who showed a booking agent his dog act. One dog sang while another played the piano. The agent, much impressed, wanted to know how he ever trained the dog to sing so well. "Confidentially," said the trainer, "he can't sing a note. The dog at the piano is a ventriloquist."

⁴Sebeok 1979a (especially Ch. 10), discusses in detail Jakob von Uexkull's semiotic concept of "Umwelt-Forschung." The talking dog phenomenon was disposed of, in this particular framework, thirty years ago, by the Menzels, the most knowledgeable specialists of their day as concerns various facets of the social relations prevailing between dog and man, and the designers of many fine experiments with dogs (see, for example, Katz 1937:10, 40, 41, 46, 66, 87, and 88). Since their article (1948) appeared in an obscure and inaccessible magazine, I would like to quote their pertinent paragraph in full:

"Der Fehler liegt in der Annahme, die voraussetzt, dass der Hund mit dem Menschen die gleiche Gegenstands- und Tätigkeitswelt gemeinsam hat, dass die gleichen Interessen ihn bewegen. Die heutige Wissenschaft weiss, dass der Hund, wie jedes andere Tier, auf Grund seiner biologischen Andersartigkeit in einer anderen Umwelt lebt als z. B. der Mensch oder die Maus, d. h., dass der Ausschnitt der Welt, den er erfasst, eben ein hundlicher und kein menschlicher ist. Es ist ein krasser Anthropomorphismus, anzunehmen, dass Hunde sich für Literatur oder Kunst, für Politik und Weltanschauungsfragen interessieren, sie interessieren sich für ihre "hundlichen" Dinge, für Futter and Lager, für vierbeinige Gefährten oder Feinde, für Geschlechtspartner, für Spiel and Kampf, für Geruche, die biologische Bedeutung für sie haben usw. Ihre und unsere Interessen treffen sich nur auf einem kleinen Ausschnitt der beiderseitigen Umwelten; es ist die Kunst der Abrichtung, diesen Ausschnitt etwas zu vergrössern und den Hund ausserdem dazu zu bringen, in diesem Bereich

1. zu verstehen, was wir von ihm wollen,
2. das Verlangte auch auszuführen."

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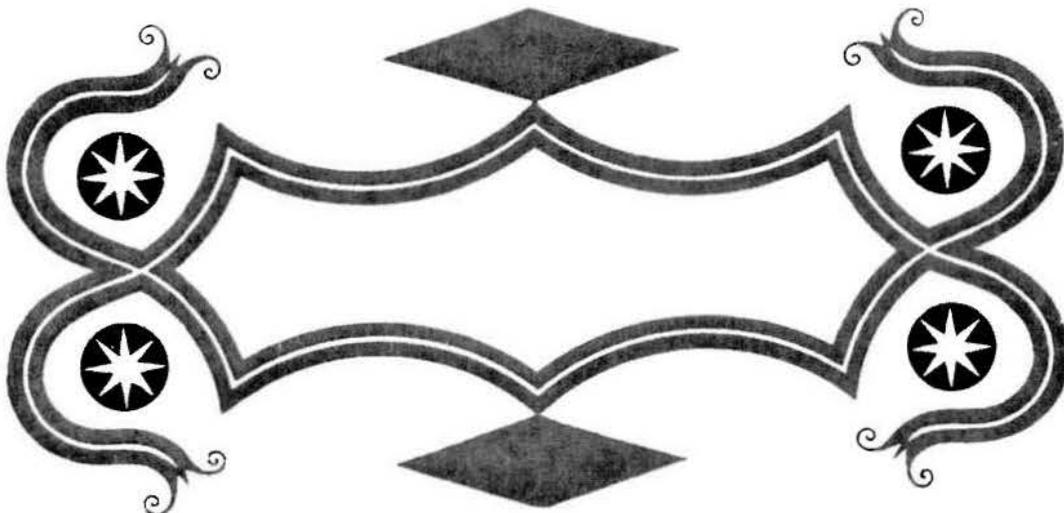
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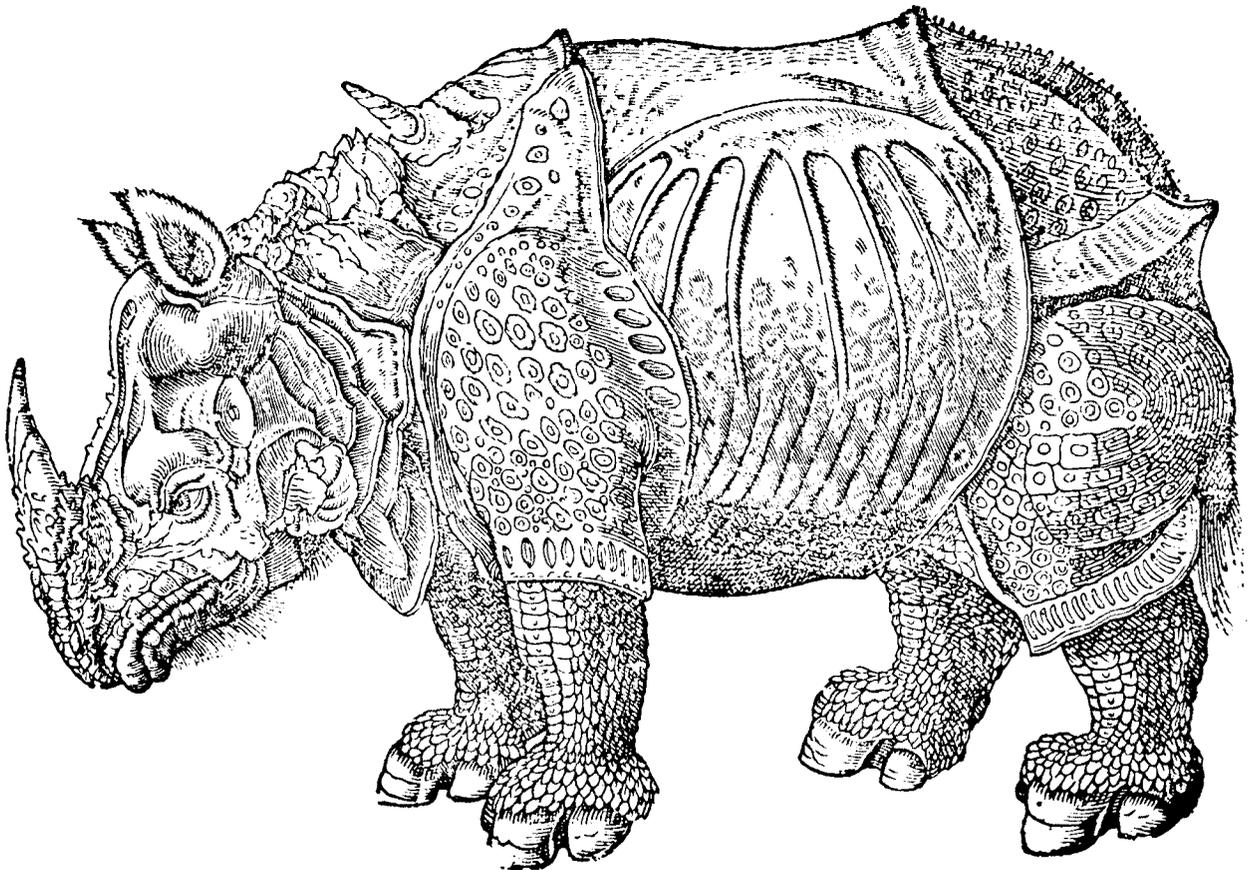
[Our special thanks for help from Harry Senn.]

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QUOTEWORTHY

"We certainly are not to deny whatever we cannot account for. A thousand phenomena present themselves daily which we cannot explain, but where facts are suggested, bearing no analogy with the laws of nature as yet known to us, their verity needs proof proportioned to their difficulty. A cautious mind will weigh well the opposition of the phenomenon to everything hitherto observed, the strength of the testimony by which it is supported, and the errors and misconceptions to which even our senses are liable."

--Thomas Jefferson
"Letter to Daniel Salmon," 1808
[Sent in by Ron Westrum.]

"The important thing is not to stop questioning."

--Albert Einstein

"Men become civilized not in proportion to their willingness to believe, but in proportion to their readiness to doubt."

--H.L. Mencken

"Every society honors its live conformists and its dead troublemakers."

--Mignon McLaughlin

"Skepticism, like chastity, should not be relinquished too readily."

--George Santayana

"There is a superstition in avoiding superstitions,"

--Francis Bacon

"There is apparent force in the argument that our national histories are founded, accepted, and trusted on evidence by no means as direct as that by which it is claimed the proofs of spiritual miracles are accompanied. But it must be remembered that the facts of profane history are vouched for by evidence which is in accord with our present experience; they are in harmony with all that is now going on in the light of day, and we are justified in accepting them on testimony, however indirect, which is nevertheless at one with the ordinary course of events. But the phenomena of spiritualism have no such support; they are commonly regarded as in contravention of the ordinary experience of mankind, and no indirect testimony concerning them can be admitted without the most thorough, the most searching scrutiny."

--From the Report of the Seybert
Commission on Modern Spirit-
ualism, 1887.
[Sent in by Laurent Beauregard.]

"It is a mistake to believe that a science consists in nothing but conclusively proved propositions, and it is unjust to demand that it should. It is a demand only made by those who feel a craving for authority in some form and a need to replace the religious catechism by something else, even if it be a scientific one."

--Sigmund Freud

"No amount of experimentation can ever prove me right; a single experiment can prove me wrong."

--Albert Einstein

"If telepathy means the hypothesis of a new force, that is, the assumption of an as yet uncomprehended mode of the output of energy, subject rigorously to the physical bonds of material causation which make possible a rational conception of psycho-physiological processes; and if, further, some one will put forth a rational conception of how this assumed action can take place apart from the exercise of the senses, I am prepared to admit that this hypothesis is (not sound, or strong, or in accordance with the facts, or capable of explaining the facts, or warranted by the facts, but) one which it is legitimate, though perhaps not profitable to consider. If, however, telepathy is put forward as a totally new and peculiar kind of action, which is quite unrelated to the ordinary forces with which our senses and scientific observations acquaint us, and which is not subject to the limitations of the material world of causation; if telepathy is supposed to reveal to us a world beyond or behind or mysteriously intertwined with the phenomena of this world,--a world in which events happen not in accordance with the established physical laws, but for their personal significance even in defiance of those laws,--then it becomes impossible for the scientist to consider this hypothesis without abandoning his fundamental conceptions of law and science."

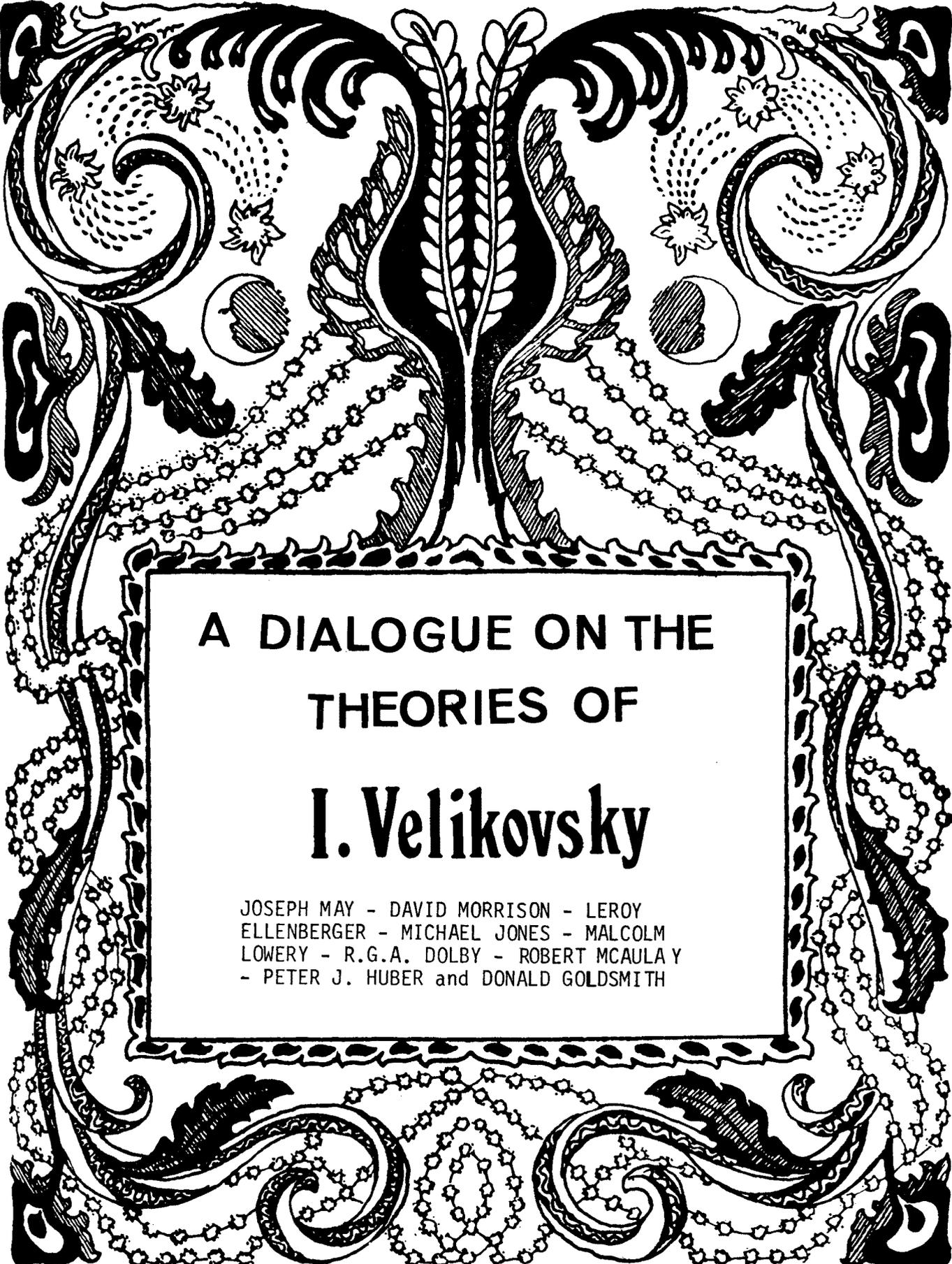
--Joseph Jastrow
Fact and Fable in Psychology, 1900
[Sent in by Laurent Beauregard.]

"Beware of false knowledge; it is more dangerous than ignorance."

--George Bernard Shaw

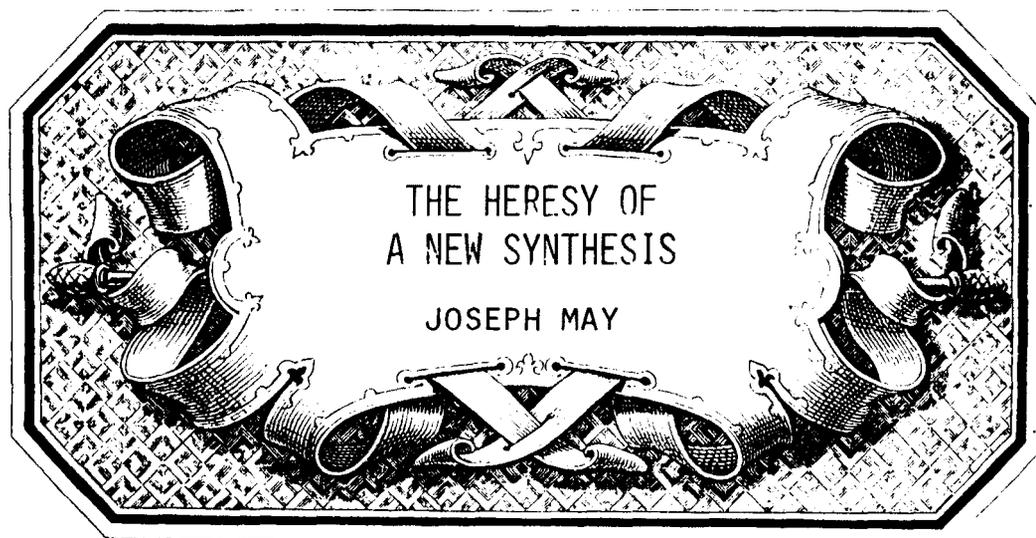
"Though a good deal is too strange to be believed, nothing is too strange to have happened."

--Thomas Hardy



**A DIALOGUE ON THE
THEORIES OF
I. Velikovsky**

JOSEPH MAY - DAVID MORRISON - LEROY
ELLENBERGER - MICHAEL JONES - MALCOLM
LOWERY - R.G.A. DOLBY - ROBERT MCAULAY
- PETER J. HUBER and DONALD GOLDSMITH



It is well known that the history of science contains many instances in which the orthodoxy of an age has been later overthrown, even concerning fundamental matters thought to be well established. This experience should serve as a caution against hasty rejection of theories going against the conventional wisdom, but it frequently fails to do so because in every period all kinds of deviations from commonly accepted assumptions, both valid and invalid departures, have been indiscriminately labelled as nonsense at the time. Indeed, viewed quantitatively, most of the heresy has earned the label, and this outcome is the crux of the problem faced by an honest seeker after the truth: on one side lies the danger of smothering some truth which is ahead of its time, hence likely to be rejected for just that reason; on the other side is the impossibility of giving full attention to every suggestion that may come along.

It appears that some guidelines are needed capable of directing efforts towards the more promising areas of heterodox thought while preventing a waste of time. Preferably they should be few and simple; ideally they should be incapable of distortion in application. Although the ideal may be impossible of achievement, perhaps we can find the former if we turn to some elementary rules of logic. My proposal is this: let us remove the presumption of falseness from an unconventional theory if it can pass some preliminary tests: (1) Is it coherent? (2) Does it make a prima facie case? In cases where failure has occurred, let the reasons be specified explicitly and not just asserted confidently with vagueness. Unless this can be done convincingly, we should then treat the unorthodox proposals with respect, hopefully gaining enough time for investigation and not prejudice to determine the question.

My suggestion of the above guidelines is prompted by a case in which I think they might have been of some help. For nearly thirty years the theories of Immanuel Velikovsky have been widely discussed

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J. Donald Adcock, Thomas Copeland, H.C. Dudley, David France, Karl Keating, Donald McLennan, Irving Michelson, Brendan Minoque, C.J. Ransom, Lynn E. Rose, Elisheva Velikovsky, Willis Webb, Ronald Yobe.

Any errors should be attributed to the author and not to the readers or to Dr. Velikovsky.

within the academic community, though not always intelligently so. Critical response has ranged from sweeping generalizations to minute analysis of particular points, but typically the critics have demonstrated a lack of understanding of the structure of thinking that they were undertaking to evaluate. While not attempting to narrate the debate, I will try to show that by the reasonable standards just suggested Velikovsky's ideas have been entitled to a different kind of reception than they have received.¹

II

Immanuel Velikovsky is of Jewish heritage, born in Russia in 1895. He began the study of medicine before the First World War, receiving his medical degree from the University of Moscow in 1921. The next year he moved to Germany where he founded and was general editor of the Scripta Universitatis series of monographs by eminent Jewish scientists worldwide. Albert Einstein became the editor of the mathematical-physical section and contributed an article himself. In 1923 Velikovsky settled in Palestine where he practiced medicine and psychoanalysis. He wrote a number of papers in these fields, some of which appeared in Sigmund Freud's journal, Imago. Velikovsky also corresponded with Freud from time to time. One of the papers of these years was the first to appear which linked epilepsy to pathological encephalograms. Thus it is safe to say that by the end of the third decade of the century Velikovsky was well within the ranks of European science.

In 1939 he moved to the United States, and in 1940 he incidentally happened to make a discovery which changed the direction of his life. This involved finding a reasonable but surprising reinterpretation of a certain Egyptian document and thereby shedding a new light on the nature and date of the Biblical Exodus. This, in turn, led to other working hypotheses which he intensively investigated for some ten years. Finally, the fruits of his study came to the public attention in 1950 with the publication of Worlds in Collision. Two years later he released Ages in Chaos, which is supplementary to, but not dependent on, his conclusions in the first volume.²

When Worlds in Collision appeared in 1950 it set off a storm of protest by scientists, even to the extent of inciting an attempt to have the book suppressed. This ignoble effort nearly succeeded when the furor forced the Macmillan Company to terminate sales, but publication continued because the Doubleday Company agreed to take up the contract. During the turmoil, "hundreds, perhaps thousands" of letters came into Macmillan's offices; science professors threatened to boycott all of its textbooks; after the ax had fallen, the editor called in the members of his staff and instructed them to answer inquiries with the reply, "We know nothing!"³ The pertinence of the affair lies not in the degree of attempted oppression, but in what it reveals. Apparently the book challenged too many of those principles commonly believed to be necessary, though lacking in direct proof, thus it provoked the kind of reaction to be expected when the perceived needs of a community take precedence over the purposes of the community. During

the past quarter of a century many radical theories have been offered to science, most of them ignored, but none so far as I am aware evoking an attempt to suppress a book already published.

III

It was not unexpected that the ideas in the volume should prove startling. The catalog of events it chronicles from historical records of ancient peoples around the world has not been extensively collated. The evidence pointed to an unwelcome conclusion: the Earth has experienced a series of cataclysmic crises that occurred in pre-human, pre-historic and even historical times. Each of these had enormous effects on the Earth and its inhabitants, directing the course of geological and biological evolution. Although he thinks he has delineated a number of separate episodes going back as far as the memory of man is retained in human records, the book Worlds in Collision pertains only to those since the fifteenth century, B.C. The story of the earlier events is reserved for volumes now in typescript.

Additionally, Velikovsky believes he has re-discovered the cause of these events. In each instance the Earth encountered objects of large mass, sometimes called filaments, comets, or proto-planets: hence, the title, Worlds in Collision. Passing bodies collided with the Earth not by actually striking it but by approaching close enough to disrupt its rotation, to cause changes in the direction of poles, tectonic movements of its crust, large-scale glaciation, interplanetary discharges of electricity, re-location of its orbit, and many other substantial results.

Of such repeated, though not periodic, occurrences, it is the last two series beginning around 1450 B.C. that Worlds in Collision has recounted in detail. It appears that the Exodus of the people of Israel from Egypt took place at approximately that time. Velikovsky discovered that Biblical plagues recorded in the Book of Exodus are echoed in Egyptian sources which independently recount the same natural and narrative events, such as the story of a Pharaoh who was killed and the mention of a place name identical to the Biblical account.⁴ The cause of the plagues was the approach of a comet or proto-planet having somewhat less than the mass of the Earth. This comet, born much earlier by fission from Jupiter, later became the planet Venus. As a comet, Venus had a long tail, and the progression of its gasses and dust onto the Earth precipitated the drama of that time. The encounter likewise produced eruptions of volcanoes around the world, hurricanes, tidal waves, the rising and falling of land masses, and a whole generation of darkness referred to as the Shadow of Death or the Twilight of the Gods (Gotterdamerung). Hydrocarbons poured down from the tail, thus contributing to the world's supply of petroleum, especially the oil found in rocks.⁵ Chemical and bacterial action converted some of the hydrocarbons into carbohydrates, being called by the Hebrews "manna" and by the Greeks "ambrosia."⁶

It should be noted in passing that some readers fail to understand that Velikovsky has documented the existence of comets of

larger mass than are presently seen. In any case the frequent quibbling over his use of the word "comet" has been shown to be unjustified from several perspectives.⁷ In short, an object with a tail and with an irregular non-planetary pattern of movement can hardly be denied cometary status. The origin of comets was within the solar system and was caused initially by collision of the major planets; those with hydrocarbons in their spectra in the first instance coming from Jupiter, and at a later time from Venus; those with water coming from Saturn.⁸ The recorded decrease in number, the loss of luminosity, and the dissipation of material in comets argue for a short life-time.⁹

Fifty two years after the Exodus the Hebrews had arrived in Palestine. In the Bible there is the famous story that while Joshua engaged in a battle the sun and moon stood still for about a whole day. Velikovsky discovered that a similar account of physical phenomena, although variously told, is found in the myths and legends of peoples all over the world. For example, in Mexico a prolonged night is recorded.¹⁰ By comparing different accounts of the same natural events it is possible to cross-check one tradition against another: one story may contain details which provide pieces of the puzzle omitted by other legends and other details which verify the information received elsewhere. In this instance the return of Venus to the vicinity of the Earth caused either a slowing of its rotation, a tilting of its axis, or a combination of these actions.

The comet Venus continued on its decreasingly elliptical orbit around the sun for centuries; in keeping with the known laws of physics, it returned to the general vicinity of the Earth every fifty two years.¹¹ Finally, in the eighth and seventh centuries B.C., there was a climactic series of planetary collisions, this time involving Mars and Venus as well as the Earth. Venus pushed Mars toward the Earth, endangering it until a final episode occurred on March 23, 687 B.C. At that time the poles of the Earth were again shifted slightly and interplanetary discharges took place between the Earth and Mars and between the moon and Mars: the process nearly destroyed the army of the Assyrian King Sennacherib, which was besieging Jerusalem. Many peoples witnessed these battles of the worlds, but the most famous literary reflection is found in Homer's Iliad and Odyssey.¹²

Through these events the comet Venus lost its tail (some of it on Mars), found its way to its nearly circular orbit around the sun, while Mars and the Earth took up their present orbits.¹³ This means that the existing order of the solar system dates back only to the seventh century B.C. By contrast, as presently accepted under the nebular condensation hypothesis of planetary formation, the planets have been in their places with bright shining faces for more than four billion years.

Perhaps some are tempted to think that Velikovsky's reconstruction is overdramatic, but the strangeness dissipates as he pursues the trail of evidence. Ancient records indicate there was

a time when the sun rose in the west and set in the east,¹⁴ when the length of the day, the month, and the year were different from what they are today. For example, the year consisted of 360 days between the fifteenth and eighth centuries B.C., changing to the present system of 365 1/4 days at the beginning of the seventh century B.C. There are shadow clocks and water clocks from ancient times which do not keep time the way we observe it today.¹⁵ The astronomical records of Babylonia, the Venus Tablets of Ammizaduga, which are neither myths nor legends but precise scientific observations, portray a solar system which did not operate the way it does today.¹⁶ Furthermore, there exist numerous rocks magnetically polarized to a much greater intensity and in an opposite direction from the way they should be in light of the magnetic field of the Earth as it is today. While it is generally assumed that reversals of the terrestrial magnetic field occurred only during geological time, Velikovsky points to magnetic dip studies on pottery which show a reversal in the eighth century B.C.¹⁷

Making these points puts Velikovsky in the camp of those who are called catastrophists, those who believe that some kind of violent forces on a large scale were the principal agents in shaping the Earth. Though presently thought to be discredited, catastrophism has considerable foundation in the development of scientific thought. Indeed, this interpretation of the Earth's record was considered respectable within geology until about a century ago, when the views of Charles Lyell and Charles Darwin became victorious over it in the context of an intellectual war waged with religious and political overtones.¹⁸ The earlier geologists of catastrophist views were unscientific when they mixed together geologic evidence and Biblical testimony without recognizing, as Velikovsky does in the construction of his argument, the sharply different nature of the two types of proof.

What replaced this view has become the dogma of uniformitarianism which is the belief that evolution of the Earth and other members of the solar system came about through forces that are in action today. That is to say, the present is the key to the past. In other words, one reconstructs what happened in the past by observing what is occurring now. Besides being an example of wishful thinking, this method only claims to be able to say what could have happened, and this at best presents only a hypothesized past. Whether or not this approach yields the actual past has never been verified, but it is staunchly assumed. Lacking direct proof of the past, the method is perfectly acceptable, indeed, necessary. However, historical evidence should be regarded as more persuasive than the most laborious hypothetical models constructed in the absence of it.

Yet there were at least two reasons for the rejection of catastrophism a century ago, both of them understandable at the time, but neither pertinent to Velikovsky's formulation of the theory. First, it was painfully obvious to skeptics in the nineteenth century that many geologists embraced the doctrine out of a religious motive, the desire to prove that the Bible is correct when it pre-

sents a catastrophist picture of the world's history. Apparently those religious geologists were not sufficiently aware that the Bible is not unique in presenting such interpretations of the Earth's history. Nevertheless, the firm linkage of this particular theory with an obscurantist fundamentalism discredited the point of view. In reaction, the successors to the early geologists properly sought to place geology and biology onto objective foundations not dependent on Biblical revelation. In order to do this, catastrophism, seen as an enemy of the autonomy of science, had to be eliminated and its merits denied. Consequently, the one unscientific extreme bred its unscientific counterpart. And so today any explanation entailing catastrophes is likely to be offensive to conventional science unless the suggested events occurred very long ago, or very far away, or were not very extensive.

The second reason was even more decisive. Convinced as they were that nature had experienced sudden and violent changes, these nineteenth century geologists could not suggest sufficient causes to have brought about destruction on the enormous scale which the evidence indicated.¹⁹ No scientific theory is viable unless it can produce an adequate cause for its claimed effects. Unfortunately, for the reason just stated, geologists abandoned the search for the cause.

It is not fair to lump Velikovsky together with the precontemporary catastrophists because in both areas his approach differs. He has no ax to grind in favor of the accuracy of the Bible any more than that of the Hindu Vedas, the Icelandic Edda, or any other religious record. He approaches all such materials in the spirit of historical analysis. His attitude is naturalistic in operating assumptions and methods and perhaps his system should be called "secular catastrophism."

Far from religion playing a motivating role, quite the opposite is the case. His studies have actually provided a naturalistic explanation for religion itself, which traces the origin of religious belief to planet worship that evolved as the "gods" became anthropomorphized and then spiritualized, and also for the origin and persistence of beliefs in astrology and other forms of superstition.²⁰ Modern science fights defensively against superstition, and decries its adherence by the masses,²¹ but up to now a satisfactory explanation of its perennial appeal has been elusive. Superstition is likely to persist until its root causes in natural history are recognized.

Another step in the presentation of Velikovsky's thesis occurred in 1955 when he published Earth in Upheaval which restricted itself to the geological evidence, or as he calls it, "the record of stone and bone." This was to demonstrate that his conclusions did not rest solely on the written record. The book tackled some of the persisting enigmas of nature. For example, the mammoths of Siberia which were encased in ice with undigested food in their stomachs and between their teeth of a kind not presently found there must have been quickly frozen or their carcasses would have rotted before preservation.²² The presence of corals in polar

regions demonstrates that the Earth's poles did not always point where they do now.²³ An explanation, however, is available. A sudden shifting of the Earth's axis moving regions of warm climate into the polar circle solves these and other problems.²⁴ For those who find the concept hard to accept, Velikovsky suggests that they try to cultivate coral at the North Pole.²⁵

Another approach to the resolution of geological contradictions is the theory of continental drift which not long ago was itself quite controversial, but for some reason seems now to be highly seductive. This hypothesis appeared earlier in the century as it was originated by Alfred Wegener, but in the form in which it is usually discussed currently, it is called "plate tectonics." Under this view the continents rest on mobile plates; over geological time they once drifted together, and later they drifted away again. One difficulty with this scenario is that no one still has explained satisfactorily where the enormous force needed to separate the continents comes from, especially, since that force has to act constantly over billions of years.²⁶ In Velikovsky's view nothing has occurred in the development of plate tectonic theory during the past two decades to warrant a conclusion that its flaws have been fully corrected. It is conceded that there were movements of the continents, but these have derived from displacement of strata by inertia and have progressively decreased with time. The present shape of continents cannot be taken as a fixed guide in reconstructing earth history through uniformitarian processes. Velikovsky sees his position as "intermediary" between the enthusiastic opponents and supporters of the drift idea.²⁷

Shifts in the Earth's axis, accompanied by the phenomena associated with them, turn out to be better solutions to geological enigmas from still a different standpoint: they resolve inconsistencies in the theory of ice ages. Velikovsky rejects the concept that gradual fluctuations of climate between cold and warm periods can account for the advance and retreat of glaciers. Advance of glaciers during a cold period poses the incompatible requirement of enough heat to evaporate the oceans sufficiently to drop their level hundreds of feet in order to supply the needed quantity of precipitation. Any degree of coolness alone is inadequate. Though the oceans would freeze, extensive glaciation would not happen even if all heat from the sun were removed. On the other hand, the succession of enormous heat from the effects of the axis shift, causing the oceans to boil, quickly followed by cooling and condensation as darkness enshrouded the Earth, best explains the glaciation that occurred. Moreover, it should be seen that some of the effects attributed to glaciation were actually caused by tidal waves.²⁸

Global catastrophes had immense results on animal and plant life as well. According to Velikovsky, biological evolution did not occur by gradual changes guided by natural selection and accumulating into the origin of new species, as Darwin thought. No new animal species are in the process of formation today, as might be expected under the doctrine of uniformitarianism, an exception which should bother the adherents of slow evolution. The appearance of new species actually occurred suddenly, caused by the simultaneous

presence of conditions which would be produced during global upheavals; chemical, thermal, radioactive, and possible acoustical agents working together in various combinations could have induced mutations in living species on an immense scale. Most of the mutations thus caused would be harmful or non-reproducible, but some would not be. Likewise, the same actions could also produce re-arrangements of chromosomal patterns. Thereafter, natural selection did serve to eliminate unfit species and ensure the best adaptation of the fittest to a new environment. This explanation solves the multiple mystery of how some new species suddenly came into being, how some survived as they were (the effects of global catastrophes would differ locally), and how some were entirely eliminated.²⁹

Whether the collision of planetary bodies could have produced evolution depends, of course, on whether it is physically possible for such bodies to collide in the manner described. The first impression of nearly every physicist is that this is precluded by the known laws of physics.³⁰ Velikovsky maintains, however, that non-standard orbits are possibly acceptable within the framework of conventional celestial mechanics, and hence, present no insuperable difficulties. True, there are still unsolved problems, but these are to be found several layers beneath the surface.³¹

As we probe deeper it seems that most opposition among skeptics of the idea that planets could come close together lies in several assumptions of classical Newtonian physics which themselves deserve unrendered skepticism. Under this accepted system the motions of all heavenly bodies are governed only by two factors--universal gravitation and inertia, with the possible addition of the pressure of light acting on cometary tails, a very small input in any case. As Einstein emphasized in correspondence with Velikovsky, additional forces are believed to be unnecessary and would be an embarrassment since even without them one can calculate the motions of celestial bodies with precision.

Yet, Velikovsky maintains, two other forces play an important role--electricity and magnetism, noting that electromagnetism in certain circumstances obeys the inverse square law as does gravitation. Hence, when planets are separated on non-intersecting orbits, an effect attributable to electromagnetism may be hard to distinguish from gravitation: a case of "charge...masked by the mass." But there are other situations in the solar system that are not explainable by the purely gravitational model but which become so when allowance is made for an electromagnetic role. One suggestion along these lines might be to assess the seemingly disproportionate gravitational influence on the orbits of Neptune and Uranus exerted by Pluto in the light of a possible substantial charge on Pluto.³²

Another place to look for such a role in the celestial mechanism might be at the pattern of cometary movement. Rather than the pressure of light or the solar wind, electromagnetic interactions cause the peculiar behavior of cometary tails as they whip around the sun with rod-like rigidity. Neither the pressure of light nor the solar wind is capable of performing this operation as it has been

observed.

Frequently forgotten also is that regardless of the degree of electromagnetic force in operation when celestial bodies are removed by some distance, the situation is changed when they come into close proximity. Under these conditions it is hazardous to predict the precise strength or manner of operation that would characterize electrical and magnetic fields. However, one cannot assume the same impotence that these fields are presumed to have with the planets in their present orbits.³³

Additional support for the view that electromagnetism acts in the cosmos on a scale befitting its extent comes from the work of a French astronomer. Anton Danjon, who discovered in 1960 that the Earth's rotation period suddenly increased by small fractions of a second after a strong solar flare, thereafter decreasing. Danjon ascribed the effect to electromagnetic action.³⁴ More recently, two investigators used his findings to predict a similar result, which they detected, but subsequently their data was disputed by others.³⁵ While the point is not conclusive, it is crucial; the effect may be small, but so is the cause. If merely a solar flare can move the Earth, how can one say that electromagnetism plays no role at all in the motion of the planets? On the other hand, if it does so, then Newtonian physics, which assumed that celestial bodies are electrically and magnetically sterile, is shown to be based on a false premise and will have to be reexamined.³⁶

The present non-recognition of the role played by electromagnetism amounts to a contention that one can move magnets (the planets) through a magnetic field (centered on the sun) with no impact on the magnets themselves. This strains credence, especially in the case of Jupiter and its satellites.³⁷ Velikovsky believes that the sun and the planets possess differing net electrical charges: the proposition only awaits open-minded investigation and is already being theoretically explored.³⁸ One consequence of charged planets operating within a charged galaxy³⁹ is that the self-starting dynamo theory of planetary magnetic fields loses any remaining plausibility. At hand is an opportunity to construct a realistic theory of planetary magnetism on the broader canvas of the solar system, bringing together and properly relating planetary charge, rotation and revolution to the sun's charge and the interplanetary magnetic field.

A challenge to some tenets in modern physics is the end-product of a search that began in an historical investigation. And yet the same Egyptian document which pointed Velikovsky down that road also necessitated a re-evaluation of accepted chronology of the entire ancient Near East prior to the Third Century B.C. It turns out that the national histories of the area are out of synchronism at many points; while some events are placed in their proper time and sequence in some of the nations, many are not--the major source of the distortion being in Egyptian chronology. This revision has not been fully released except in summary form, but three detailed volumes, out of a projected series of five, have

appeared: Ages in Chaos, which recounts the beginning point of the initial synchronism (the Exodus in 1450 B.C. at the end of the Egyptian Middle Kingdom), came out in 1952; Peoples of the Sea, the book covering the end of the revision, arrived in 1977; and Ramses II and His Time, released in 1978.

Whereas Velikovsky's other volumes have engendered extensive discussion over a period of time, these works, even the earlier one, have been met with comparative silence by specialists in the field, except for occasional adverse comment about scattered points over matters where differing interpretations are possible by any standard. Such hesitation is peculiar since the thesis here is constructed quite independently from the arguments put forth in Worlds in Collision and Earth in Upheaval. Though both studies started from the same investigation, conceivably Velikovsky could be entirely wrong about cosmology without affecting his chronology at all. On the other hand, a portion of the evidence used to buttress his cosmology depends on the correctness of the chronological reconstruction. Hence, the volumes have been delayed so that they could be all the more carefully completed, drawing from the widest variety of sources, in order to spell out precisely how the sequence of events in the Near East must be re-aligned. Unfortunately, too many specialists, unable to come to terms with the evidence already presented, comfort themselves with the thought that nothing has been proved until everything has been proved. That is a standard they would never accept for themselves. However, if the books published thus far cannot be successfully attacked, is it reasonable to assume that questions about remaining problems are unanswerable? Up to now students have been handicapped by a lack of coherent criticism of the three cornerstones already in place.

Ages in Chaos starts with the last king of the Middle Kingdom, an obscure Pharaoh whom the Greeks called Tutimaous (Tom-Taoui-Toth). Following this came the rule of foreign invaders known as Hyksos, limited by conventional history to around 100 years. Velikovsky, however, sees them as holding sway over Egypt for over 440 years.⁴⁰ The upshot is that the New Kingdom did not begin until the time of King Saul of Israel. This re-arrangement makes for some interesting consequences. For example, the famous visit to King Solomon of the Queen of Sheba was the same journey, as found in Egyptian records, of Queen Hatshepsut who went to a land called "Punt." At a later time, according to the Bible, the Egyptians removed the furnishings from Solomon's temple in Jerusalem, and these items as described in the Bible are shown to be remarkably similar in detail to the spoils as illustrated on the walls of the temple of Karnak in the annals of Pharaoh Thutmose III. Furthermore, Velikovsky is able to mesh Egyptian records and Palestinian accounts of later dates with the same success.

Marshalling equally persuasive evidence, Velikovsky turns in Peoples of the Sea to an 800 year divergence between the accepted scheme and his revision. Supposedly the twelfth century, B.C. saw Pharaoh Ramses III battle the "sea peoples," barbarians from Greece and the "Pereset," the Philistines from Palestine; instead, the proper time was the fourth century, B.C., Ramses III was Pharaoh Nectanebo I, the "sea peoples" he fought were Greek

mercenaries under Persian service, and the "Pereset" were the Persians. Not only does the extant record of fourth century B.C. Greek, Persian, and Egyptian sources correspond quite well to information in the twelfth century B.C. annals of Ramses III, but unmistakable physical evidence also conforms: the palace of Ramses III contained tiles somehow bearing Greek letters in the style of the fourth century, B.C., despite having been carved into the tiles before they were fired.⁴² Likewise, the headgear of the "Pereset" are shown to be the same as that of the fourth century, B.C. Persians by comparing the bas reliefs in the mortuary temple of Ramses III to those located on the palace of Darius at Perseopolis.⁴³ Written and visual evidence combine, transporting historical personalities across the centuries to resting places where they seem much more at home. For example, Si-Amon, reputed to be the father-in-law of King Solomon, becomes merely a priest-king in the desert playing a subordinate role to the Ptolemaic kings.⁴⁴

In the latest book Velikovsky uncovers one of the reasons for the bogus time by which Egyptian chronology has been unduly extended: the Nineteenth and Twenty-sixth Dynasties are comprised of the same men who appear twice centuries apart under different names. For example, the famous Ramses II, who actually lived seven centuries later than presently thought, is the same as Pharaoh Necho II of the Twenty-sixth Dynasty. Ramses II, then, was a contemporary of Nebuchadnezzar, indeed fought against him in the battle of Kadesh, which is the same in Chaldean history as the battle of Carchemish. Subsequently Ramses II signed a peace treaty with Nebuchadnezzar and married his daughter, an event memorialized in Egypt at Abu-Simbel. Additionally, Nebuchadnezzar is found to have left a revealing autobiography once it is realized that the records of the so-called "Hittite" Empire really belong to the Chaldean period of Babylonian history. The successor to Ramses II, Pharaoh Merneptah, frequently thought to be the Pharaoh of the Exodus, is shown instead to be the Pharaoh of the Exile.⁴⁵

As with all serious studies of the past, Velikovsky's historical reconstructions have impelling implications for the present, and also for the future. Tying the three together, Velikovsky offers a theory of collective amnesia, explaining why so much of the past has been forgotten, why the condition of man today is so baffling, and what constitutes the central hazard for the species in the future.

Building on foundations laid by Carl Jung and Sigmund Freud, he takes their work, revises it, and carries it one step further. It appears that the most terrifying events of history have been deliberately repressed. The global catastrophes of the past decimated the world's population and produced a severe psychological shock in the survivors. It is well known that victims of a trauma sometimes develop amnesia concerning the content of the trauma. In the case of the ancients, a compulsion to communicate their experiences competed with their need and that of their survivors to forget about them. The dilemma was solved by the construction of cosmological myths which served to mediate and transform horrible memories. Forgetfulness at the level of consciousness

won out. Acting from the same necessity, later generations interpreted these myths allegorically, so that their plain meaning was not perceived. If the reality of the trauma can be denied, it was thought sub-consciously, perhaps its return can be prevented.

Nevertheless, as Freud believed, a victim of a traumatic experience tends to have an impulse to repeat the trauma symbolically and in action, reasoning to himself that if he can do so, the next time he will triumph and put the nightmare behind him. In Freud's view, the human race in general exemplified the typical behavior of a victim of amnesia; hence, the compulsion to repeat is a source of the neurotic tendency in man which results in aggression in the individual and war in society. But what could have caused so great a trauma? In trying to answer this, Freud invented the "Oedipus Complex," the ancestral urge of the son to possess his mother and dispossess his father. Regardless of the validity of this, Velikovsky himself sees the "Oedipus Complex" as unnecessary for explaining the amnesia, believing as he does that natural history was the source of the trauma. Indeed, perhaps anxiety is passed on phylogenetically because it comes not from one single ancestor, but from many ancestors who experienced the same events.

The real danger for the future lies not so much in new cosmic catastrophes, something remote despite several remaining orbital intersections within the solar system,⁴⁶ but in the possibility that man's neurotic state may overtake him. In an atomic age when men have acquired the ability to melt mountains, humanity now stands in a race between self-discovery and self-annihilation.⁴⁷

IV

Having glimpsed but a few points of Velikovsky's reformulations, it is appropriate to return to the questions offered at the beginning. First, is his thesis coherent? Although requiring a re-interpretation of accepted ideas at many points, there is no place I can see where he has entrapped himself into a contradiction, given the premises of his theory. If one can maintain the internal consistency of a theoretical structure cutting across many disciplines, this is a substantial achievement. A faulty hypothesis can be expected to exhibit hidden assumptions at odds with its conclusions. The absence of such flaws by no means establishes the proposal, but checking internal consistency is an economical starting place. Of course, unsolved problems abound for the most acceptable thesis, the existence of which stand as a bar to acceptance; but unless the problem is insurmountable within the terms of its own argument, the question remains open. This approach to new ideas is preferable to the normal tendency to reject a proposition out of the subjective reaction that it sounds strange or because it undermines certain unexamined assumptions.

One excuse frequently offered by specialists in some disciplines for refusing to give consideration to a bold hypothesis is that they

can conceive of less radical solutions to the difficulties it resolves; Occam's razor should be used, it is argued, to eliminate more drastic alterations of theory when less drastic ones are available. Under this parochial application the very boldness of the hypothesis is the reason it is assigned a low probability. Of course, every question in scholarship yields more than one answer. But the proper task is to find the correct one, not the one most consistent with previous thinking.

In Velikovsky's case, there is hardly a single spot in the reconstructions where an alternate view is not possible in terms of the standard approach; by the same token, critics have failed to show where his interpretation at any crucial point is a mistaken or an unreasonable rendering of the evidence. As a rule, critiques of the scientific aspects have rested on uniformitarian assumptions, so that the negative appraisals amount to an exercise in circular reasoning. There is yet to appear a refutation that does not fundamentally beg the question, that comes to grips with the argument as a whole and under its own terms. From the vantage of particular disciplines the postulation of worldwide catastrophes may seem to violate the rule of parsimony, but one should consider how the entire hypothesis provides a simple solution to many problems crossing disciplinary boundaries, an accomplishment which applies that rule in a wider context. Additionally, besides begging the question, much criticism of Velikovsky's proposals has amounted to an argumentum ad horrendum, the invalid argument that something cannot be true because of the horrible consequences that would follow.

The second test involves whether a prima facie case has been presented. Do the proposed facts, if later found to be true, sufficiently support the proposed revision? If the evidence offered is inadequate on its face to sustain the theoretical structure, then no compelling case for serious study has been made. On the other hand, if the facts would be convincing, if later found to be true, then the argument deserves respectful attention immediately. It is reasonable at that stage to ask skeptics to provide rebuttals, if they can, to the strongest points, as well as exercising their prerogative of exposing the weakest.

Because Velikovsky has amassed great quantities of consistent evidence, this alone makes his theory worthy of serious study. As always, the burden of proof lies with the proponent of a new idea. Yet there is a point after the burden has been met where the lack of a convincing rebuttal increases the viability of the hypothesis.

It is obvious, of course, that a vast amount of theoretical development remains to be done before many implications of the reconstructions are fully clarified. It is not realistic, though, to expect that one person in a few pioneering volumes can provide answers to all legitimate questions raised by what is nothing less than a new synthesis of knowledge. Critics seem to require that every last detail be laid out before they come to grips with the already detailed exposition found in the books published to date.

Such a heads-in-the-sand attitude shows an inadequate regard for the truth, or perhaps a fear of finding it. The amount of work completed by Velikovsky and other scholars following in his path justifies placing the reconstructions under the spotlight of sympathetic scrutiny.

Not only is a full legitimization justified, the tenets of scholarship make it imperative. Accordingly, even if a scholar rejects a theory, it is supposed to be re-evaluated upon the presentation of fresh support. Velikovsky's writings have anticipated numerous facts authenticated later by discoveries. A few examples pertaining just to the solar system are these: the heat of Venus, radio noises from Jupiter, the magnetosphere of the Earth⁴⁸ (which gives rise to the Van Allen belts), and recently, the steep thermal gradient of the moon's soil and the remanent magnetism of its rocks.⁴⁹ Frequently, even one correct anticipation in science endows a theory with standing; the occurrence of multiple successful predictions at least should counteract a presumption that the theory is incorrect because it is unorthodox. Several further anticipations yet to be fully confirmed are hydrocarbons in the clouds of Venus, a large quantity of argon and neon in the atmosphere of Mars, and the emission of low energy cosmic rays or X-rays from Saturn.⁵⁰ What impression will it make on those who have ignored the record to date, if these also achieve success?

Thomas S. Kuhn has revived the old argument that scientific progress does not proceed by the accretion of small steps built on the foundation of previous certitude, but rather by a process involving first the domination of a particular "paradigm" or set of governing principles (i.e., catastrophism, uniformitarianism), next its collapse under the weight of too many anomalies, and finally a shift to a new paradigm.⁵¹ This analysis of scientific advancement in the past has a degree of usefulness.

But I wonder why it is necessary to waste so much time in the present serving the dictatorship of a single paradigm. Were the prevailing principle of science today catastrophism and its heretical contender uniformitarianism, instead of the other way around, I would be just as opposed to having a dogma enshrined as I am in the present situation. Are our minds so weak that they cannot permit the co-existence of competing basic principles? Truth is the end-product of a search and cannot properly be presumed to be known at its beginning. Conservatism is just as likely to block progress in science as it is in other fields.

One gathers that an important reason for the resistance to new hypotheses like Velikovsky's that require major revisions of theoretical structures lies in the vested interests and ego involvement of those who have devoted years of study under the guidance of the accepted assumptions or who have committed themselves in print. How many authors are willing to retract their publications until forced to do so, no matter how convincing the arguments are? The problem is multiplied in the case of an interdisciplinary thesis. Velikovsky's very boldness in re-ordering

the foundations of received knowledge in reality constitutes the essence of his heresy, especially since a new vision of temporal relationships is now required, just as in the past the Copernican revolution demanded a new comprehension of spatial relationships.

Continuing Pavlovian reactions involves still another mistake. The truth or falsity of a proposition is not the only relevant concern regarding its value to science. Even should a theory prove to be false ultimately, it can still serve usefully as a guiding principle in the process of investigation. In the past many experiments performed on the basis of false assumptions have yielded information which has advanced knowledge. Whether Velikovskian catastrophism is correct or not, it ought to be tested where possible. Being specific, why continue to ignore theoretical possibilities suggested by Velikovsky over two decades ago which point to even more significant results? --"The origin of new species from old could be caused by the processes that can be duplicated in laboratories--by excessive radiation or some other irritant in abnormal doses, thermal or chemical...."⁵² Indeed, use of widely differing hypotheses to discover which best illuminates the data is supposed to be basic to the scientific method. How ironic that so many humane people, proud of their liberalism in political and social matters, should vehemently defend the belief that science cannot function unless it intolerantly rejects departures from past belief.

If real progress in the condition of the human race is to be made, then the value of truth must be given priority over all other considerations. At least that experiment ought to be tried before adopting pessimistic conclusions on the possibility of genuine progress. This means that scholars must return to the fundamental tenets of scholarship, and cease making Velikovsky an exception to the rule. The first duty of a scholar is to understand, and only then to pass judgment. Whenever the latter precedes the former, the latter usually precludes the former. His critics have rendered an intuitive judgment before they have grasped what they were criticizing: they think they know the answer to it before they know anything about it.⁵³ Actually, neither intuitive acceptance nor intuitive rejection has any more validity than the conclusions drawn from the data provided by Robert McNamara's Pentagon computers which assured him that the Communist national movement in Vietnam was on the verge of extinction. Those who beg the question about Velikovsky are depending upon luck rather than analysis for the vindication of their position.

We must have the courage to face the past or we will not have the means to confront the future. Indeed, it is opportune to appreciate this interdisciplinary synthesis which explains so much and provides a unified perspective to the Earth's and man's history.

Footnotes

¹The works by Velikovsky cited are: Worlds in Collision, 1950; Earth in Upheaval, 1955; Ages in Chaos, 1952; Peoples of the Sea, 1977; Ramses II and His Time, 1978. Velikovsky has published an additional work not mentioned here, Oedipus and Akhnaton which maintains that the Pharaoh Akhnaton was the historical prototype of the Oedipus legend. All of these books are still in print and obtainable from the publisher, Doubleday and Company, Garden City, New York.

An indispensable tool for the students of the reception of Velikovsky's ideas is a collection of articles edited by Alfred de Grazia and Ralph Juergens, The Velikovsky Affair. Likewise, for an excellent survey of the entire field of Velikovskian studies including the most recent developments, consult C. J. Ransom, The Age of Velikovsky, Fort Worth, Texas 76117 (5413 Stephanie Drive): LAR Research and Publishing Co., 1977.

Hereafter these works are referred to respectively as WC, EU, AC, POS, VA, and AV.

The Student Academic Freedom Forum, Box 414, Portland, Oregon 97207 dedicated ten issues of its journal Pensee to a series titled, "Immanuel Velikovsky Reconsidered" before the demise of the journal. However, most of the issues in this series are still obtainable from the above source. Some articles in the series were reprinted in Velikovsky Reconsidered, 1976 put out by Doubleday. Citations from Pensee are by issue number.

The work of continuing the scholarly exploration of Velikovskian themes has been taken up in the United States by Kronos. This journal can be obtained by writing Kronos, Glassboro State College, Glassboro, New Jersey 08028. In Great Britain a society has been formed for the same purpose, the Society for Interdisciplinary Studies which issues the S.I.S. Review. For information contact R.M. Amelan, 6 Jersey House, Cotton Lane, Manchester 28.

²VA, 11-16.

³VA, 23-32.; Harold S. Latham, My Life in Publishing (New York: E.P. Dutton & Co., 1965), 74-76.

⁴AC, 12-47.

⁵WC, 57.

⁶The chemical processes involved are discussed by Wong Kee Kuong, "The Synthesis of Manna," Pensee, III, 45-46.; M.G. Reade, "Manna as a Confection," S.I.S. Review, Vol. I, Nr. 2, 9-13,25.

⁷AV, 73-84.

⁸Pensee, VIII, 41.

⁹EU, 152-53.

¹⁰Joshua 10:12-13; WC, 44-46.

¹¹WC, 153-54.

¹²WC, 227-60.

¹³WC, 362-67, 384.

¹⁴WC, 105-14.

¹⁵WC, 330-48, 320-24.

¹⁶WC, 198-200; Lynn E. Rose, "Babylonian Observations of Venus," Pensee, III, 18-22. Also see Lynn E. Rose and Raymond C. Vaughan, "Analysis of the Babylonian Observations of Venus," Kronos, Vol. 2, Nr. 2, 3-26.

¹⁷EU, 143-47; An Address Before the Graduate College Forum of Princeton University on October 14, 1953, EU, 283.

¹⁸Some political implications of the controversy are related in George Grinnell, "The Origins of Modern Geological Theory," Kronos, Vol. 1, Nr. 4, 68-76.

¹⁹EU, 234-35.

²⁰WC, 382, 65-66.

²¹"Objections to Astrology: A Statement by 186 Leading Scientists," The Humanist, September-October, 1976.

²²EU, 3-7.

²³EU, 44-46.

²⁴EU, 136-39.

²⁵EU, 46.

²⁶EU, 120-22.

²⁷"Author's Note," Pocket Book edition (1977) of Earth in Upheaval, p. xxii-xxiii.

A useful collation of difficulties suggested in the scientific literature at various times with regard to the drift thesis is found in Paul S. Wesson, "Objections to Continental Drift and Plate Tectonics," Journal of Geology, Vol. 80, 1972, 185-97.

²⁸EU, 130-43.

²⁹EU, Chapters XIV and XV. The mutagenic effect of sound waves was discussed by the author with Frederic B. Jueneman in 1972.

Jueneman suggested their possible mutagenic effect and I mentioned their likely ability to re-arrange chromosomal patterns efficiently. Subsequently, Jueneman passed on the idea in a letter to Velikovsky published in Pensee, II, 49-50.

³⁰Robert W. Bass, "'Proofs' of the Stability of the Solar System," Kronos, Vol. 2, Nr. 2, 27-45.

³¹WC, 384-87. Robert W. Bass has drawn attention to another potential mechanism: see "Can Worlds Collide?" Kronos, Vol. 1, Nr. 3, 59-71. Also see two other useful discussions: Lynn E. Rose and Raymond C. Vaughan, "Velikovsky and the Sequence of Planetary Orbits," Pensee, VIII, 27-34; Earl R. Milton, "As Worlds Collide," Kronos, Vol. II, Nr. 3, 3-7.

³²Velikovsky, "On the Advance Claim of Jupiter's Radionoisies," Kronos, Vol.3, Nr. 1, 27.

³³C. J. Ransom provides an important discussion of this issue, which is admittedly far from resolution. Ransom cautions against overestimating the role of electromagnetism even under the conditions discussed here. See AV, 101-04.

³⁴Velikovsky, "Additional Examples of Correct Prognosis," VA, 234.

³⁵John Gribbin and Stephen Plagemann, "Discontinuous Change in Earth's Spin Rate following Great Solar Storm of August 1972," Nature, Vol. 243 (1973), 26-27. It should be noted in passing that Velikovsky does not necessarily endorse the catastrophistic conclusions of the same authors in their work which has received popular notoriety, The Jupiter Effect.

For the contrary view on the spin rate see N.P.J. O'Hora and C.J.A. Penny, "Rotation of the Earth during the 1972 Solar Event," Nature, Vol. 244 (1973), 426-27.

³⁶Princeton Forum Address, EU, 297; WC, 387.

³⁷Velikovsky, "My Challenge to Conventional Views in Science," Pensee, VII, 13.

³⁸Pioneering work in this field is being done by Ralph Juergens. Two of his articles in particular should be consulted: "Reconciling Celestial Mechanics and Velikovskian Catastrophism," Pensee, II, 6-12; "On the Convection of Electric Charge by the Rotating Earth," Kronos, Vol. 2, Nr. 3, 12-30.

³⁹"Changing Views of the Earth and Man's Past," an informal address by Dr. Immanuel Velikovsky at Youngstown State University, April 11, 1973. Copy of transcript in the possession of the author.

⁴⁰AC, 76.

⁴¹POS, 38-39.

42POS, 6-12.

43POS, 32-34.

44POS, 179-88.

45Ramses II and His Time. The entire reconstruction is presented in summary form in "Theses for the Reconstruction of Ancient History," in a monographic series titled, Scripta Academica Hierosolymitana, copyrighted, 1945.

46WC, 372.

47Velikovsky's views on the subject are detailed in an unpublished volume, Mankind in Amnesia.

48Letter by Victor Bargmann and Lloyd Motz, Science, Vol. 138 (1962), 1350-52.

49"A Record of Success," Pensee, I, 11-15.

50WC, 365-69; Memorandum of September 11, 1963 to H.H. Hess, Pensee, II, 27-28.

51Thomas S. Kuhn, The Structure of Scientific Revolutions Chicago: University of Chicago Press, 1962.

52EU, 259. Such experiments would merely seek to duplicate in the laboratory the processes that, under Velikovsky's hypothesis, occurred rapidly in nature. Care should be taken to replicate as nearly as can be done the conditions of the catastrophes, including radioactivity, heat, chemical effects, strong magnetic fields, acoustical waves, and the mimicking of cosmic rays, if practical. The experimentation itself could determine the more promising mixes of intensity and agent.

53A good example of how preconceived ideas can skewer the judgmental processes of science was provided recently when the Cornell University Press paid Velikovsky the supreme compliment of "confronting" him with a volume of essays setting forth the appraisals of certain prominent scientists. In the main the book consisted of four papers previously read at the symposium, "Velikovsky's Challenge to Science" held at the 1974 annual meeting of the American Association for the Advancement of Science.

Each author exhibited several difficulties in constructing a critique that could withstand critical examination. Aside from other considerations, however, a fundamental problem with them all lay in the inability of the authors to understand accurately or very profoundly the thinking they were attempting to refute. Velikovsky can hardly be held responsible for the way his critics misread his words, or fail to read them. The contributors gave ample evidence of having missed the broad outlines of the theory, not just the minor nuances. Needless to say, arguments grounded on misinterpretations

cannot yield a valid evaluation. Yet the papers varied in quality. The essay that seemed most scientific in spirit was the one by David Morrison, the one least so by Carl Sagan, although perhaps it should be allowed in extenuation for the difficulty of being objective about astronomy when one is himself a star.

See Donald Goldsmith (ed.), Scientists Confront Velikovsky, (Ithaca and London: Cornell University Press), 1977. A detailed analysis of this effort is found in a special issue of Kronos, Vol. III, Nr. 2, titled, "Velikovsky and Establishment Science."



COMMENTS ON "THE HERESY OF A NEW SYNTHESIS"

BY DAVID MORRISON:

In the nearly thirty years since the publication of Worlds in Collision, the ideas of Immanuel Velikovsky have been widely discussed and frequently rebutted. The most extensive critical discussion has been published by Cornell University Press under the title Scientists Confront Velikovsky, edited by Donald Goldsmith. In this book, derived from a 1974 AAAS Symposium, several authors criticize in some detail the astronomical and physical basis of Velikovsky's catastrophism. Unfortunately, Dr. May's paper appears oblivious of this responsible, scholarly criticism. His apologia for Velikovsky might as well have been written one or even two decades ago. It is hardly possible in this brief comment to point out the literally hundreds of places where Velikovsky's grand synthesis stumbles on the pedestrian stones of hard reality, but readers who have not looked at the Cornell University Press book are strongly urged to do so before accepting the undocumented claims of Dr. May - or of Dr. Velikovsky.

I would like to point out a few fundamental problems with Dr. May's paper in the space available here. His discussion of catastrophism in the Earth Sciences is largely irrelevant. Velikovsky's followers often see him as a proponent of a catastrophist viewpoint, struggling against a uniformitarian paradigm in contemporary geology. They fail to realize that he is fundamentally opposed to essentially all geology (whether called uniformitarian, catastrophist, or phrased in terms of more contemporary concepts) because of the absurdly short time scales that he would assign to such fundamental geological processes as mountain building, continental motion, and changes in the Earth's rotation and magnetic field. While some of his ideas sound seductive when phrased in qualitative, popular prose, they contradict by many orders of magnitude all we know about geological time scales.

Another irrelevant issue raised by May concerns the concept of Venus as a comet. If he wishes to call the Velikovskian Venus a comet that is fine, but it is inappropriate to draw conclusions about the physical nature or orbital behavior of Venus by analogy with contemporary comets, which are totally different in composition and the very largest of which is ten million times smaller than Venus. And as to May's assertion that comets in general came from Jupiter, Venus, and Saturn, I cannot imagine what evidence supports such an idea.

Everyone admits that today planets do not burst forth from Jupiter and career about the solar system throwing thunderbolts at each other. As Dr. May notes, no unknown forces or arbitrary assumptions are required to account for the motions of planets and space probes today, and indeed celestial mechanics is one of the most exact and quantitative fields of modern technology, as the successes of the space program witness. However, May devotes a substantial discussion to the idea that electromagnetic forces may intervene to

upset the gravitational order of planetary motions. This is an assertion often made by defenders of Velikovsky, but it is pure poppycock. Electromagnetic forces are as well understood as gravitation, and even if one hypothesises absurdly large electric charges and planetary magnetic fields, they cannot begin to generate the effects required by Velikovsky.

I have always believed that the most lucid tests of Velikovsky's ideas can be made from straightforward examination of the facts, rather than from arguing the might-have-beens of unknown forces that he hypothesises as having acted 3000 years ago but not influencing planetary motions today. Rather than debate ad hoc notions of how planetary near-collisions might have taken place, let us see if there is evidence today that such events did, in fact, take place within the past few millenia. Here Velikovsky's record is one of unmitigated failure. Every important prediction he made in 1950 concerning conditions on the planets, such as hydrocarbon clouds on Venus, large amounts of argon in the atmosphere of Mars, recent melting of the lunar surface, large internal heat sources on Venus and perhaps Mars, large-scale recent cratering of Earth and Moon, and synchronized planet-wide volcanism on Earth have been shown decisively to be in error. Every new space mission, such as the recent Pioneer Venus probes, pounds another nail in the coffin. Remarkably, defenders of Velikovsky like Dr. May refuse to acknowledge these facts. But the cruel truth is not only that astronomical evidence fails to support Velikovsky, but that a great deal that seemed plausible or at least possible when suggested in 1950 has since been shown to be incorrect, and indeed to strongly contradict his theories.

The Velikovsky phenomenon is an interesting one, and a discussion of its significance for modern science might be both entertaining and useful. Often fundamental truths are exposed through the study of pathological examples, and the whole Velikovsky affair seems to me to represent a pathological case of the workings of academic science. Unfortunately, Dr. May's article does not contribute to such a discussion. By uncritically repeating all the standard Velikovsky cant and ignoring responsible criticism, he adds little to critical scholarship. What most disturbs me, however, is his apparently profound ignorance of the evidence from contemporary astronomy and geology that relates to Velikovsky's ideas. His citations, for instance, are almost wholly to other pro-Velikovsky literature, written in turn largely by those who are not competent to set themselves up as judges of these unfortunately rather technical fields. I consider it inconsistent with the purposes of The Zetetic Scholar to deal with the interesting subject of the response of organized scholarship to an outside critic such as Velikovsky in such an uncritical and ill-informed manner. And I regret that, once the subject has been raised, it is impossible to present the extensive evidence that confronts Velikovsky. For it is not scholarly prejudice or inertia that contradict Velikovsky (although both may indeed be present), but the natural record itself upon which his hypothesis of recent planetary encounters founders.

BY LEROY ELLENBERGER:

Joseph May commendably presents clearly and objectively the far-ranging scope and coherence of Velikovsky's work. The explanation of collective amnesia (pp. 38-39) is one of the better such. His answers to many criticisms, e.g., Venus' early cometary nature and relegating Occam's Razor to its proper place, are most welcome as is the explanation of the limitations of uniformitarianism. When discussing plate tectonics (p.34), May might have pointed out that, contrary to Sullivan¹, the assumed validity of plate tectonics does not necessarily vitiate Velikovsky's catastrophism because the two theories can be viewed as independent concepts. May's is the voice of reason countering almost 30 years of emotional, signal reactions.

However, any survey necessarily excludes much interesting, relevant material. While May is not to be faulted for his exclusions, three themes his essay either suggests or cuts short deserve elaboration. These are Polanyi's attitude, objectivity in science and the reason for the hostile reaction in 1950.

At several points in his essay, May comes very close to meeting Michael Polanyi² head on. The two questions that May proposes to test unconventional theory are a welcome alternate to the casual, even subjective, appraisal of "plausibility" argued by Polanyi and which once rendered in one discipline should be accepted as authoritative in other disciplines through the operation of an "indirect consensus." May also differs from Polanyi in the importance to be attached to correct predictions. Polanyi writes "a theory rejected as absurd will not always be made plausible by the confirmation of some of its predictions... This may lead to the neglect or even suppression of valuable contributions, but I think this risk is unavoidable."³ Unlike May, Polanyi would be content to forsake Velikovsky in order to protect the soundness of the scientific literature.

The practice of science described by Kuhn⁴, paradigm elucidation and subsequent overthrow, is neither the only nor necessarily the best way. Here, again, May comes very close to a key idea in the literature. To answer May's questions (p. 41), it is not necessary to waste so much time serving the dictatorship of a single paradigm, and our minds are not so weak that they cannot permit the co-existence of competing basic principles. The current practice of advocating a single dominant hypothesis while efficient, is not objective. T.C. Chamberlin⁵ recognized this in 1890 when he advocated the method of multiple hypotheses. Advocacy necessarily and conveniently simplifies the management of scientific endeavor.

When scientists adopt a dominant hypothesis, they become its advocate. They seek evidence to confirm this hypothesis, presenting it in such a way as to put their hypothesis in the most favorable light. The null hypothesis is often ludicrously simplistic. In other words, successful scientists are not objective; they are biased advocates as Mitroff⁶ amply documented among Apollo scientists.

A multiple hypothesis strategy, on the other hand, appears to

be both efficient and unbiased. By adopting this strategy, the role of scientist is changed from advocate to experimenter. Bias is reduced because the scientist works with all of the leading hypotheses; they are all "his." Polanyi's unavoidable risk would not appear to be so were science to abjure the paradigm. In a forthcoming article, Armstrong⁷ presents a well-documented comparison of alternate research strategies, finding for Chamberlin's method of multiple hypotheses.

Many instances can be cited in which objectivity suffered from the bias in advocacy. Psychology Today⁸ provides the latest example. "Contrary to our beliefs about how science proceeds from facts to conclusions and interpretation, Shields [investigator of the history of the debate on the relationship among intelligence, brain size and gender] discovered that scientific agreement on the interpretation of the facts determined the facts themselves."⁹ Exacerbating this condition is the observation that advocates tend to discount contradictory data. Contrary to logic, a person with a strongly held belief when confronted with disconfirming information does not re-evaluate the belief, but instead becomes more concerned with defending and justifying himself. Batson¹⁰ concludes "The more one publicly proclaims one's conviction about personally significant truths, the more one seems bound to these truths. One is less free to modify one's position, to take account of new, discrepant information." Indeed, it would appear there is room for serious consideration of Chamberlin's recommendation.

At the end of Sections II and IV, May skips fleetingly over an explanation of the hostility in the scientists' reaction in 1950. Stove¹², Stecchini¹³ and Parry¹⁴ make compelling, eloquent statements explaining the hostile reception, but theirs lack total conviction. The first two cite Velikovsky's challenge to the comforting dogma of an eternally stable solar system, while Parry remarks on the blindness of disciplinarians to the interdisciplinary synthesis which admits to scientific consideration subjects not ordinarily considered to pertain to science. Other spokesmen, such as Storer¹⁵, avoid a visceral, individual level explanation, preferring the aloofness of an institutional one. This simply will not do because science is performed by individuals whose personal motivations are relevant. Two obscure articles, amplifying May's sentiment, provide the convincing explanation.

In discussing scientific prejudice, using Velikovsky as an example, Graff¹⁶ observes "The intensity of reactions shows clearly that something within the scientist has been threatened... the prejudiced scientist has failed to distinguish between self and object, that his ego is under threat if his world concept is not absolute and that his identifications have not become sublimatory. It is because of this that the prejudiced scientist must attack. Failure to do so would precipitate depression."¹⁷ Bennett¹⁸ supports this view when writing "... the orthodox scientist lives his suppositions. Destruction of an entrenched frame of reference destroys the conventional scientist because, in effect, it eliminates the personal commitment so essential for self-sustainment... Such a person must uphold his preconceptions or risk losing all."¹⁹ Thus, it is clear, that under the influence of strong ego forces scientists who felt threatened by Velikovsky's

thesis reacted with the violence they did. The impact of that original reaction persists to this day as many original participants are still alive and younger scientists appear to follow blindly the judgments made so hastily in 1950 and before²⁰. In commenting on Scientists Confront Velikovsky, Talbott²¹ remarks "One must decide whether he wants to read or misread. Communication requires cooperation of the recipient, and if the recipient wishes to misunderstand, he is surely free to do so."²²

As May's paper outlines, Velikovsky's work poses a formidable challenge to many disciplines in science and the humanities which may be viewed threateningly by the less secure. The works of Bass (refs. 30-31) and Juergens (ref. 38) amply illustrate what may be very serious oversights in the conventional astronomers' Weltanschung. These articles are essential for an understanding of the plausibility of Velikovsky's celestial dynamics. In archaeology, a new book by John Dayton²³ shows, on the basis of the latest analytical developments in metallurgy, that the whole chronology of the Near East before about 700 B.C. is erroneous. Although the book does not mention Velikovsky, its conclusions are generally supportive. Such independent corroboration is strong support. It would appear the tide is turning. The point is not that Velikovsky is 100% accurate, but that there is room for legitimate discussion of his ideas.

In closing, Meynell²⁴ observes: "... the impressiveness of Velikovsky's thesis is due as much to its coherence within itself as to its correspondence with ascertainable fact. It is not that he has been fertile in the invention of ad hoc explanations of puzzling data of archaeology and ancient history; but that one at first sight extremely improbable hypothesis [that Venus and Mars menaced the Earth in historical times, causing global catastrophes], which should be liable to falsification in any number of ways, is in fact apparently confirmed in as many ways."²⁵

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- ¹⁵Norman W. Storer, "The Sociological Context of the Velikovsky Controversy," Scientists Confront Velikovsky, Donald Goldsmith, Ed. (Ithaca, NY: Cornell University Press, 1977), pp. 29-39.
- ¹⁶Harold Graff, "Scientific Prejudice: The Velikovsky Incident," Bulletin of the Philadelphia Association for Psychoanalysis, Vol. 23, No. 4 (Dec. 1973), pp. 288-306.
- ¹⁷Ibid., p. 301.
- ¹⁸Andre M. Bennett, "Science: The Antithesis of Creativity," Perspectives in Biology and Medicine, Vol. 11, No. 2 (Winter 1968), pp. 233-246.
- ¹⁹Ibid., p. 242.
- ²⁰The leader of the opposition, Harlow Shapley of Harvard, without reading Velikovsky's manuscript, wrote to Horace Kallen on May 27, 1946, "... if Dr. Velikovsky is right, the rest of us are crazy." See Horace Kallen, "Shapley, Velikovsky, and Scientific Spirit," Velikovsky Reconsidered, Editors of Pensee, (New York: Doubleday, 1976) p. 23.
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- ²⁴Hugo Meynell, "Schools of Thought--A Reply," Soc. for Interdisc. Stud. Rev. I,4 (Spring 1977): 5-8.
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BY MICHAEL JONES:

To begin with it is necessary to banish once and for all the absurd suggestion that 'the vested interests and ego involvement' of some scholars have prevented them from taking a rational approach to Velikovsky's work. This kind of statement is exactly that which can be calculated to antagonize scholars most effectively, and provide a justifiable reason for thinking that Velikovsky's writing and followers are trivial and uninformed. The appearance of new evidence constantly causes scholars to modify their views, and it must be realized that no closed caucus of scholars exists with interests at stake. No-one, however eminent, can appear a fool either before colleagues or the public, if his work is thorough, competent and truthful, so long as he adjusts his views when new material comes to light and is prepared to believe in his own fallability.

In my paper read at the Glasgow "Ages in Chaos" conference, I proposed a number of criteria by which the validity of Velikovsky's arguments might be assessed. These involved an examination of how he has dealt with information in archaeologists' reports, what his proposals show of his ability to examine Egyptian texts in their original versions, and what the implications of his proposals are in the light of the great quantity of important and informative material which he does not discuss. By putting these tests into operation it became apparent that the "evidence" described by Joseph May as "persuasive" regarding the scheme in Peoples of the Sea is only superficially convincing. The core of the plan is the replacement of Ramesses III of the 20th dynasty (c.1175 B.C.) into the person and period of Nectanebo I of the 30th dynasty (c.375 B.C.) This suggestion is supported by the existence of the same name or title in the titulary of both Pharaohs, the appearance of Greek letters on the reverse of tiles from a palace of Ramesses III at Tell el-Yahudiyeh, the close similarity between plumed head-dresses worn by Persian soldiers in reliefs at Persepolis and those worn by warriors of the Pr(1)s.t in the mural decoration of Ramesses III's mortuary temple at Medinet Habu, the meaning of the Egyptian Pr(1)s.t as "Persians" not "Philistines" as it is usually translated and the close similarity between the architectural styles of temples of Ramesses III at Thebes and the well known Ptolemaic temples at Edfu, Esneh, Kom Ombo and other sites. Each of these suggestions must be looked at to determine its value in Velikovsky's reconstructed chronology.

The source which Velikovsky used for the titles of Ramesses III was compiled by E.A. Wallis Budge and published in 1908. Budge's transcriptions are not only out of date but also wrong. If Velikovsky had been aware that since 1963 the correct version of the titles has been available to students and scholars both in its original version and in translation, the mistaken identity of Ramesses III would not have occurred. Where Budge has written "Nekht-a-neb" which Velikovsky saw as the name of Nectanebo I, Ramesses III's sculptors actually wrote "nekht-Ca/neb khepesh" meaning "great of strength, possessor of power." This group is only an epithet of Ramesses III and never occurs in a cartouche containing the royal name, whereas Nectanebo is the

personal name of that Pharaoh, and is always given in a cartouche in his inscriptions. In interpreting the characters on the reverse of tiles from Tell el-Yahudiyeh as Greek letters of the fourth century B.C., Velikovsky is reviving an old theory first put forward in 1887. At that time it was thought that these tiles may represent restorations made to much earlier buildings by Ptolemaic rulers. When, in 1930 in Mahmoud, Hamza wrote an article about the marks found on other tiles of the Ramesside period, it became clear that the Tell el-Yahudiyeh marks belonged to a corpus of potters' marks much larger than is represented only on the tiles in question. They are shown in Hamza's article to be derived from hieratic versions of hieroglyphic signs the forms of which can be verified from other sources. The particular sign that Velikovsky sees as a Greek "A" is clearly the hieroglyphic lotus flower, which is far from being an unknown sign "never before found on a papyrus or on stone."

Many of the questions surrounding the Sea Peoples, known from monuments of Merneptah and Ramesses III as enemies of Egypt, are unsolved. The various groups of peoples who formed the confederacies spoken of in Ramesses III's texts can be distinguished by their peculiar clothing and the names written over their figures in the reliefs at Medinet Habu. The nature of the hieroglyphic script however, complicates the matter by recording only consonants and using the signs for "r" to mean "l" also. In this respect the Egyptian writing of the name Pr(l)s.t, which applies both to a group of people, and to a country, is at first sight open to a number of interpretations. The translation "Persians" or even "Persia" is in direct conflict with the fact that in Egyptian the name for "Persia" is written consistently in a way quite distinct from the writing of Pr(l)s.t, although in English transliteration they appear similar. On epigraphic grounds a reading "Peleset" at Medinet Habu seems more appropriate in view of the later Hebrew "pelishtim", Greek "Palaistine" and Arabic "Falastin." Furthermore on a statue of the 22nd dynasty the name Pr(l)s.t used topographically, is closely associated with K3n^{CC}n, "Canaan." Since Ramesses III seems to have settled Peleset in at least four Canaanite cities, Gaza, Ashkalon, Ashdod and Dor, in a region which has had their name ever since, Velikovsky's poorly researched alternative explanations appear very unnecessary.

The Peleset at Medinet Habu wear a type of feather head-dress which is fixed in place by means of strap tied under the chin. They are all clean shaven, and their battle dress is a knee-length striped kilt and a sword carried across the chest. They are bare-footed and seem not to wear armour. The soldiers depicted at Persepolis, with whom Velikovsky would wish to equate the Peleset, all wear long robes with flowing sleeves. These of course, are ceremonial costumes and as such not comparable to the battle dress of the Peleset, but they all have bushy hair and beards which have become characteristic of the Persian appearance, and cannot have been grown specially for state occasions. Their weapons are long spears held vertically and on their backs they carry quivers. Their head dresses are quite unlike those of the Peleset, being much straighter, shorter and without the chin strap which would have been unnecessary for a small, crown-like head gear, which can be

seen in the reliefs at Persepolis resting comfortably on the head.

Finally the architectural evidence must be considered. Velikovsky maintains that the similarity of temple buildings erected under Ramesses III, the 21st dynasty and the Ptolemaic period provides a strong case for their virtual contemporaneity. Ramesses III's surviving monuments are limited to two temples at Karnak and his mortuary temple at Medinet Habu, and temples of the 21st dynasty are nearly all totally destroyed except for the forecourt of the temple of Khons at Karnak completed by Herihor. Ptolemaic temples are well preserved in numerous centres all over Egypt. A cursory glance at photographs of all these buildings is enough to show that they belong to different architectural styles. The Egyptian temple was constructed according to a plan which the Egyptians regarded as a sacred foundation decreed by the gods and preserved in ancient writings. This idea is found in Egyptian religious texts and is not derived from a visual survey of the standing remains within the confines of the accepted chronology. So Egyptian temples of all periods exhibit unchanging features which derive not from the whims of architects but from the Egyptian view of the functions and significance of a temple. Individual elements within the temple developed dramatically however, and the most marked are the column capitals, column shafts and lintels. During the 30th dynasty a composite floral column capital appeared in temple buildings which was unknown in Ramesses III's time, and is clearly visible in the colonnade of Nectanebo I at Philae, which Velikovsky strangely, does not mention. In many of the Ptolemaic temples and their associated "birth houses," which are themselves a distinctly Ptolemaic feature, half engaged columns with walls rising between them provide a typically Ptolemaic appearance to the buildings. At Edfu, where the temple of Horus is the best preserved of all Egyptian temples, all these features can be seen, together with the "broken lintel" device occurring over inner doorways. The most striking feature of Ptolemaic temples however, which is unlike anything previously done by the Egyptians, is the mural decoration. In appearance the hieroglyphs are composite and new signs abound whose reading differs from that of earlier inscriptions. In content the religious texts provide information about mythological events which was previously kept on documents hidden in temple libraries. The temples of Ramesses III and the 21st dynasty belong to the tradition of the New Kingdom builders, with their closed lotus bud capitals and rather fat column shafts. The two types of architecture are very unlike one another.

When the complete state of the evidence is weighed against Velikovsky's suggestions it becomes clear that his claims cannot stand up to scrutiny because they are based on an inadequate survey of the data. His ineptitude in dealing with Egyptian texts which constitute primary source material, becomes obvious with every step he takes. The notion that the Pharaoh Siamun, of the 21st dynasty, was alive during the fourth century B.C. and was buried in the Siwa Oasis demonstrates this point. Siamun is the most fully documented of all the rulers of the 21st dynasty, and his activities all over Egypt are attested by remains of his buildings at Tanis, Memphis and Heliopolis and inscriptions from Thebes. He was not a "priest king" as Velikovsky describes him, and his name is always written in Egyptian in a cartouche as,

S3-Imn "Son of Amun," with the prefix nsw "king." The tomb at Siwa identified by Velikovsky as belonging to this ruler actually contains reliefs dedicated by a man named Si-mn which in hieroglyphs looks different to S3-Imn, but in English transliteration receives vowels which give it a deceptive similarity with the Pharaoh's name. If Velikovsky were able to understand Egyptian, this kind of elementary mistake would never have occurred. If he had a wider knowledge of the Egyptological bibliography, he would be capable of drawing on a much more useful background of published information, and if he could assess the value of his sources the out of date and unreliable authors of early Egyptology would not be used in preference to their modern counterparts.

If we now turn to Ramesses II and his Time we see that not only is Velikovsky's latest book filled with mistakes similar to those mentioned above but that the whole is based on a false premise. On the second page of the introduction we read that the activities of the 26th dynasty rulers of Egypt are known from Biblical and Classical sources "but not from the extant Egyptian texts." This is not the case. Hieroglyphic inscriptions on stelae, tomb and temple walls, coffins, sarcophagi and graffiti are probably more numerous from individuals of the 26th dynasty than from any period since the 19th or early 22nd dynasty. Not all these texts are published, but some are, and among Egyptologists there are those who have specialized in this period and whose work reflects that interest. None of the monuments of private individuals find a place in Velikovsky's work, either for the 26th dynasty or for the time of Ramesses II, the 19th dynasty. If they were included they would provide little support for his ideas. Velikovsky has relied heavily on Schmidt's Ramesses II, a chronological structure for his reign. This is clear from a handful of footnotes and from his comments on the few Ramesside texts which he does include. I doubt however, whether Velikovsky has read the important review of Schmidt's book in the Journal of Egyptian Archaeology 61 (1975) where its shortcomings were pointed out.

At Deir el-Medineh in Western Thebes, the necropolis workmen of the New Kingdom led a sheltered life. They represented a specialized branch of Egyptian society, but they have left behind a legacy of ostraca, stelae and tomb paintings which illustrate family bonds, the relationship between the villagers and the Theban authorities and the workings of the Egyptian legal system. The importance of this material lies in its density, since individuals named in several documents can be identified, linked with their family and colleagues and placed chronologically by means of the numerous inscriptions dated by regnal years of the Pharaohs of the 19th and 20th dynasties. Thus the archive of Deir el-Medineh provides historical information which is invaluable in any study of New Kingdom Egypt. Velikovsky does not mention it in either People of the Sea or Ramses II and his Time. It is possible to compile a chronological scale for the 19th and 20th dynasties from the genealogical data at Deir el-Medineh which corroborates the accepted royal chronology. Workmen such as Neferhotep, who began his career under Horemheb, can be identified as members of families spanning reigns and even dynasties. One of Neferhotep's descendants Amennakht, known from his petition in the Salt Papyrus, links the reigns of

Sethi II and Siptah with that of Ramesses III by his involvement with the Chief Workman Paneb and the Vizier Hori. The disjoining of the dynasties proposed by Velikovsky would attribute lifetimes of several hundreds of years to these individuals, as well as to many others.

As we read through Ramses II and his Time, more and more "facts" are introduced by Velikovsky which, upon examination, turn out to be useless. A Pharaoh appears whose name, according to Velikovsky, is Seti-Ptah-Maat. No ancestry is given for this ruler although he is cast in the important role of father of Ramesses II, and no monuments bearing his name are listed. In fact he is totally unknown in Egyptian records, and seems to have been contrived from a distortion of the real name of Ramesses II's father, Men-maat-Ra Sethi-mer-en-Ptah whose father, Ramesses I and grandfather, Sethi, are known together with their links with Horemheb and thus with the immediately preceding 18th dynasty. In the section dealing with the problem of squeezing Ramesses II's conventional sixty seven years long reign into Necho II's sixteen, we read that "hardly any document is dated in the last two or three decades of his (Ramesses II) reign." Besides being a rather vague assessment of what must be a vital length of time in Velikovsky's calculations, it must be said that there are at least twenty more positively dated inscriptions surviving from the second half of Ramesses II's reign than from the first thirty three years. This mistake would never have come about if Velikovsky were aware of the real state of the archaeological evidence. It seems that having established the foundations for his reorganized chronology, Velikovsky now realizes that he cannot proceed without the complete evidence for the periods he discusses mounting up in conflict against his views. As a result most of this evidence is overlooked.

One of the most important arguments which Velikovsky puts forward to justify the identification of Ramesses II with Necho II, involves a massive duplication of Ramesses' regnal years based on a long coregency with his father. In formulating this view Velikovsky has overlooked a vital piece of evidence, a mistake arising from his lack of knowledge of the Egyptian language and Schmidt's confusion of the same point in the work cited above. The figure of sixty six years found in Manetho for the length of Ramesses II's reign does not occur in isolation. It is verified by contemporary records, and the last dated document of his reign is a legal papyrus from Kahun belonging to regnal year 67. Ramesses II therefore probably survived for a full sixty six years and died during his sixty seventh. The Egyptian term used since the Old Kingdom for dating the years of a reign was h3t-sp "regnal year," and Ramesses II's first inscriptions known from Abydos are introduced by h3t-sp 1 "regnal year 1." The regency of Ramesses with his father, whose inauguration is described so vividly in the famous "inscription dédicatoire" at Abydos is said to have begun in rnpt tpyt "first year." h3t-sp and rnpt tpyt are orthographically completely different and should never be confused. Ramesses II's reign cannot be dated from his appearance as prince regent because the Egyptian texts distinguish between these two periods.

The length of the reign of Sethi I, Ramesses II's father and predecessor, was probably fifteen or sixteen years. This is suggested

by evidence supplied in inscriptions on the statue of the High Priest of Amun, Bakenkhons, in Munich. The text gives an outline of Bakenkhons' career up to the date of its dedication (which must have been during Bakenkhons' lifetime since he prays for a long life on earth), and the stages of his progress add up to seventy years, and clearly the final years are not included. The important points are the length of Bakenkhons' life and the date of the statue indicated by the cartouches of Ramesses II. Bakenkhons' son and successor in office, Roma Roy, states that he was inducted into the High Priesthood by Ramesses II, but since he is otherwise only attested under Merneptah and Sethi II, this event, together with the dedication of Bakenkhons' statue must have occurred very late in the reign of Ramesses II. Obviously not all Bakenkhons' life could have been spent under Ramesses II, and on his Munich statue he says that he spent his first eleven years as a youth, hry iḥw-n-shpr n nsw Mn-m3Ct-RC ie. under Sethi I. Now, if Bakenkhons is to have survived most of Ramesses II's sixty seven years, as we know he did, Sethi I could not have ruled for much more than fifteen full years. Velikovsky does not deal with evidence of this kind from stelae and statues of private individuals which have a direct bearing on the chronology of the New Kingdom.

Under Velikovsky's revised scheme the 22nd or Libyan dynasty ruled during the ninth and eighth centuries B.C., as they do according to accepted chronology. Thus if Ramesses II is in reality Necho II (610-595B.C.) how could Sheshongq III of the 22nd dynasty have erected a gateway at Tanis out of the dismantled blocks of one of Ramesses II's colossal statues? If King Seti-Ptah-Maat, Ramesses II's father, is really the ruler known in Egypt as Sethi II, one of Ramesses II's sons, how could that Sethi have had carved an oracle text dated in his first regnal year, in the temple of Ramesses II at Abu Simbel? If the 18th, 19th and 20th dynasties are really separated by hundreds of years as Velikovsky suggests how can workmen from Deir el-Medineh together with state officials of various ranks in different parts of Egypt begin their careers under one dynasty to die centuries later early in the next? For example: how could the future Ramesses I have risen through the army as a colleague of Horemheb, have been appointed vizier by that Pharaoh, and then have succeeded to the throne as Ramesses I two hundred years later? How is it that the Viceroy of Nubia, Paser, appointed by Horemheb is followed in office by his son two hundred years later under Sethi I? How is it that the Vizier of Upper Egypt, Hori, attested under Siptah and Queen Tawosret is still alive under Ramesses III a hundred and twenty five years later? How can the 20th and 21st dynasties have been contemporary when Herihor, the first ruler of the Theban branch of the 21st dynasty, proclaims his investiture as High Priest of Amun on stelae bearing the cartouche of Ramesses XI? Similarly, how can Smendes and Ramesses III be regarded as contemporary when Smendes is attested during the reign of Ramesses XI and then succeeded him as Pharaoh for at least twenty years? These are some of the questions which Velikovsky should have attempted to answer in his "Questions and Answers" section of Ramses II and his Time, instead of the hypothetical arguments which he introduced and which really involved him in more anomalies.

In the field of Egyptology Velikovsky has explained nothing.

The glossy praise for Velikovsky's work, clothed in disdain for Velikovsky's critics with which Joseph May concluded his article demonstrates this. A picture of events in the ancient Near East is compiled by collecting all the data available for study in whatever form it occurs, and then assessing it soberly without trying to fit it into a preconceived chronological scheme. If all the evidence could be shown to support a 20th dynasty eight hundred years later than accepted history places it, then no Egyptologist could afford to reject this view. Velikovsky however, has not demonstrated that any of his proposals can be verified by serious examination, and this is the reason why scholars can give no support for his work.

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BY MALCOLM LOWERY:

I can only give a fervent welcome to Dr. May's attempt to persuade science and scholarship to adopt a rational approach to Velikovsky's ideas and wish it every success; on every point he makes, I find myself in full agreement with him, and I applaud the facility with which he has compressed an overview of the salient issues into a paper of necessarily limited scope.

In his closing section, Dr. May speaks of being "opposed to having a dogma enshrined" and espouses a situation of "competing basic principles." This rejection of an uncompromising black-and-white view is one which this Society also hopes to foster; and the act of allowing two opposed views to co-exist must also imply a recognition that neither need be totally right or totally wrong. In a paper in *Pensee*, Dr. Euan MacKie, a founder of the S.I.S. and a participant in both the MacMaster symposium and the recent Glasgow conference (1), distinguished Velikovsky's "two, possibly three, General Theories and a large number of Specific Theories stemming from them" and suggested that "some of the specific theories could be drastically modified, or even disproved, without affecting the general theories" (2). This, I think, is a position which it is only reasonable to take: not every principle of present-day knowledge is to be thrown overboard; and no more should any one innovator expect every detail of even the most inevitable breakthrough to vindicate itself without amendment.

To cite a case in point: according to Egyptological authorities, monuments from Old Kingdom Egypt unimpeachably and unequivocally record a year consisting of twelve thirty-day months plus five "days on the year"; and this 365-day year is confirmed by students of other Near Eastern civilizations. This situation (which I intend to research if ever my editorial duties allow me the time) obviously will not square with Velikovsky's proposal that, until the Mars disasters of the 8th and 7th centuries BC, the year had a length of just 360 days (3); but, if borne out, it would not imply a complete rejection of Velikovsky's theory, or even of that part of it which postulates a catastrophic sequence at that time. It would simply mean that such events as there were had no permanent effect on the calendar. The necessary adjustment, in terms of the specific theory, might be large, or it might be small: it would not shake the foundations of the general theory. And the very presence of these "epagomenal days" in numerous

calendars, considered inauspicious by the Egyptians (and coincidentally called "unlucky days" by the Maya) and surrounded with myths referring to celestial deities, is a strong hint that, at some time in the past, catastrophic events have changed the calendar.

In the historical arena, too, a discriminatory approach is called for. More and more evidence is mounting up in favour of Velikovsky's recasting of the history and placement of Egypt's 18th Dynasty: in the "revised chronology," with the Exodus at the end of the Middle Kingdom, an extended Hyksos rule, Sheba identified with Hatshepsut and Shishak with her successor Thutmose III, the synchronisms desperately lacking in the conventional scheme fall into place of themselves (4). Similarly, papers in Velikovskian journals have put the onus firmly on conventional scholarship to justify its construct of a "Dark Age" in Greece, Anatolia and elsewhere (5). But in other areas the evidence is not so clear-cut. In seeking to separate the 18th and 19th Dynasties by almost two centuries, Velikovsky is faced with evidence, both from royal tombs and from secular records, which provides severe problems for his hypothesis (6). Moreover, his proposal to identify Dynasty 19 with the 26th Dynasty of Manetho and other writers brings him up against archaeological and epigraphical material from both inside and outside Egypt which provides some apparently quite insuperable difficulties; nor is it easy for the researcher acquainted with the subject to accept that the Hittites were merely the Chaldaeans under other names (7). However, if the traditional sequence of the dynasties following the 18th is maintained, once again evidence appears to materialize of itself to accommodate a revision essentially devolving from Velikovsky's revised placement of the earlier period (8). Also on the positive side, two recent developments should be noted, the first concerning Velikovsky's suggestion that the origin of species traces to mutations caused by the excessive radiation environment of the time of the catastrophes. Mainstream geological work of the last two decades has increasingly concentrated on correlating glaciations, geological periods, geomagnetic reversals and the sudden origin or extinction of species. Papers by the late Otto Schindewolf and other specialists have been collected in a recent issue of a geological journal (9), and consider these features at length: while not committing themselves to Velikovsky's scenario, they propose a common element for these phenomena in recurring catastrophic circumstances in which radiation plays an important role.

In the field of celestial dynamics, Dr. Robert Bass, speaking at the Glasgow conference last Spring, drew attention to a proposal which (with a deep breath) he introduced as "Melvin Cook's sophisticated Madelung-force dynamic-lattice plasma-theoretical electromagnetic theory of gravity" (10) - a theory which would allow electrically-charged planets to behave in a perfectly Newtonian manner, even to their perturbation effects, except on close approach. Within a certain distance (corresponding to the Debye length of plasma physics), their charged nature would lead to all the non-Newtonian effects predicted by Velikovsky's theory but difficult to reconcile with orthodox models.

Dr. May demands a "return to the fundamental tenets of scholarship," which it seems are easily forgotten. I sincerely consider that

perhaps the best "map" of these tenets for anyone seeking a path through the Velikovsky controversy is the approach suggested by Dr. Hugo Meynell of Leeds University in a current issue of the S.I.S. Review (11). Admitting his debt to Bernard Lonergan, he maintains that the misunderstandings, misrepresentations and impasses with which the nascent debate is still encumbered derive from the critics' (and occasionally Velikovsky's advocates') inability or refusal to "be attentive; be intelligent; and be reasonable." To this Dr. Meynell adds: "Be responsible." With abstract "truth" often taking a very low priority, it may be that attention to the evidence, intelligence in considering alternative explanations, reasonableness in preferring the best-suited of these "whether it conforms to one's convenience or mental habits or not," and responsibility in acting appropriately are a good deal to ask. But it is the only way the debate will emerge from a mire of recrimination and continue on a proper scholarly plane.

NOTES:

1. The four-day symposium, "Velikovsky and the Recent History of the Solar System," held at McMaster University, Hamilton, Ontario in June 1974, is reported in depth in Pensee VIII, 37-43. The Proceedings of the April 1978 weekend conference "Ages in Chaos?" are planned for publication by this Society early in 1979 on behalf of the organizers, the Department of Adult and Extra-Mural Education of Glasgow University; a report summarizing the papers given has appeared in S.I.S. Review, Vol. III No. 1, 1-6.
2. E.W. MacKie: "A Quantitative Test for Catastrophic Theories," Pensee III, 6-9. MacKie's proposals did not meet with universal approval: see Lynn E. Ross: "The Logic of Theory-Testing: Some Criticisms of MacKie," Pensee V, 34-5, with a reply by MacKie.
3. Two references must suffice here: (a) Hastings: Encyclopaedia of Religion and Ethics (Edinburgh, 1908-1926), II (1910), 93: "As it has just been described [with a year of $(3 \times 4 \times 3 \times 10) + 5$ days] . . . the Egyptian calendar appears throughout the whole of its history. However far back we may trace it, we cannot reach the moment of a change in it . . ." (b) Helck/Otto (eds.): Lexikon der Ägyptologie (Wiesbaden, 1975-), III, 298, article: "Kalender" by J. von Beckerath: "Auf der Grundlage eines [unregelmässig 12- bzw. 13-monatigen] Lunisolar jahres wurde in Ägypten schon früh ein . . . Kalender . . . geschaffen, der aus unveränderlich 365 Tagen bestand. Er war nach dem Vorbild des natürlichen Kalenders in 12 Monate zu je 30 Tagen eingeteilt, wozu noch 5 Zusatztage (Epagomenen) kamen."

Even if the Egyptologists' confidence in an ancient 365-day year should prove to be misplaced, Velikovsky is certainly in error when he states that the epagomena "are known from the documents of the seventh and following centuries" and "must have been added to the 360 days subsequent to the end of the Eighteenth Dynasty" (W in C, 321-2, Gollancz edn.), as the "days on the year" (hryw rnpt) are named, and even listed, in tomb inscriptions of the 5th Dynasty (see Lexikon I, article: "Epagomenen"; also K. Sethe:

"Die Zeitrechnung der Ägypter . . . ," Nachr. d. k. Ges. d. Wiss. Göttingen, philolog.-hist. Kl., 1919-20, 304, and references given therein). This is not in conflict with a 360-day "year" at that time, as the very name shows that these 5 days were considered "external" to the "year proper" (Sethe, loc. cit.).

4. See especially E. Danelius: "The Identification of the Biblical 'Queen of Sheba' with Hatshepsut, 'Queen of Egypt and Ethiopia' in the Light of New Archaeological Discoveries," Kronos I.3, 3-19 and I.4, 8-24; and papers by J.J. Bimson, E. Danelius, P.J. James, I. Velikovsky, M. Sieff and G.J. Gammon in S.I.S. Review II:3 (Special Issue - From Exodus to Akhnaton).
5. I. Velikovsky: "The Lion Gate at Mycenae," Pensee III, 31; idem: "Tiryns," Pensee VI, 45-6; idem: "The Scandal of Enkomi," Pensee X, 21-3; idem: "Olympia," Kronos I.4, 3-7.

From other Velikovskian researchers, see especially I. M. Isaacson on Pylos and Gordion, Pensee IV, 26-32, and on Troy, Mycenae and Tiryns, Pensee IX, 5-20; also L.M. Greenberg: "The Lion Gate at Mycenae," Pensee III, 26-30; and papers by J.J. Bimson and P.J. James to be included in Glasgow Proceedings (note 1).

An introduction to the problem via a consideration of V.R. d'A. Desborough's book The Greek Dark Ages (1972) in S.I.S. Review I.1, 15-16; summary of arguments in pamphlet available from the Society.

6. See G.J. Gammon: "The Place of Horemheb in Egyptian History," S.I.S. Review III:2, - ; M. Jones: "Some Detailed Evidence from Egypt against the Revised Chronology," in forthcoming Glasgow Proceedings.
7. Gammon, op. cit. last note; J.J. Bimson: "Can There Be a Revised Chronology without a Revised Stratigraphy?" in forthcoming Proceedings; idem: "An Eighth-Century Date for Merenptah," S.I.S. Review: 2, - ; P.J. James: "A Critique of 'Ramses II and his Time,'" S.I.S. Review III:2, - ; idem: "Velikovsky's Revised Chronology and the Archaeology of the Hittites," in Glasgow proceedings.
8. Bimson, "Merenphah" (op. cit.).
9. Otto H. Schindewolf (Tübingen): "Neocatastrophism?"; L.J. Salop (Leningrad): "Glaciations, Biologic Crises and Supernovae"; Harold Aspden (Southampton): "Galactic Domains, G Fluctuations and Geomagnetic Reversals"; V.A. Obruchev: "Fossil Cemeteries" (book extract); J.B. Kloosterman: "Apophoreta" on anomalous high radioactivity in fossil bones, in Catastrophist Geology (ed. J.B. Kloosterman), Year 2, No. 2 (Dec. 1977), from C.P. 41.003, Rio de Janeiro, Brazil.

(Of particular interest are the "occasional short-term fluctuations [of g] at times of reversal of the geomagnetic field" proposed by Aspden: this is a startling parallel to one of Velikovsky's earliest (1946) and most controversial published claims.)

10. Summary in Cook's Science of High Explosives (Am. Chem. Soc., 1958 / Krieger, 1971), 420-6. Dr. Bass's discussion is intended for inclusion in the Proceedings.
11. H.A. Meynell: "A Philosophy for Interdisciplinary Studies," S.I.S. Review III:1, 22-7. Cf. also: R.G.A. Dolby: "On Schools of Thought," SISR I:3, 26-30 (reprinted from Social Studies of Science 5, 165-175) and H.A. Meynell: "Schools of Thought - A Reply," SISR I:4, 5-8.

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BY R.G.A. DOLBY:

Many thanks for sending me Joseph May's paper on Velikovsky. I appreciate receiving up-to-date material on the Velikovsky affair. May I suggest, however, the following chain of difficulties in May's simplistic analysis of which heterodox ideas we should take seriously. In reading it I noted a latent inconsistency. It seemed to me that May's casual rejection of orthodox alternatives to Velikovsky like the modern theory of continental drift, showed such a level of ignorance that he must be defying his own principles of what to take seriously. Plate tectonics as explained by its supporters (though not by such critics as Velikovsky) is both coherent and makes a prima facie case. To be consistent, May should be as receptive to these ideas as to Velikovsky's, even though the two theories are inconsistent with one another. Each can be regarded as a provisional approach to the truth.

If this is conceded, however, May's initial criterion that we should require heterodox ideas to be coherent becomes less natural. For surely a heterodox thinker who asks us to take seriously two incompatible sets of ideas, each of which is prima facie plausible and internally consistent, should not be treated differently from two people offering the sets of ideas separately. Of course, once this criterion goes, we are left only with the requirement that we should take everything seriously which we find prima facie plausible. The implication of my remarks is that by making his criteria of what to take seriously so weak, May makes the criteria he does offer harder to defend.

BY ROBERT MCAULAY:

For the most part I find Joseph May's piece a cogent and useful discussion of certain strengths of Dr. Velikovsky's work. It is to May's credit that he has been able, in a limited space, to make a lucid case supporting the notion that Velikovsky's ideas are worthy of serious consideration. Whatever the paper's merits, however, I find that May, in suggesting certain standards for the future assessment of heterodox views, glosses over some very real difficulties. It may well be that Mr. May and I would have little to disagree about were we able to sort these things out in advance but, on the surface at least, he appears to simply assume that because he finds Velikovsky's ideas "coherent" and feels that a prima facie case has been made that other reasonable individuals (who had themselves properly scrutinized Velikovsky's work) would also agree. I suggest, however, that for a number of reasons those who support and those who oppose Velikovsky have continued

to assess the coherence and prima facie grounds of his ideas differently.

The problems here are especially intricate because Velikovsky advances notions which not only (to some) clearly conflict with, say, the laws of conservation of energy and angular momentum but, in addition, seem to embody a world view at odds with that characterizing modern science. [I have tried to indicate the competing ideologies tacitly associated with Catastrophism and Uniformitarianism in my "Velikovsky and the Infrastructure of Science: The Metaphysics of a Close Encounter," Theory and Society 6 (3), 1978]. In this light, I would argue that it is first incumbent upon us to ask how it is that what is seen by some (e.g. May and those associated with Kronos and Pensee) as substantiating Velikovsky's claims is not so seen by others (e.g. "mainstream" scientists) and, consequently, to attempt to uncover the nature of the apparent gestalt impasse here.

Whatever the extraordinary facets of the case, moreover, the dispute over Velikovsky's ideas can be fruitfully used to highlight certain ordinary features of scientific practice. Hence, I take exception to May's description of a uniformitarian "paradigm" as simply entailing a sort of theoretical "dictatorship" -- a point of view which partially prevents May, I suspect, from appreciating that, paradoxically, the very feature of normal science which furthers certain scientific work may, during times of revolutionary science or paradigm disputes, serve as a set of blinders. Said differently, the Velikovsky controversy makes accessible the very processes which Thomas Kuhn suggests are likely operative in time of paradigm crisis. If we examine closely the interchanges and arguments over Velikovsky's work we may well discover precisely those things which make the resolution of any paradigm dispute difficult. This does not preclude arguing for specific correctives (as does May) but makes those possible recommendations contingent upon a systematic and detailed examination of arguments, counter-arguments, apparent misunderstandings and cognitive blindspots.

Furthermore, May's suggestion -- however reasonable -- that we initially suspend judgment on the truth or falsity of unconventional ideas in order to assess their coherence and prima facie support, would not, in my opinion, substantially alter the way in which novel ideas are assessed now. Logical coherence and empirical support are, of course, the two central criteria by which we assess truth claims. Complicating matters, however, is the fact that "coherence" is a much more nebulous concept than May acknowledges and, like judgments of plausibility, consistency, etc., often depends on the eye of the beholder. (Kuhn's treatment of the Copernican Revolution, for example, suggests that astronomers of Neo-Platonic persuasion found in sun-centered astronomy a plausibility and mathematical coherence which others of a more Aristotelian bent did not). Hence, May's argument that we use coherence, i.e. internal consistency, as one basis for determining which heterodox ideas to take seriously does not help a great deal because it leaves unanswered the question of who is assessing that coherence. I suggest that a well-intentioned

but nonetheless heterogeneous group of individuals -- even if they agreed on May's particular definition of "coherence" -- would have considerable difficulty agreeing that something as complex as Velikovsky's work met that standard.

Secondly, May argues that the existence of a "prima facie case" should be an additional criterion for assessing unconventional ideas. Here May seems to operate with an unexamined notion of evidential support and appears to overlook the problem that certain "facts" are only apparent. Whatever their often partial knowledge of his work, Velikovsky's scientific critics have regularly argued that the evidence does not in fact support Velikovsky's claims. It is, for instance, difficult to find an advance claim by Velikovsky (Venus' surface temperature, etc) which is not at some point disputed as support for Velikovsky's reconstructions. This challenge to apparent evidence is not, however, simply a cantankerous resistance to novel ideas as seen by the fact that Velikovsky and his supporters have themselves challenged other apparent evidence (e.g. Plummer's analysis of Venus' upper atmosphere, and the radio-carbon dating of certain materials) which might be seen to falsify Velikovsky's claims. Again May minimizes the question of who is judging the "fit" between facts and theory and seems to imply that because he finds prima facie support for Velikovsky's case that other informed observers will as well. But before we can, in this case and others like it, simply rely on the "evidence" as advanced, we must first understand how "facts" are often equivocal and how people with different cognitive (or interested) points of departure may variously interpret the same evidence.

All of this having been said, the above comments should not necessarily be construed as at odds with the spirit of what May proposes. But unless the very real complexities which underly the differential assessment of Velikovsky's work are first brought to light, we advance little beyond seeing one side as simply correct and the other side as fanatically, nefariously or dogmatically wrong. In fact Zetetic Scholar is an ideal place to attempt to extend and improve upon May's analysis by offering a comparison of certain key arguments and counter-arguments in order to document the ways in which Velikovsky supporters and antagonists have often simply talked at cross purposes. We might thus be able to discover conflicting assumptions which partially underly the dispute. At another level, one could (as I have tried to suggest in the footnotes to my own work cited above) demonstrate the ways in which each side in the controversy has attempted to challenge the evidential support marshalled by the other side. In the process the equivocal nature of empirical claims could be made visible and we might well go a long way toward eradicating a much too narrowly empiricist view of science. While some may be most concerned with re-fighting the Velikovsky wars, I would argue that ZS serves a higher purpose by self-consciously and dispassionately attempting to understand the reasons for the failure of cognitive consensus which lie at the heart of the matter.

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BY PETER J. HUBER:

One problem with Velikovsky and his followers is that all too often they maltreat their sources (I drew attention to a crass example on the first page of my contribution to Scientists Confront Velikovsky). Another one is that they tend to repeat the same, clearly wrong assertions ad nauseam (for example, the 360-day year mentioned by May is a fairytale, it has no more physical reality than the 360-day year nowadays used in interest calculations). This situation is not very conducive to a continuing fertile discussion.

I see also other reasons why the chronological works of Velikovsky (starting with Ages in Chaos) have been met with comparative silence. In Worlds in Collision, Velikovsky was spanning a bridge between the historical and the natural sciences. The natural scientists were upset - they felt that Velikovsky's conclusions were physically impossible, but that he had made a convincing case, which would mislead the non-scientists, in view of the extensive historical evidence he had quoted. With regard to his chronological works the situation is less complex, since only a single discipline, history, is involved. Among historians the attitude seems to prevail that Velikovsky's claims simply will not be taken seriously by anybody even a little bit familiar with the original sources. Why should a specialist then waste his time to refute, say, Velikovsky's ridiculous assertion that Hattusili III and Nebuchadnezzar II are one and the same person?

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BY DONALD GOLDSMITH:

Dr. May has a temperate style, but, like other Velikovsky defenders, seems unwilling to face the fact that Velikovsky's physics cannot stand the test of reality. Dr. May does go so far as to say that "The first impression of nearly every physicist is that this [the heart of Velikovsky's assumptions] is precluded by the known laws of physics." This is true; it is also true, despite Dr. May's implication, that this is also the second, third, and continuing impression of every physicist I know. And whom does Dr. May cite, in scholarly fashion, to disprove these impressions? Why Velikovsky, of course! Who else?

Dr. May seems impressed by the coherence of Velikovsky's assumptions, and feels that this is an important point in any theory's right to be considered. Of course this is so; the fact that I do not find Velikovsky's theories particularly coherent hardly affects the fact. But is this enough for a theory to have much claim to acceptance? No; Dr. May admits that a "prima facie" case is needed. Velikovsky's prima facie case consists of the great sales of his books. I consider this a good reason to discuss Velikovsky's theories, but not a good reason to take them seriously at a scientific level.

Dr. May winds up by assuming that once-rejected theories will eventually be accepted. If this were so, we would be in awful trouble. Velikovsky's interdisciplinary synthesis gives readers a lot of comfort. So do the Bible and the Koran; so did the Ptolemaic system; so does the

coherent system of astrology. I do not consider the scientific rejection of these theories to be a head-in-the-sand attitude, despite the many efforts to make these works have a scientific foundation. I hardly think that those who reject Velikovsky's theories are making the same mistake as Robert McNamara's Pentagon computers. Rather I believe that completely adequate evidence exists for disproving Velikovsky point by point. I am impressed by the wholehearted commitment of those who support Velikovsky, but it cuts no ice with the facts. Since I have edited a book that presents these facts, I am content to refer readers of the Zetetic Scholar to it (Scientists Confront Velikovsky, Cornell University Press, 1977).

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DR. JOSEPH MAY WILL RESPOND TO HIS CRITICS IN A FUTURE ISSUE.

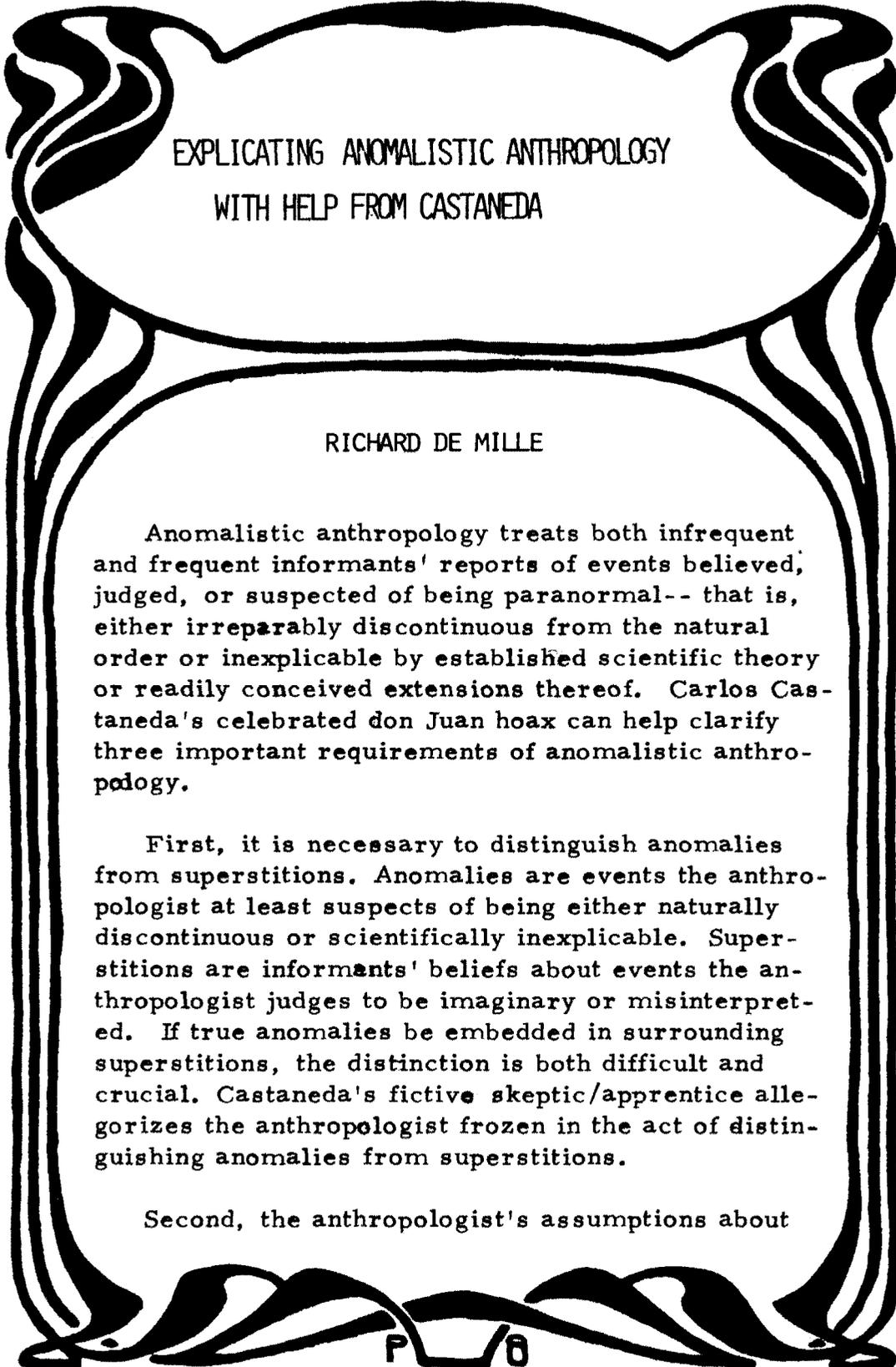
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EPILOGUE

Comments were received from approximately half of the scientists I contacted. Objections to this dialogue came from both sides. Friends of Dr. Velikovsky's views felt that Dr. Velikovsky was making no paranormal claims and therefore should not be discussed in a journal such as ZS that deals with matters like parapsychology. Contrary to his critics, these defenders argue that no fundamental physical laws are violated by the scenario conjectured by Dr. Velikovsky. Thus, in a sense, these defenders argue that Dr. Velikovsky is putting forward too legitimate a theory for ZS consideration. On the other hand, some of his critics argue that Dr. Velikovsky's theories are so contrary to fundamental laws of physics that his ideas are on a par with things like the Flat Earth theory and thus do not deserve consideration in a serious scientific journal at all. In other words, they think the claims are too illegitimate for ZS to consider them seriously. This is what makes the controversy particularly interesting (especially for the sociology of science) and alone should justify attention to it by ZS.

Upon receipt of Dr. May's paper, I found it a good and temperate statement by one sympathetic to Dr. Velikovsky's views. I did not agree with all it contained--I find that I respect Dr. Velikovsky's views but find them very unconvincing--but I thought it an excellent starting point for discussion. Unfortunately, many critics of Velikovsky believe that his theories have been completely discredited by the criticisms to be found in Scientists Confront Velikovsky. I think these critics have simply not carefully read the responses to Sagan et al. in the issues of Kronos and the S.I.S. Review. Though some of that critical response may be "nit-picking," much of it is substantial. I do not suggest that Velikovsky is right and his critics wrong --overall, I am inclined to agree with his critics--but it appears to me that very much if not most of the criticism of Velikovsky remains badly done. His critics misread him and too often do not do their "homework" properly. In addition, Dr. Velikovsky is too often opposed not because of what he conjectures but because of the "comfort" some of his ideas may give to some fundamentalist religious groups. Dr. Velikovsky should not be held responsible for some of the religious-political uses of his ideas when he has clearly dissociated his views from such purposes. Even if only parts of what Velikovsky has put forward stand up under scrutiny, it deserves our attention. We need less authoritarianism from both sides. I hope this ZS dialogue is a beginning.

--M.T.



EXPLICATING ANOMALISTIC ANTHROPOLOGY
WITH HELP FROM CASTANEDA

RICHARD DE MILLE

Anomalistic anthropology treats both infrequent and frequent informants' reports of events believed, judged, or suspected of being paranormal-- that is, either irreparably discontinuous from the natural order or inexplicable by established scientific theory or readily conceived extensions thereof. Carlos Castaneda's celebrated don Juan hoax can help clarify three important requirements of anomalistic anthropology.

First, it is necessary to distinguish anomalies from superstitions. Anomalies are events the anthropologist at least suspects of being either naturally discontinuous or scientifically inexplicable. Superstitions are informants' beliefs about events the anthropologist judges to be imaginary or misinterpreted. If true anomalies be embedded in surrounding superstitions, the distinction is both difficult and crucial. Castaneda's fictive skeptic/apprentice allegorizes the anthropologist frozen in the act of distinguishing anomalies from superstitions.

Second, the anthropologist's assumptions about

the paranormal should be made explicit at all stages of work, whether planning, observing, analyzing, or reporting. Every anthropologist makes assumptions about the paranormal. Most of these are tacit assumptions. When apparent anomalies are unexpectedly encountered, hidden assumptions bring about skeptical or subscripitive reactions that may not represent the best interpretations the anthropologist is capable of. Either following such an unexpected encounter or before intentional observation, the anthropologist should interrogate himself (includes herself) about all beliefs, feelings, and predispositions toward manifest or alleged paranormal events and make his findings about himself explicit in writing. At every new experience, the interrogation should be repeated and changes noted. Only by making the assumptions explicit will the anthropologist recognize the provenance of his plan, maximize his receptivity to field experience, analyze his observations in clear cut and useful categories, and write an unambiguous report. Good work can be done by skeptic or subscriber, but not by one masquerading as the other.

Third, the authenticity (versus the fraudulence or other spuriousness) of fieldwork and resulting data should be assessed to the full extent this is possible before judging the validity (theoretical correctness) of findings and interpretations. Though generally regarded by anthropologists as wholly inauthentic, the don Juan books contain many elements apparently borrowed or adapted from authentic works of others and generally recognized as valid --for example, shamans do perform feats of agility atop Mexican waterfalls. Validity in Castaneda's work has been confused with authenticity. The published assertion that no doctoral committee can effectively assess the authenticity of a dissertation is a travesty of science and an insult to countless fieldworkers who have reported in good faith to the best of their ability. Fraud is a perennial hazard in anomalistics, but abject submission to it is neither honorable nor necessary.

* Abstract for the symposium "Extrasensory Phenomena and Medical Anthropology II: Cultural Patterns, Empirical Evidence, and Problems of Verification," at the 77th annual meeting of the American Anthropological Association, Los Angeles, November 1978.

ASTROLOGY: A REVIEW SYMPOSIUM

PROLOGUE

Responsible concern with inquiry into paranormal events must focus upon their best, their most responsible proponents. The occult press, especially sensationalistic newspapers and magazines, should be of little interest to the serious inquirer. Too much discussion of astrology has centered around "popular astrology." Attacking simple sun-sign astrology is largely a waste of time for the serious since such popular forms are admitted to be nonsense --or mere entertainment-- by those astrologers claiming scientific status. A manifesto denouncing newspaper astrology columns could as easily be signed by the leading astrologers as by a group of respected scientists. But such public manifestos are in any case of little effect and really are better examples of scientific authoritarianism or public relations than they are scientific. For as philosopher Paul Feyerabend succinctly put it recently, what's the point of a petition with 186 signatures if you have arguments? Claims of the paranormal must stand or fall on the basis of evidence, so the important thing is for us to seek out the best arguments both pro and con. Initiating such consideration, ZS is pleased to call attention to a major new work which surveys and evaluates the scientific evidence on astrology. It is:

Geoffrey Dean, Arthur Mather, et al., Recent Advances in Natal Astrology: A Critical Review 1900-1976. Bromley Kent, England: The Astrological Association, 1977. 598pp. \$25.00, paperback. [In the U. S. A., the book may be ordered through: Para Research, Inc.; Whistlestop Mall; Rockport, Mass. 01966. Otherwise write to: Recent Advances; 43 Granville Rd.; COWES; Isle of Wight; PO31 7JF, U. K.]

To most modern scientists, the idea of significant cosmic influence upon human behavior seems implausible. The matter is made worse when no concrete mechanisms are suggested by which such effects are obtained and made worse still when the explanations offered are couched in language reminiscent of supernaturalism and occultism. Those concerned about the absence of mechanisms seem to overlook similar opposition to Newton's theory of gravity with its action at a distance, once seen by critics as occult; and some have suggested that Newton may have been untroubled by the action at a distance problem largely because of his own early involvement with astrology. In any case, many sciences (e. g., psychology and sociology) have sought to develop on an exclusive and nonreductive level of analysis. If B. F. Skinner can be allowed a "black box" human being, it might be asked, why can not the "scientific astrologer" be allowed a "black box" universe? If the correlations claimed by the scientific supporters of astrology are not spurious, and if they represent law-like regularities (both very big ifs), why should it be necessary to immediately give reductionistic explanations through mechanisms known to the other sciences?

We should also avoid condemning all astrology claimants because of past supernaturalist connections for this is mere guilt by association. We may be throwing out the baby with the bathwater. As the father of positivism, Auguste Comte noted: "In the early stages of the human mind, these connecting links between astronomy and biology were studied from a very different point of view, but at least they were studied and not left out of sight, as is the common tendency in our own time, under the restricting influence of a nascent and incomplete positivism. Beneath the chimerical belief of the old philosophy in the physiological influence of the stars, there lay a strong, though confused recognition of the truth that the facts of life were in some way dependent on the solar system. Like all primitive inspirations of man's intelligence, this feeling needed rectification by positive science, not destruction; though unhappily in science, as in politics, it is often hard to reorganize without some brief period of overthrow" [Quoted by Paul Feyerabend, Against Method]. And even Sir Karl Popper, who has branded astrology as non-falsifiable and therefore a pseudoscience (an error pointed out by historian Thomas S. Kuhn among others), has noted that "In fact, Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that 'influenza' epidemics are due to an astral 'influence.' And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology." [in Conjectures and Refutations: The Growth of Scientific Knowledge].

On the other hand, we must, of course, exercise great caution in dealing with evidence for extraordinary claims. To some degree, critics may even have too quickly accepted the alleged significance of some pro-astrological evidence that has been put forward. Thus, when correlational studies favoring astrology have been offered, many critics have immediately suggested that the findings might be spurious and have demanded replications. Insisting on replications is appropriate, but it must be remembered that even if the correlations claimed were fully valid, such correlations do not establish causal relations. There are many reasons why two patterns might be correlated but still causally unconnected. (e.g., similar internal but independent rhythms may be present, or they may be connected only through some chain of other factors). In other words, some of the extraordinary evidence being offered by neo-astrologers may be quite valid but really not so extraordinary as it might appear when viewed in the context of astrology.

ZETETIC SCHOLAR is pleased to present a dialogue between a carefully selected group of reviewers and the authors of Recent Advances, Dr. Geoffrey Dean and Mr. Arthur Mather. Whatever one may think of astrology, this book is an important landmark, for it attempts to strip astrology of its pseudoscientific elements and present it in true protoscientific outline.

The review symposium consists of six experts, representing widely different perspectives, and who are both friendly and critical of astrology. These include: Prof. George O. Abell, a prominent astronomer and critic of astrology; Dane Rudhyar, a leading astrologer and a respected figure among many humanistic psychologists; Prof. Hans J. Eysenck, the internationally renowned psychologist and personality theorist; Michel Gauquelin, a psychologist and the generally recognized leader in astrobiological research; Malcolm Dean, a science journalist and the editor of Phenomena, a journal on cosmic influences; and Prof. Joseph Agassi, an eminent philosopher of science particularly known for his writings on social change in science.

The dialogue begins with the first five reviews, followed by replies from Dr. Dean and Mr. Mather. Because Prof. Agassi's review is of article length and is rather different in its character, it is presented separately (as a second section) followed by the replies of Dean and Mather. I have tried to arrange the pieces in a manner that would best allow the issues to unfold for the reader. I found the writings to be revealing, provocative, and sometimes surprising. I hope you will agree.

-- M. T.

REVIEWED BY G.O. ABELL:

Recent Advances in Natal Astrology is a monumental compendium. It is both a survey and, in many cases, a critical review of a large portion of the astrological literature of the past three-quarters of a century. More than 1000 books and journal articles are cited, and these references have been selected from a much larger number of books and papers searched through. It must certainly be the most comprehensive survey of the astrological literature in print, and in large part it is a thoughtful and objective analysis of this literature.

Almost every imaginable subject related to natal astrology is examined, along with many topics only peripherally related to it. There are thorough discussions of the roles of signs, houses, rulerships, planets and other astronomical objects (including asteroids, stars, comets, and even unknown planets), aspects (and lack thereof), angles, midpoints, parallels, retrogradations, and many other astrological patterns. There are also rather extensive descriptions of what I would call nonastrological phenomena that may conceivably suggest relations between celestial and terrestrial events.

More than 50 people collaborated in the compilation, but the lion's share of the work was done by Geoffrey Dean, of Perth, Australia, who lists himself as an analytical chemist, science writer and astrologer. I shall, therefore, refer from here on the Dean as the author, but with the understanding that many others have contributed.

I gather that Recent Advances has occupied a major part of Dean's time over the past several years. I do not know to what extent astrology is his profession, but to judge from the text I would not expect him to be a typical practicing astrologer. One thing is clear: he has a technical scientific background, and a thorough understanding of practical and spherical astronomy, and also of statistics. The technical level of the book is certainly beyond that of the overwhelming majority of astrologers, and the book is in no way intended as an introduction for the layperson. Indeed, many parts are so terse as to be difficult even for one with a technical background. I found it no easier to plow through Recent Advances than the Astrophysical Journal because of the compactness of the material. Dean is writing for the serious scholar.

I did not take time to check all of the mathematical discussions, but I did check through a good many, and among those I found no error in statistical treatment. In fact the worst technical mistake I found was a minor error on page 504, where Dean confuses the sun's motion with respect to the local standard of rest with that about the center of the Galaxy; in the present context, I regard this as an excusable slip. In short, I gained an excellent impression of Dean's competence in elementary statistics and practical astronomy.

But this is a book about astrology, not astronomy and statistics. What does it have to say about astrology? Although Recent Advances is not specifically so divided, I would regard it as consisting of two main parts--the first dealing with traditional astrology,

and the second with the broader questions of relations between celestial aspects and human affairs, or terrestrial events in general.

First let us consider traditional astrology. It derives from a time when the planets were believed to be gods, or abodes of gods, or manifestations of gods (it varied from time to time and place to place). Belonging, as they do, to the celestial realm, the planets were not thought to be composed of the base "elements"--earth, air, fire, and water--that made up the earth according to the ideas of the time. Now from an early period the ancients recognized regularities in the motions of the planets, and because they were believed to influence humans, it was natural to assume that if one could only understand well enough the rules of behavior of the planets--that is, how they move--he might hope to understand the seemingly chaotic and unpredictable events on earth. With such notions, would it not seem that the entire character and destiny of an individual was determined at the time of his birth, when he got into step with the eternal motions of the planet-gods? Thus the key to natal astrology is the natal horoscope--the chart showing the configurations of the planets in the sky as seen from the place and at the time of one's birth. The proper analysis of his natal horoscope was thus thought to reveal one's nature and fortune.

There has always been debate over the significance of birth, rather than conception, or some other period in the subject's early development. Dean, in fact, addresses this matter (pages 465ff), and concludes that the first breath is the significant factor. Now it was not always known that the planets are outside the earth's atmosphere, nor even that they are permanent objects. People of some early cultures believed that the planets formed as they rose and dissolved upon setting; a person near the sea in the West might even hear the sun "sizzle" as it dipped into the water! Imagine a planet, say Mars, just forming as it rises. It is easy to believe that its "influence" can spread through the atmosphere and into the lungs of a newborn baby taking its first breath. Can we not understand how the importance of birth, as well as the object in the ascendancy, may have come to be regarded as important?

The earliest of the many and complex rules for interpreting the horoscope (astrological "laws") came from antiquity. Many are given in Ptolemy's Tetrabiblos. Ptolemy himself actually gives a physical rationalization for some of the rules. For example, the moon, being nearest the earth, soaks up moisture from the earth and so has a dampening influence, while Mars, being nearest the sun (as was incorrectly believed in the Ptolemaic system of cosmology) is hot and arid, and so has a drying influence (actually Mars has considerable water--frozen today--and the moon is bone dry).

Three planets were discovered in relatively modern times, but traditional astrology has rules for them as well. The alleged effects of Uranus, Neptune, and Pluto are just what you expect of the gods of the same names! Traditional astrology is based on a magical correspondence between the planets and the gods for which they are named. It is imbued with a symbolism that is in turn based on the polytheistic religion of the Babylonians and Greeks.

Today we know real rules that govern the motions of the planets.

Men have walked on the moon, and we have landed scientific probes on Venus and Mars. They are not gods; they are worlds composed of the same hundred or so chemical elements that make up the earth.

Physicists and astronomers rejected traditional astrology centuries ago. It is just no longer tenable in the light of modern knowledge. I would think that informed and intelligent people today would no more believe that Venus influences their love lives than that the number 13, black cats, broken mirrors, or walking under ladders bring bad luck, or that futures can be told from analysis of the entrails of animals. It would seem so preposterous to me that there could be any validity to traditional astrology (other than, perhaps, psychological) that I would need to see truly convincing evidence for it to take it seriously.

I rather suspect that Geoffrey Dean shares my views here (although he does not quite say so), at least as far as the astrology based on traditional symbolism goes. He quotes opinions of an enormous number of astrologers on the significance of signs, houses, planets, rulerships, and the like, and describes a surprising number of tests by astrologers which purport to verify certain influences. Dean summarizes the "fog" of much of this work early in the book (page 15):

"The complexities of astrology provide an almost inexhaustible source of new 'discoveries.' What usually happens (but not always) is that the astrologer works out a complete system, adds plausible interpretations, supported by carefully selected examples, and presents as a 'discovery' that in fact is only a hypothesis. A large amount of self-justifying work is then carried out, all of it subjective, and the 'discovery' thereby 'confirmed.' At no time is properly-controlled objective evidence presented. Hence all the astrologer has done is to recycle his hypothesis in the guise of fact. This does not mean that it is necessarily mistaken, only that it could be mistaken."

Although he says, "but not always," in the chapters to follow I found not a single example in which Dean felt that a significant case had been made for any astrological influence of signs, planets, houses, or rulerships. While he does not explicitly reject all of traditional astrology, he certainly does not appear to be defending it. This is bound to disturb many astrologers, and I am not surprised to find strong objections to Recent Advances in some of their reviews of the book.

On the other hand, Dean does describe some studies that seem to him to make a strong case for cosmic influences on terrestrial affairs that would be quite unexpected by the scientific community. The latter part of the book deals mainly with this evidence for what some have called "neoastrology."

And now (if I may be forgiven this terrible phrase) let me make one point perfectly clear: By my polls and those of my colleagues, about a third of all Americans believe in astrology, and about a fifth

are regularly guided by it. When these people consult astrologers to have their horoscopes cast and analyzed, or seek advice from astrological literature, or even consult the daily astrology columns published in newspapers, they are applying traditional astrology--based on symbolism--on magical correspondences between planets and gods--the very astrology for which Dean's exhaustive research reveals no significant evidence of validity. Professional astrologers, such as Carl Payne Tobey, Sydney Omarr, or Zipporah Dobyns, who will analyze your natal chart, or Noel Tyl, who claims to provide diagnostic help based on astrology to physicians, are not basing their analyses and predictions on Gauquelin's studies of planets and professions, on John Henry Nelson's ideas about solar flares and planetary configurations, nor on Piccardi's reports on the precipitation of bismuth oxychloride. They are applying the rules derived from Ptolemy and his followers (unless they simply fabricate their advice; there is ample evidence that customers are equally satisfied with the latter^{1,2,3}).

Of course there are cosmic influences on man. The apparent rotation of the celestial sphere gives us day and night, with sleepfulness and wakefulness. The motion of the earth about the sun combined with the obliquity of the ecliptic are responsible for the seasons, on which depend weather, growth of vegetation, and a host of other phenomena. The moon, and to a lesser extent the sun, cause the tides. And many organisms have developed biological rhythms related to day and night, the tides, and the seasons. These effects, however, are explicable in terms of well understood science, without recourse to unknown laws or ancient magic.

There may, of course, be other cosmic effects yet to be discovered, and possibly some may not be understandable within the framework of known physical laws. It would be very exciting to learn of these; indeed, it is for such discoveries that Nobel prizes are awarded! But such newly-found relationships, even totally unexpected ones, would have nothing to do with traditional astrology; they would have been unknown to the ancients and could not have led to the formulation of the so-called "laws" of astrology. Even if a correlation were found, say, between a certain planet at the ascendant in a natal chart and a human characteristic that it would have predicted according to traditional astrology--even that improbable event would suggest no validity to classical astrology. No such evidence for the effect could have been known in antiquity. There were no statistical studies like those of Gauquelin (statistics had not yet even been invented). Any such discovery from an objective modern investigation could not have been the basis of ancient belief--unless we are to suppose that certain of our ancestors had a magical wisdom, or that the gods had informed them by divine inspiration. Even Ptolemy makes no such claims as this, and certainly such a hypothesis can not be invoked as part of a "scientific proof" of a religious doctrine.

Yet, modern astrologers frequently claim that the validity of traditional astrology is verified by new discoveries of science, ranging from that of radio radiation from the magnetosphere of Jupiter to pulsars. I think this is unfortunate, for it confuses valuable scientific research with ancient superstition, and raises the risk that useful and objective studies may not receive the attention they deserve.

I am sorry that Dean describes many modern investigations within the context of natal astrology.

I feel, however, that Dean is less discriminating in his acceptance of "neoastrological" studies than of studies of traditional astrology by astrologers. He seems to accept uncritically rather astonishing results that in many cases are reported by only single experimenters. It concerns me, because each year many thousands of experiments and observations are reported in the literature. Some are no doubt sound, but many, even by respected professional scientists, are carelessly performed, have inadequate controls, are carried out with biased techniques or selection of data, or occasionally even are fudged.

Consequently, new results at the frontier of science, especially if they are highly unexpected, are not generally accepted without very careful scrutiny, and usually not without repeated replication by other scientists. To be sure, science, being conducted by humans, does not always work perfectly, and there are embarrassing examples in which wrong results have been taken too seriously for a while--such as Piltdown man (a fraud) and van Maanen's measures of the rotations of galaxies (errors of measurement). But science is self-correcting, and eventually these wrong ideas are weeded out.

The more bizarre the discovery, the more convincing must the evidence be for it to achieve general acceptance. In 1887, for example, the physicists Michelson and Morley attempted to measure the absolute speed of the earth in space by comparing the speed of light in different directions. To their complete surprise the experiment gave null results, as if the earth were not moving at all. Even they did not accept the result, but repeated the experiment with better apparatus. Same result. Subsequently, this and other equivalent experiments were performed again and again, and always with the same result, leading to the remarkable conclusion that the speed measured for light is independent of the motion of the observer. Mind, the original result was not rejected, but it was not accepted until its replication was beyond question. A scientist does not have to "believe" a result is true or false, but can defer judgment until a result is so well documented that he can arrive at a rather firm informed opinion.

The result of the Michelson-Morley experiment was not compatible with the ideas of motion held by Galileo and Newton; it required new physics. Einstein, of course, gave us that new physics in his special theory of relativity, in which he showed that different observers, in motion with respect to each other, perceive space and time differently. Even Einstein's relativity theory was not immediately accepted. In fact, his Nobel prize was awarded not for his relativity, but for his work on the photoelectric effect. Soon, though, the evidence that relativity is correct was beyond question; the generation of nuclear power, either in a nuclear reactor or in a bomb, attests to the fact!

The above examples are of results that turned out to be correct, but I could list many times as many cases where surprising results, on further investigation, turned out to be wrong. My point is that one must be cautious in judging new discoveries--especially unexpected ones. Every bizarre result that one can find reported in the literature is

not likely to be correct; probably at best only a small fraction are. It is a mistake, I think, to comb the scientific journals for surprising results of experiments of isolated investigators, and then to list them as evidence that there are cosmic influences that cannot be explained by known scientific theory. Some may eventually be found to be right, but without the kind of verification the Michelson-Morley experiment and relativity theory had to undergo, they are no more convincing than much of the astrological research that Dean rejects.

There is a further danger. Suppose you start out wanting to believe a particular idea (say, cosmic influence) and begin trying to correlate various sets of data in the search for something significant. There is an old adage, "If ye search hard enough, ye shall find." But what you find may be correlations of poor or biased data, or of very few selected results that seem to be significant out of a very large number or trials. Out of every 100 random experiments (say, coin flipping) you can expect 5 or so results that would occur by chance only 5 percent of the time. Similarly, one time in 8 you will throw 3 heads in a row; the occurrence of that event does not prove the coin is biased or has two heads. I note again that many thousands of experimental and observational results are reported each year. Beware of selecting just the one or two that appear to prove your case!

I do not mean the above remarks to deny cosmic influences, but only as an appeal to judge data objectively. In spite of everything, if even one bizarre result among those described by Dean should represent a new principle in science, it is exciting and important (even though it may be entirely irrelevant to astrology). I am in no position (having finite time) to investigate and judge all of the studies mentioned in Recent Advances, and can only state that they have not yet received general acceptance by the scientific community. This does not, understand, mean that they have been rejected by scientists. I would, however, like to discuss briefly two studies that Dean describes at length and seems to regard as of convincing quality.

The first is John Henry Nelson's study of the relation between planetary configurations and disturbances in the reception of high-frequency radio signals. Those disturbances are caused by charged particles from the sun perturbing the ionized layers in the earth's atmosphere, and the particles themselves are ejected in especially large numbers during explosive events on the solar surface called flares. The occurrence of solar flares has long been known to be correlated with other solar phenomena, such as sunspots and prominences, and are part of the general pattern of magnetic activity on the sun. Nelson, however, believes he has found statistical evidence that flares are triggered when certain of the planets reach configurations whereby they are at or near 0° , 90° or 180° apart as seen from the sun. So far as I can determine, Nelson's work was brought to the attention of the general public by astrologer Sydney Omarr. In any case, astrologers have made a point that the traditional astrological idea that squares and oppositions between planets are associated with malefic events on earth seems to be verified by Nelson's findings.

But the traditional squares and oppositions are geocentric, while Nelson's are heliocentric. When two planets are squared as seen

from the sun, they generally cannot be squared as seen from the earth. Moreover, it is not at all clear that solar flares should be considered malefic. They, like all solar activity, were nearly or completely absent during the so-called Maunder minimum from 1645 to 1715, and that was the time of the Little Ice Age in Europe! (In fact, there is accumulating, but still not conclusive evidence that solar activity is correlated with weather on the earth.)

So Nelson's findings say nothing about traditional astrology. Still, if correct, they are interesting. I am familiar with some of Nelson's papers. The first two^{4,5} were statistical analyses that I found unconvincing. Later Nelson⁶ concedes that "The subject is extremely complicated and difficult to produce in a statistical form, since no nine-planet combination will be reproduced by the solar system in several hundred thousands of years." Thus he proceeds to justify his correlation by noting a few individual cases where the correlation seemed to work. He acknowledges that it does not always work, but nevertheless claims that his forecasts of radio disturbances "maintained an accuracy of close to 90 percent for several years."

It is difficult to evaluate Nelson's work. One can not, of course, prove a statistical correlation by invoking individual examples of success. His forecasts also involve three other more conventional criteria besides planetary alignments, and it is vague how he counts a "hit" or a "miss." (In many areas of the world a weather forecaster can obtain 90 percent accuracy by predicting that tomorrow's weather will be the same as today's.) The forecasting center at the Space Environmental Services Center in Boulder, Colorado informed me that they had informally evaluated Nelson's forecasts but found his categories of "hits" too broad to verify the accuracy of his techniques. They have not found his methods useful.

It would not be easy to disprove any effect of the planets on solar activity. Such effects have been searched for for many decades, and even fairly recent claims for correlations have been made^{7,8,9}. However, careful statistical studies^{10,11} have failed to verify these claims. At present there is no thoroughly documented relation between planetary configurations and either solar activity or geophysical effects.

Dean's other "good case" is the work of the French psychologist Michel Gauquelin and his wife, Francoise. Dean says (page 215):

"Of the astrological research conducted worldwide since 1950, that by Gauquelin in France and elsewhere represents without doubt the most fundamental and rigorous work of all. Until recently his was perhaps the only major work in astrology which met the standards of orthodox research."

And on page 217:

"Until recently his was perhaps the only major work in astrology which was scientifically credible, and his published data remain the prime source for fundamental research."

As a youth Gauquelin was very interested in astrology, and he

eventually carried out extensive tests involving tens of thousands of subjects in an effort to verify predictions of traditional astrology, but found no basis whatsoever for any effects of the positions of planets among the signs or houses, and he has now long rejected traditional astrology as worthless. Gauquelin has, however, found what he believes to be significant tendencies for certain planets to either occupy or avoid two sectors of the sky at the times of the births of very successful professionals. These sectors are the one just above the eastern horizon, and the one just west of the celestial meridian. At the birth times of 3647 scientists, for example, Gauquelin finds that Saturn occupied one of the two critical sectors 704 times, as opposed to 598 that would be expected by chance. For 3438 successful military men, Jupiter figured in the critical sectors 703 times, as against 572 expected by chance. These differences are significant, although less so than might appear because there are 10 planets (including the sun and moon) and either can be over or under represented in the critical sectors, giving 20 chances to find a significant relation for each profession.

To date, Gauquelin's results have been critically examined by others only for sports champions, for whose birthtimes Gauquelin finds Mars in the critical sectors more often than expected. For brevity, we call this the "Mars effect." Gauquelin asked the Comité Para, a Belgian committee of scientists who have agreed to investigate apparently anomalous phenomena, to attempt to replicate the Mars effect. He helped the Committee obtain the data for about 500 additional sports champions, and the Committee obtained the same results that Gauquelin did for his original sample of about 1500. However, they criticized Gauquelin's procedure for calculating the expected frequencies on a couple of technical grounds and therefore refused to endorse the findings.

Subsequently my son and I, in collaboration with the Gauquelins¹² analyzed the full sample of 2087 sports champions by an independent method that avoided the problems raised by the Comité Para. Our results were negative, but we had to divide the sample in such a way that the significance of any correlation would be weakened, so the best we could say is that the effect, if present, is weak and that a large sample is required to reveal it.

In 1976 Marvin Zelen, a statistician now at Harvard University, proposed a test¹³ that avoids all of these problems, but that required data on the births of all other individuals born on or near the same day and in the same town as each of the sports champions. Gauquelin cooperated in obtaining these data for a subset of a few hundred of his sportsmen. The results of the Zelen test¹⁴ were marginally significant (at the 4-percent level) in favor of the Mars effect; however, the significance was due entirely to those athletes born in Paris, and the Mars effect did not appear at all for those born in Belgium.

Where, then, do we stand? If there are no irregularities in the procedure, the result of the Zelen test is slightly significant, despite the disparity between the athletes from Paris and Belgium. If the Mars effect were not real, only one time in about 25 would a random sampling of athletes be born with Mars in the critical sectors as often as was

observed. One time in 25, of course, is not an incredibly rare event, but this statistic refers to only a portion of Gauquelin's total sample of athletes; if his original procedure was, in fact, legitimate, the result could be considerably more significant. I think, therefore, that the results are interesting enough to see if they can be replicated by a completely independent investigation.

As a result of the uncertainty of the Mars effect after the completion of the Zelen test, a new study of American sports champions is now underway under the direction of the Committee for the Scientific Investigation of Claims of the Paranormal, an American counterpart of the Comité Para, currently headed by philosopher Paul Kurtz, of the State University of New York, Buffalo. The results should be announced imminently, if they are not already public by the time this review is printed. If the results are negative it will be a setback for Gauquelin's theory, although perhaps not a fatal one. On the other hand, if they are positive, further investigation is called for.

Frankly, I cannot imagine how the Mars effect, or any of Gauquelin's suggested correlations, can be real. On the other hand, I know Michel and Françoise Gauquelin personally, and I have the highest regard for them both, and cannot imagine fudging of data or fraud on their part. But, objectively, nothing can be ruled out until unequivocal evidence has settled the matter. I strongly suspect that in the end Gauquelin's results will turn out to be spurious. But if by any (to me) miraculous chance they should be even partly correct, it would be a tremendous milestone in establishing cosmic influences on man. As in all radical results, however, caution is in order; at present the jury is still out. Gauquelin's findings represent an anomalous result that remains unconfirmed to the degree necessary to be accepted as scientific fact.

But please note that if Gauquelin does turn out to be completely correct, it has nothing to do with traditional astrology; indeed, his results are not compatible with tradition, as Gauquelin himself has frequently pointed out.

In summary, Recent Advances in Natal Astrology is an extraordinary compilation. I am impressed with the job Geoffrey Dean and his collaborators have done in collecting data and in interpreting it. I do not agree with all of the book's conclusions on the significance of some of the material presented, especially in the area of nontraditional astrology, but the book is fully and carefully referenced, and other investigators will find it to be an outstanding resource for all areas of natal astrology, as well as many topics only peripherally related to astrology.

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REVIEWED BY DANE RUDHYAR

This book was certainly meant to be an inclusive attempt to make some sense out of the obvious chaos of systems and opinions constituting what we today call astrology. It is, however, an entirely biased attempt because it starts from a black-and-white, either-or opposition between "truth" (defined as objective and exact knowledge based on scientific research) and "belief." Belief is identified with "symbolism (based on dubious tradition), intuition and holistic understanding" and with "merely subjective" responses having little to do with "facts." And on page 2, a long footnote presents and attempts to discredit the approach to astrology of which I am said to be "perhaps the leading exponent." This approach, however, cannot be understood unless it be in terms of my entire life work which extends far beyond astrology. Neither can it be understood by people who, though they take the position that they speak for "science," are apparently little acquainted with or refuse to accept the new concepts of many atomic and nuclear physicists who, as "philosophers of science" are attempting to give meaning to the evolution in scientific thought produced by quantum physics and related concepts.

The writers of the book (and probably most astrologers occupied with statistical research) seem to have a Newtonian idea of the universe. On page 2 they dispose summarily of the holistic approach which nevertheless is the foundation of the most recent physical theories in which the universe is shown to be a web of relationships, and all separate objects -- including even atomic particles -- are taken to be nothing but patterns of relationships. The writers might do well to read Fritjof Capra's The Tao of Physics -- but this now widely circulated book simply pushes a little further a trend which already began long ago with the great French scientist Henri Poincaré, and of course Einstein, Bohr, Heisenberg, et al.

As an illustration of biased thinking, I might refer to page 2, third paragraph: "No amount of holism leads to the understanding of a clock unless it has been taken to pieces first." A clock is a mechanism made of assembled parts according to a human idea. A human being is not a mechanism, and our body starts from one fecundated cell dividing itself according to a plan or pattern -- whether one calls it "genetic code" or "archetype" depends on one's basic philosophy or cosmology suggesting the place and function of man in the universe. If one accepts the philosophy of Cartesian mechanism or 19th century "scientific" materialism and the concept of every human being as a separate, essentially self-determined atomistic kind of individual, then man is understood in a certain way. I do not accept this philosophy -- and many progressive scientists do not either -- and therefore, in order to give some kind of evocative formulation to what to me is "reality," I found it useful to use a symbolic language, astrology, based originally on the universal experience of mankind facing the dichotomy of, on the one hand, the order and predictability of celestial movements and, on the other hand, the chaotic, unpredictable jungle-state of the biosphere. Today, the physical jungle has become a psychological and sociological jungle; and the symbolism of astrology can be very useful -- somewhat as the symbolism of group-algebra has proven useful -- in interpreting the tracks produced by nuclear particles when atomic nuclei are violently broken down.

Facts (etymologically from factum, i.e. "made") are made by the mind interpreting sense-data according to the paradigms of a particular culture. In our rationalistic and intellectual culture, many experiences which in older cultures were considered caused by objective facts are called "subjective." To speak of an absolute truth or an unquestionably objective reality seems today as meaningless as to think of a chair as a solid inert mass of matter when science tells us it is mostly empty space in which myriads of particles whirl at incredible speed. The only "fact" left is a pattern of relationship between particles which are not "material" but only "waves of probabilities."

This book, Recent Advances in Natal Astrology, however valuable it no doubt is in showing the confusion prevailing in the field of astrology as a result of a fantastic proliferation of new systems and techniques, is, I repeat, a biased book. It should have been called "The Confused World of Empirical Astrology." After decrying in the first pages the philosophical, symbolical and holistic approach, it hardly ever makes reference to these writers who have formulated it in modern astrological terms, Marc Jones and myself. When I am quoted (as on page 79) the criticism fails to mention the answer I and Marc Jones gave to the problem, which in this case was that our present civilization is north-hemispheric and, when it spread south of the equator the people belonging to that civilization kept using the same symbols and way of life. Astrology being a symbolic language has therefore retained a north-hemispheric character. I have not heard of ancient Bantu or Maori or Inca systems of astrology. If they exist, they should be studied.

May I conclude these remarks by saying that I am only interested in astrology as a means to help human beings to give a fuller, richer meaning to their lives and to their relation to the universe in which they live. I see no value in the prediction of exact events or even of precise character analysis. Since I started writing on astrology in 1933

(over 1,000 articles and some 25 books ago), I have received many letters from people telling me how fearful or psychologically confused they had become after consulting even a well-known astrologer and being given biased character analyses and/or predictions of illness, catastrophe, or even death. Statistical research may help to show how unscientific such predictions usually are, but to me what is most necessary is to give to the question I have consistently asked, "What is astrology FOR?" a more basic, philosophical and spiritual answer. Evaluating the relative validity of the present multitude of personal opinions and inconsistent systems on the basis of how they "work" in terms of statistical percentages seems rather futile, considering that many statisticians privately admit that you can prove almost anything by statistics according to the way you formulate the problem or questions asked.

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REVIEWED BY H.J. EYSENCK

Like most psychologists my view of astrology was entirely negative. I believed it to be a relatively harmless superstition which in modern times seemed to take the place of religion for the unthinking masses. I was somewhat shaken in this belief when I came across the work of Gauquelin which seemed to me to demonstrate beyond argument that there was a relationship between a person's personality and the position of certain planets at his birth. Admittedly all that Gauquelin had discovered was a correlation, but then many great discoveries in the hard sciences start with the finding of a correlation - modern cosmology is essentially based on Hubble's discovery of just such a correlation. My wife and I later collaborated with the Gauquelins in a study to demonstrate a quite close relationship between personality and planetary positions, which showed that introverted people tend to be born much more frequently than would be expected by chance just after the rise and just after the upper culmination of Saturn, while extraverted people were much more likely to be born just after the rise or just after the upper culmination of Jupiter and Mars.

My own work with J. Mayo, a British astrologer, tested the hypothesis that introverts tend to be born under the even-numbered signs of the Zodiac, while extraverts tend to be born under the odd-numbered signs, and again the results were highly significant statistically. Many studies such as these are reviewed critically by Geoffrey Dean in this book, which is unique in presenting data in the customary academic and scientific manner, and in using appropriate critical considerations in the evaluation of the data. Such critical acumen is very necessary in assessing the value of published studies. Astrologers, and even scientists with reputations in other fields, have often made errors of design and of analysis in this field, and unless these are pointed out their results may be taken at face value. It is often extremely difficult to assess the probability value of a given finding. This has to be assessed against what is often an unknown number of degrees of freedom. There is an infinitude of astrological factors that could be taken into account, and in every single experiment some of these will undoubtedly appear significant at conventional levels. The significance disappears when a single finding is seen as only one of many possible ones, but it is unknown in most cases just how many variables the experimenter had

originally taken into account, or had looked at; often only the significant findings themselves are published. Many other similar errors are pointed out by Dean, and if his criticisms are taken to heart by investigators in this field, future work would undoubtedly benefit.

Much of the book is inevitably destructive, but such destructive criticism is absolutely essential if the field is ever to become a truly scientific discipline. Even in its most certain findings, of course, the study of the influence of the planets on terrestrial phenomena suffers from the fact that little in the way of causal factors can be postulated or even hypothesized. Thus few people would quarrel with the empirical findings of J.H. Nelson, as explained in his book on "Cosmic Patterns," yet little advance has been made in explicating causal patterns involved. Astrology shares this weakness with the study of E.S.P., but it would not be reasonable to deny a field the status of a science on these grounds; most if not all the physical sciences had to pass through a similar phase where causal explanations were at a minimum.

When all the dross is eliminated by suitable criticism, there are still certain studies which remain to suggest that future research may be useful; Gauquelin's work is of course the outstanding example. I think it is a pity that Dean has given almost equal space to good and bad, large and small studies; it might have been more apposite had he given over much more space to a detailed discussion of the Gauquelins' work, which towers over the rest in a truly Newtonian fashion. A detailed discussion and refutation of the criticisms made of their conclusions would have been worth more than the demolition of some obviously absurd little studies which take up much of the space in Dean's book.

I think it may be useful to make a distinction between astrology in the narrow sense and astrology in the broad sense. The former is a body of doctrine peddled by astrologers, while the latter relates to empirically ascertained relationships between the positions of the planets and terrestrial phenomena, in particular the personalities, diseases, and behaviours of human beings. It would be possible for astrology in the broad sense to become a scientific discipline, and to throw up important findings, without necessarily supporting astrology in the narrow sense. As astrology in the narrow sense has been so much criticized, it might be better to find a new term for astrology in the broad sense, and many people have of course tried to do this. The work of the Gauquelins falls into this category; they have explicitly denied any relation between their findings and classical (narrow) astrology. My own work with Mayo, on the other hand, would support astrology in the narrow sense, provided it could be replicated and shown to be free of artefacts.

Dean's book deserves to be recommended wholeheartedly. It represents an enormous amount of work; it is as complete a summary of published studies as one is likely to get; and it combines elucidation with appropriate criticism. One could of course argue with specific points, but on the whole this is a gigantic labour which has produced a monument worthy of the time and energy spent on it. Future workers in this field will benefit enormously by having available such an excellent compendium.

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REVIEWED BY MICHEL GAUQUELIN

Dean's book certainly is a publication that all people interested in astrology (believer or skeptical) must have in their library. I think it is the most valuable effort for gathering in one volume what astrologers have published since the beginning of this century (and, God knows, how over-abundant the production was!). The references alone deserve consideration.

Having exchanged letters with the author during the preparation of his book, I can also assume that his work was done with a constant regard for the truth. Can I say that I agree with the "philosophy" or the facts developed in the book? Dean states at the first page of his introduction: "In this book 'astrology' means study of correlation between living organisms (especially man) and extraterrestrial phenomena. It does not mean Lucky Stars or similar absurdities masquerading under the same name." Can one share the author's definition of astrology? One can consider, for instance, that the recent work investigating possible influence of solar activity on the earth (unknown from ancient astrologers) is not astrology. In his chapter "Solar Cycles," Dean quotes the Piccardi effect (relationships between chemical reactions and solar activity). My friend, the late Professor Piccardi, who was Director of the Institute of Physical Chemistry at Florence University, Italy, totally ignored astrology, especially natal astrology, and did not agree to call his work astrology. As far as I know, scientists involved in this field of research share the same feelings as Piccardi. On the other hand, an astrologer like Dane Rudhyar does not think that all correlations between living organisms and extraterrestrial phenomena are astrology. Actually, could it be possible to couple Piccardi's scientific thought with Rudhyar's astrological conception? Likewise, according to Dean, my own research into the relationships of the planets and human personality is astrology, and I am not very happy to see my work quoted among others that I do not admire very much. Piccardi, Rudhyar, Gauquelin, three examples showing how Dean's definition of astrology can be debated.

In any case, his definition is not mine: I think that astrology - serious or not - is for the present parked in a restricted area of some theoretical and practical concepts which do not seem in accordance with modern, even advanced, science (though ancient astrology probably had some interesting and profound ideas). But can we criticize Dean for his conception of astrology if his book contains interesting and valuable information as it does? We can note that the sub-title of the book is: "a critical review." Actually, Dean not only presents many astrological works but gives a scientific evaluation of them. Some - maybe many - astrologers were upset by that, but obviously, in many cases, Dean conducted a truly scientific examination. The chapter devoted to "Signs" (of the Zodiac) for instance, is excellent. Courageously, Dean does not hesitate to write: "numerous statistical and psychological studies show that signs as traditionally applied appear to have negligible validity." Dean rightly points out the methodological difficulties of the statistical studies on the Zodiac, especially on Sun-sign experiments. He discusses the necessity to apply astronomical and demographic corrections and to avoid sampling errors, and he indicates the lack of consistency between the outcomes of several experiments. Another informative example is his remarks on the validity on Jonas' theory (the so-called astrological

birth-control) which had wide-spread acceptance in astrological circles during the past ten years. Quoting several verifications, especially Kimball and Kautz's work, Dean shows what dubious value Jonas' theory has.

But obviously, Geoffrey Dean believes in astrology. He is thus induced to give too much importance, in my opinion, to speculative ideas or pseudo-scientific works which - as he honestly admits - do not present any scientific validation. On several occasions, Dean even points out that no data are available so no assessment is possible. In such cases, I think he would have been better inspired to reduce the length of his presentation of such astrological attempts (for example the pages devoted to 'non-planets,' 'lack of aspects,' 'midpoints' and many other "lucky findings" of more or less unknown authors).

Let us also consider Vernon Clark's experiment in more detail, especially since Dean speaks about "Gauquelin's failure anywhere in his writings to cite Vernon Clark's results" (p.563). The fact is Clark's experiment is often quoted as a proof in favor of astrology. Twenty years ago, Clark tested the ability of astrologers by tests in which astrologers had to match some birth charts with some descriptions of occupation and case histories. Dean thinks that "the results clearly indicate strong apparent support for astrology" (p.547). Frankly, I do not think Clark's story so clear for several reasons. 1) Dean writes: "the US psychologist Vernon Clark is one of the very few orthodox scientists who, wishing to investigate astrology, has studied it in depth. In fact he sat the UK Faculty of Astrological Studies Diploma Examination and won their medal for proficiency" (p.544). Well. Good for him. But may I ask in which US university this 'orthodox scientist' was teaching psychology? I have never read anything like that. So, without any additional information, I instead would view Vernon Clark as an orthodox astrologer with some training in psychology; 2) Dean notes that "Clark's trials involved a total of 50 astrologers from Britain, Europe, USA and Australia" (p.545). I regret that we are not clearly informed of the names of the astrologers who so brilliantly succeeded in this test; 3) I am well informed only about what happened in France. Some leading French astrologers completed Clark's tests in 1961 and completely failed. In the well known French astrological journal Les Cahiers Astrologiques (N°94, Sept-Oct 1961, p.226), Paul Colombet, past president of the International Astrological Center, explained these very disappointing results as follows: "The test was not in relation with the normal practice of an astrologer..Vernon Clark's test contains a fundamental defect: a lack of psychology and we are surprised that our astrological friends of In Search (the US journal where Clark's experiments and results were published) did not warn Clark against this lack of psychology which is very surprising for a psychologist." Dean probably was unaware of the French outcomes of Clark's test, for he does not mention them in his book. Of course, Clark's test is not all there is to astrology, and there is much other interesting information in Recent Advances about which I agree and disagree.

In any case, Geoffrey Dean, his assistant Arthur Mather and the Astrological Association - which made possible the publication of the book - deserve our esteem. It was necessary that somebody should undertake such a tremendous work. Now the book actually exists and will

allow people to improve their knowledge of this vexing question that is astrology.

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REVIEWED BY MALCOLM DEAN

Recent Advances in Natal Astrology (R.A.) is a brilliant and daring book. But in many ways I regret to say that its publication in this fashion and at this time is a mistake. Let me place this in an historical context.

During 1975, two significant events in the history of Astrology took place. On one hand, we had the spectacle of about 200 scientists condemning Astrology as unscientific, fraudulent and dangerous. These claims were widely publicized through carefully orchestrated media appearances and press releases, and none of the positions taken by those who were later to form the Committee for the Scientific Investigation of Claims of the Paranormal (CSICP) were supported by proper scientific studies. In fact, few if any of the statements made against Astrology at that time and in subsequent articles have survived critical analysis.¹

That same year, the Recent Advances project was concluding. Involving several dozens of contributors, representing the best astrologers available, the book was to provide the first extensive bibliography of 20th century Astrology - a monumental achievement in itself and one of the most valuable features of R.A. (Note that one of the stated aims of the CSICP was to produce bibliographies, yet none have appeared which could compare in any way to this effort.) Recent Advances is therefore a product of serious Astrology, as clearly distinguishable from popular Astrology. This distinction has never been clearly admitted by the CSICP in their literature; indeed, serious astrologers would have gladly joined with critics in condemning popular Astrology as nonsense. But as a product of serious Astrology, Recent Advances suffered greatly from shortage of time and funding. Obviously, since the scientific community largely believes, antecedent to inquiry, that Astrology is nonsense, it is thought to be unworthy of investigation. So we have the spectacle of a group of prestigious scientists, including Nobel prizewinners, condemning an extremely ancient branch of science as unworthy in every sense, while the astrologers who are condemned have offered time, talent and funding to gather and analyze their vast, unco-ordinated literature. Considering how confidently the skeptics attack Astrology, and how convinced they appear to be that there is no grain of truth to it, is it not surprising that they did not go the extra mile and fund or instigate a similar project as Recent Advances, using all their academic might and prestige?

Given a purely neutral observer and these two events, it is not difficult to determine which is the truly pseudoscientific and which the scientific group.

When I first saw Recent Advances, it was a limited edition draft, containing paste-ups of quotations from various sources, organized

according to concepts and theories. Such a work struck me immediately as both powerful and much needed. The draft contained little critical analysis, and was more in the way of a compendium of the most important sources. Due to extreme difficulties with time and funding, I am informed that Dr. Dean (no relation, by the way) undertook much of the final draft by himself, and accomplished the work in a very short period, considering the vast topic at hand. However, such circumstances are not conducive to a balanced point of view, and I have heard it said that Dr. Dean had to be dragged back from the precipice of negativity, as it were, on more than one occasion. This is quite understandable, (I go through the same experience while reviewing the current astrological literature for each issue of Phenomena) but it does mean that there is a residual tone of negativity inherent in Recent Advances, both in conclusions and style of writing. One leading scientist who examined the book characterized its style as "repellent," and I find I reluctantly agree. The conclusions, it must be made clear, do contain a strong element of personal bias, despite all efforts to the contrary. Harmonics, for instance, receive what amounts to a rave review, even though evidence in their favour is largely as weak as the evidence for other concepts discussed in Recent Advances. And while Dean criticizes astrologers for their lack of precision, Michael Erlewine is criticized for giving his heliocentric ephemeris to several decimal places of accuracy. Readers should therefore be strongly cautioned against accepting Dr. Dean's conclusions on any topic, until they have received proper analysis and discussion. But so huge is the gauntlet that he has thrown down, that this is not likely to happen. The reactions of the various authors quoted in the book and who are known to me have been strongly against entering into any further discussion. Many feel they have been misquoted, and conclusions against their work unwarranted. I feel it is possible that in attempting to come to a conclusion on each topic, Dr. Dean may have inadvertently thrown the baby out with the bathwater, cutting off positive research avenues and conclusions in some cases. It is thus possible for the critic of Astrology to read Recent Advances, remain fairly ignorant of contemporary astrological practice, and come to a negative opinion as to the evidence for Astrology. There are several reasons for this:

Some of the best evidence available for Astrology actually began to emerge just as Recent Advances was in preparation. So we do not find references to the studies of Eysenck (and subsequent replications), and the failure of the Committee for the Scientific Investigation of Claims of the Paranormal to disprove the Mars Effect through the Zelen Test.² Thus, Recent Advances should be considered as a retrospective volume, already slightly out of date, and any negative conclusions which skeptics might draw from it are to the same extent retrospective and incomplete.

Secondly, Dr. Dean has deliberately set out to separate the theoretical from the symbolic elements of Astrology. Humanistic Astrology, a major school of contemporary thought despite its well-deserved reputation for verbosity and platitudes, is glossed over in one hyper-analytical paragraph in the introduction. Similarly, important elements of Jungian and Alchemical symbolism receive little or no mention. The book is written within a certain modern scientific paradigm, bizarre as this may sound to certain critics of Astrology. It emphatically does not represent the mainstream of astrological philosophy, nor does it do an adequate job of reviewing the many astrological concepts which are not

easily subject to judgement by the analytical/reductionist model. Thus, Recent Advances has been characterized as "recent retreats" by at least one astrologer.³ A similar approach is displayed by Neugebauer in his History of Ancient Mathematical Astronomy,⁴ a book which manages to cover the entire history of ancient Astrology while systematically cutting off the symbolic, interpretational and practical levels of the field. This is logical if one cannot allow, even for one instant, that there might be something to Astrology. But Dr. Dean and his colleagues are obviously not only capable of erecting horoscopes; they even use them from time to time. Thus it is extraordinary to find the symbolic and intuitional levels of Astrology so divorced from the contents of Recent Advances. Dr. Dean has stated that he intended R.A. for the isolated students of Astrology,⁵ but I suspect that the book is actually written for the isolated scientist who might be attempting to break into Astrology. It is certainly of little use or interest to the majority of serious astrologers, apart from its bibliography. Finally, we have the question of the political role which Recent Advances might have played. For reasons given above, I suspect R.A. will be of only passing interest to skeptics and critics; they will certainly only use its negativity and negative conclusions to support their own preconceptions, even though Mather has argued to the contrary.⁶ By alienating mainstream astrologers from the current evolution of Astrology, Recent Advances merely hastens the day when the New Astrology may divorce itself from Traditional Astrology.⁷ This would be extremely unfortunate, because Astrology is so all-embracing and has such deep roots in human history. The field demands expertise from dozens of fields to make a good astrologer (obviously few exist), and modern Astrology is now being approached simultaneously from a behaviouristic and a symbolic/intuitional direction. The potential this creates for new scientific paradigms is incredible. Surely no one will seriously argue that the current materialistic scientific model can survive much longer? One needs only to examine the works of Buckminster Fuller to observe the union of astrological theory with physics, numerology, and Art.⁸ As an attempt to bring Astrology within reaching distance of current scientific methods and paradigms, Recent Advances will certainly fail, simply because the initial assumptions of contemporary scientists are so contrary to those in Astrology. Besides, I believe the proper response, which will be observed over the next few centuries, will be for mainstream science to move closer to the position of Astrology, not vice-versa. Fuller has already shown how this might be accomplished. Outrageous as it may sound, there is much of value in Traditional Astrological practice, and those who involve themselves in it for years, in a thoughtful and experimental manner, generally tend to concur with its broad outlines. Thus we are still awaiting the full and responsible treatment of these more difficult aspects of Astrology which Recent Advances has chosen to ignore.

Given the dis-ease felt in astrological circles during the hey-day of the CSICP's attack on Astrology, the authors and sponsors of Recent Advances would have been well advised to consider the tactical implications of their efforts. As far as the scientific community is concerned, I suspect the book will have little effect. As far as the Astrological community is concerned, it might have had a tremendously important effect, if it had simply remained a compendium of relevant texts and sources. As it is, few people can obtain most of the sources given, so we are left with Dr. Dean's bias and conclusions. And because

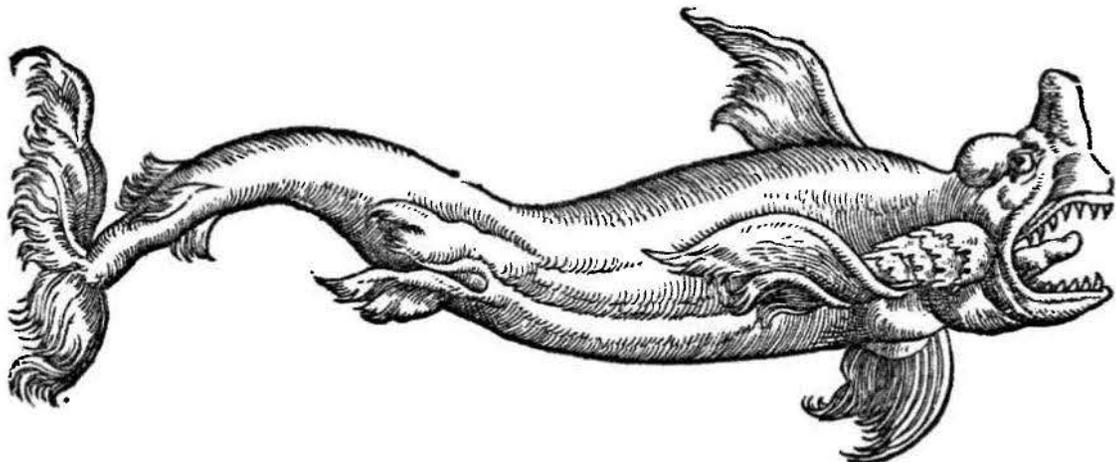
these are often negative, and at variance with mainstream practice, they will largely be ignored. But consider what might be happening if Recent Advances presented a compendium of the important literature in this century! New ground for scholarship would be opened up in the several dozen serious astrological journals. Everyone would very quickly be forced to quote relevant chapters and verses (as is rarely done in astrological literature these days), and we would now be in common possession of a uniform literature of modern Astrology. Certainly, practice and theory would remain, in the strict sense of the term, unscientific, at least for a while. But we would quickly have begun to see a series of commentaries upon the literature, among them Dr. Dean's, with a wide variety of approaches and results.

An information revolution would have commenced. As it is, we are still waiting.

NOTES

1. see Carl Sagan, The Humanist, Vol 36(1), Jan/Feb 1975, p.2; Rockwell, Phenomena 2.2, Mar/Apr 1978, pp.22-27; Feyerabend, Science in A Free Society, 1978.
2. see Phenomena 2.1, Jan/Feb 1978, pp.1,3; Phenomena 2.2, Mar/Apr 1978, pp.1,6-11,18-21.
3. a review of various comments on Recent Advances in the current astrological literature will be published in a forthcoming issue of Phenomena.
4. Neugebauer, O., A History of Ancient Mathematical Astronomy, Springer Verlag/Berlin/Heidelberg, New York, 1975. See also the review in J. Hist. Astron, Vol 9(3), October 1978, p.203: "Neugebauer deals exclusively with mathematical astronomy and successfully avoids subjects that we know are repugnant to him: philosophy, metaphysics, religion."
5. Phenomena 2.3/2.4, May-Aug 1978, p.36. col. 2.
6. Ibid. p.37, col. 2.
7. Prof. Truzzi uses the term Classical, but I prefer "Traditional" because it is both methodologically correct and avoids any confusion as to the historical period under discussion.
8. see for example: Utopia or Oblivion, The Overlook Press, New York, 1972, pp.54-56.

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the authors respond

RESPONSES TO PROFESSOR ABELL:

FROM GEOFFREY DEAN:

Professor Abell has raised some interesting points, with which we are largely in agreement, as follows:

He is correct in saying that we neither attack nor defend astrology. It is true that some astrologers have voiced strong objections to the book, mostly for the reasons given under item 5 of our response to Mr. Rudhyar; under the smoke screen thus erected they conveniently ignore the disconcerting lack of evidence for their beliefs. Two astrological reviewers have condemned us for errors and misquoting but have failed to provide details when cordially invited to do so. On the other hand other astrological reviewers (so far in a 3:1 majority) have voiced strong approval.

Abell makes the penetrating point that the results of modern studies could not have formed the basis of traditional beliefs because the necessary techniques were unknown to the ancients. Thus Mars rising at birth could not have been empirically related to achievement in sport, not because the ancients could not recognize sportsmen, but because the effect (if real) is small and cannot be detected without large samples and modern methods of analysis. Similarly, in traditional astrology the minimum package of 10 planets in 12 signs, 12 houses, and making 5 major aspects, provides nearly 500 different factors of which about 30-40 are present in the average chart. Hence to claim, as many astrologers do, that tradition is the result of millenia of empirical observation, is to claim that the meaning of each of 500 factors can be deduced when any 40 can be present at the same time. This is clearly untenable. Conversely, if the factors were so easy to observe, why is it that today there is no convincing evidence for any of them? The few intriguing results like those of Gauquelin and Eysenck/Mayo may well be statistical artifacts as Abell suggests, but at least we can give them a fair go.

Because of this incompatibility between modern studies and natal astrology, Abell understandably regrets that the former are cited in the context of the latter. We can only say that we sought to include everything of possible relevance (if only because of the paucity of data generally), are careful to state that they are not necessarily relevant, and cite those not supporting astrology as readily as those that do. We readily concede that RECENT ADVANCES contains deficiencies; the sheer weight of material and collaborators, and the

absence of any precedent, virtually guarantees it. On reflection we agree that the critique of nonastrological studies could be stricter. However, if we cite more positive studies than negative studies it is not because of deliberate selection but because few negative studies were found. If they exist then we certainly want them for the second edition.

Abell says it is difficult to evaluate Nelson's work, and inasmuch as his published work leaves much to be desired, we agree; to obviate this problem we checked everything exhaustively with Nelson personally (as indeed we did with the work of Gauquelin and others) and here Professor Abell will recognize the problems involved in seeking help from those you are about to be critical of. On the other hand it should not be difficult to apply the most acid test of all, namely to compare Nelson's forecasts published in 73 RADIO MAGAZINE with what actually happened. Maybe the SESC did not find Nelson's methods useful, but obviously RCA (Nelson's employer) did. So who do we believe? Hopefully someone will publish a definitive study and end this dependence on hearsay.

FROM ARTHUR MATHER:

Professor Abell's review is in my opinion the first which comes fully and fairly to grips with the book. His essential conclusion is that we effectively reject traditional astrology while making a strong case for a scientific "neoastrology". This is not unreasonable for the purposes of discussion subject to two qualifiers. Firstly, overall assessments were deliberately avoided because of the insufficiency of evidence. Secondly, the balance of evidence does point to the rejection of signs, houses and rulerships, but it tends to support planetary characteristics, aspects and angularity.

Abell proceeds to make a very reasonable case against traditional astrology on the basis of the superstitious nature of the fundamentals. He concedes that there may be unknown cosmic influences (possibly with as yet unknown mechanisms) but that these could in no way be connected with ancient beliefs no matter whether the latter were derived from observation, inspiration or elsewhere. As this happens to be the basic hypothesis in question, one cannot accept a straightforward statement of its invalidity, however reasonable the statement may seem, a more rigorous assessment is necessary. (Here Abell is at his closest to Agassi in that he is almost saying that traditional astrology is unworthy of study--the same answer applies.)

On the other hand I also would oppose the claim "that the validity of traditional astrology is verified

by new discoveries of science" - this is loose talk more appropriate to used car salesmen. It is a trifle selfish, though, to suggest that astrological investigators should refrain from citing good scientific work for fear of demeaning it. Why should we be responsible for the prejudices of the scientific community? We have done our best to maintain standards, and full references are given so that serious readers can check the quality of papers cited for themselves. Paradoxically, we then come under fire for citing some substandard scientific work, our standards on the scientific side being "less discriminating." There may well be some cases (please let us know), but in the main I would dispute this for three reasons:

1. Any publication in the scientific literature comes under some sort of editorial scrutiny, so the average level is way ahead of that in the astrological press.
2. All known relevant work is cited, not just that supporting cosmic influence.
3. Our own expertise, such as it is, was applied to the assessment of all material cited.

We do point out that in order to make the survey comprehensive much material is considered which would not normally be acceptable. It has all nevertheless been treated with an equal rigour (we hope). The book is consequently not intended to be a final pronouncement on the validity of traditional astrology, but a gathering (and sometimes weaving) together of many loose ends for the benefit of modern researchers. This is a far cry from trying "to prove [a] case" by scouring the literature until sufficient supporting material is found.

Of those scientists whose work relates to astrology, Abell selects the two most prominent for detailed examination - Nelson and Gauquelin. On the work of Nelson, we would agree with him that there is a subjective element which desperately needs checking; but his results have been published for many years, and we know of no refutation. Insofar as his techniques have not been made completely clear, we hope that our book has remedied this; hopefully some tests will now be made. Apart from using his techniques to generate fresh predictions, Nelson's own past predictions can be checked. Abell's analogy with weather forecasting is not directly comparable as Nelson's predictions were, I believe, made well in advance, not on the previous day. Our use of the implications of Nelson's work in further discussion does not necessarily mean we accept its validity without

question.

On the work of Gauquelin, Abell recognizes its merit while reserving judgement pending further studies, a replication being currently underway in the USA. This is indeed very welcome, but we do suggest (page 394) the importance of the question of controls. It has already been suggested that the expected frequency distribution might not be as regular as supposed. One possible cause of this would be a bias in birth-times through the day. Rawlins examined and rejected this, see Phenomena, 2 (3-4), p. 22, 1978. However, it is still possible that a combination of this with an uneven distribution through the year could give a spurious effect. A simple test would be to take Gauquelin's own data and alter the years randomly. If the characteristic distribution persisted, then all the results would be spurious. Both those who are for and against astrology (in the broadest sense) as a serious field for study recognize the importance of Gauquelin's work. It is probably not putting it too strongly to say that everything hangs on it.

The review as a whole has highlighted the key issues and areas, and basically I would disagree only in occasional emphasis. Professor Abell's perspicacious and fair-minded assessment has truly been a delight to read.

RESPONSE TO MR. RUDHYAR:

Here are Mr. Rudhyar's main points together with our response:

1. The book is biased because it distinguishes truth from belief. A curious view.
2. It attempts to discredit Rudhyar's symbolic approach to astrology. This is correct. However we criticise the approach rather than the symbolism, as shown by the following two excerpts:

"The point is not that symbolism is useless (which it is not) but that it is sufficiently vague and subjective to justify disbelief unless supported by objective evidence. This does not mean that symbolism is necessarily wrong, only that it could easily be wrong" (page 4). Such niceties evidently do not worry Rudhyar. It is not surprising therefore that in his writings he consistently fails to present all sides of the story, such as the effects of universal validity, gullibility and intuition (which alone can explain many allegedly astrological effects), thus denying the very holism

he purports to promote. If Rudhyar were more credible he would be much less a target for discrediting.

Also: "In view of the unique insights which symbolism can provide it is unfortunate that the writings of its proponents [meaning Rudhyar et al] tend to be characterized by a minimum of facts and a maximum of abstraction and diffuse circumlocution. Such an approach serves only to obscure truth and frustrate understanding" (page 2). Here the evidence is before you.

3. Our statement that "Holism does not lead to understanding clocks etc." is biased thinking because humans are not mechanisms, many progressive scientists do not accept 19th century philosophies, and Rudhyar finds astrology useful. Another curious view. It seems that Rudhyar is attempting to evade the discipline of classical physics by clinging to speculations about the "new physics". However, much of the "new physics" is being verified by the methods of classical physics; hence although the latter may not be the only approach it is not without validity.

4. It rejects the holistic approach. This is incorrect. We say "Holism alone is not enough...It is a fact of life that all complex problems have to be simplified in order to be tackled, and everything is won or lost by the way they are simplified. Hence reductionism must precede holism if we are to gain genuine insight, and neither is indispensable" (page 2).

5. It fails to cite modern symbolic and holistic approaches to astrology. On page 9 we point out that to do so would fill many volumes, and that even these would not be useful because they would be full of contradictions. Furthermore such approaches are based entirely on concepts unsupported by facts, and you cannot critically review concepts without facts. it is hardly fair for Rudhyar to complain about omissions without also pointing out the problems attendant upon their inclusion.

6. It is more important to examine the philosophical and spiritual purpose of astrology than to examine it scientifically. We believe that both are important. Rudhyar's criticism is rather like condemning a chemistry book because it contains no psychology.

7. Objective studies are futile because you can prove almost anything by statistics. Which implies you can prove almost nothing by statistics. This is

a manifest untruth. In any case the answer is to learn enough about statistics to avoid being misled. If Rudhyar wishes to lose all credibility, he can hardly do better than this. Of course statistics can be only as good as the person using them, and indeed we have shown that in many studies their use is faulty. Our own use has been with great care and has been independently checked, but we do not claim infallibility. No doubt many of the studies are of little value, but we can hardly be blamed for the work of others. No doubt we have failed at times to ask questions which would make the available data more meaningful, but this was not our main aim, which was to assess existing studies.

In a nutshell Rudhyar rejects science (and hence our book) in favor of symbolism, and hence accepts belief in lieu of truth. He demonstrates the point noted in the previous issue of ZS (p. 126) that no-one who derives spiritual benefit from astrology is going to accept evidence for its invalidity. In other words my mind is made up: don't confuse me with facts. Those who were expecting a book review rather than a parade of obfuscation will be disappointed.

RESPONSE TO PROFESSOR EYSENCK:

Professor Eysenck suggests that too much importance is given to work of little value. This point is also raised by Mr. Gauquelin. Unfortunately most of this work is not available through most libraries. Hence to dismiss it out of hand would deny independent appraisal, and in any case would resemble the authoritarian approach of the 186 scientists, to which astrologers and genuine scientists are justifiably hostile. We believe that, to maintain credibility, worthless work has to be seen to be worthless. Hence description is necessary. This has the advantage that the resulting parade of inutility is far more pointed than any comment from us.

Professor Eysenck rightly points out how little is known about possible causal factors. We have shown (page 515) how a consideration of solar and terrestrial cycles provides a plausible model of causation which not only accommodates cycles and astrology but also the findings of Nelson, Gauquelin and Addey. It is too early to say, but it may well link up with Eysenck's theory of the biological basis of personality. We admit there are many gaps, but at least everything is consistent with the facts, and with further study a breakthrough should not be impossible.

RESPONSE TO MR. GAUQUELIN:

FROM G. DEAN & A. MATHER

(Mr. Gauquelin's quote from Colombet's article could be enlarged to read as follows, otherwise one of the points we both mention will be obscure: "The test...contains a fundamental defect: he gives us a purely external picture of each subject, whereas experience shows that astrology mirrors the internal reality...we are surprised...etc".)

Here are Mr. Gauquelin's main points together with our response:

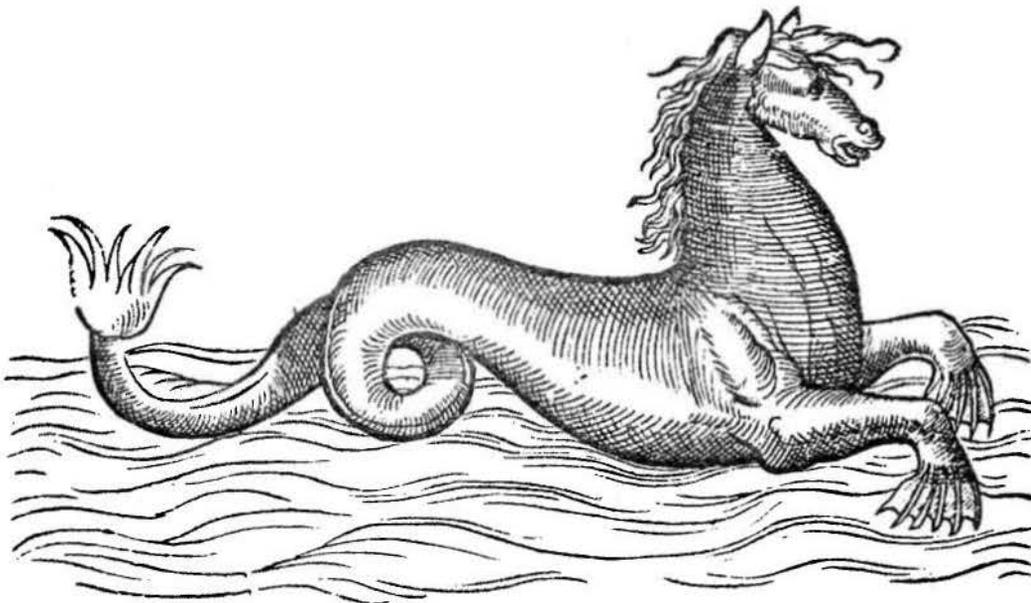
1. Definition of astrology. We used "astrology" because none of the alternatives (cosmobiology, biometeorology, etc.) are widely recognized or sufficient. Also it is surely obvious that any scientific work on astrology would consider relevant phenomena irrespective of their labels. Mr. Gauquelin is correct in pointing out that even astrologers differ in their definition of astrology. For this reason we were careful to define not astrology but astrology-as-used-in-the-book. Hence even the unhappiest reader will know what to expect.
2. "Obviously Geoffrey Dean believes in astrology." Astrology should be a matter of demonstration, not belief. I (Dean) certainly believe that enough can be demonstrated to merit further attention. When I see a correspondence between a person and his chart I can appreciate how easy it is to believe. At the same time I recognize how most (if not all) of it could be due to gullibility, universal validity, bias in self-assessment, halo effects, coincidence, and so on. To demonstrate the correspondence unambiguously poses great problems but I have been working on it with some success. The results are promising and will be published in due course.
3. Too much importance is given to work of little value. This is discussed in our response to Professor Eysenck.
4. Vernon Clark. Mr. Gauquelin's quote that "the results clearly indicate strong apparent support for astrology" is correct, but the key word is apparent, and when discussing these results (page 554) we say "In fact however this conclusion is not justified" because "they have not demonstrated that astrology works but only that astrologers work". In other words it could all be due to psi effects. Hence we share his scepticism.

Re Mr. Gauquelin's numbered points: (1) The details given in Clark's papers give no grounds for doubting his orthodox qualifications and practice. (2) The 50 astrologers are not identified for the same reason that participants in any blind trial are not identified, be it in psychology or chemistry. (3) We were not aware of the French study which he kindly sent us. It describes a repeat of Clark's first test in which 10 birth charts had to be matched in two groups of five to 10 descriptions of occupation. Unfortunately the number of participants is not given, nor full details of the results, nor a statistical analysis (which in this case is complicated). Hence the alleged failure and disappointment cannot be verified. The deficiencies which Colombet remarks upon were rectified in Clark's other two tests, but these were not repeated by the French group.

RESPONSES TO MR. DEAN:

FROM GEOFFREY DEAN:

Malcolm Dean brings to this review symposium the typical media journalist's preoccupation with sensation at the expense of objectivity and balance. Thus he begins with a condemnation of the scientific community because some scientists condemn popular astrology without knowing or investigating serious astrology, whereas serious astrologers also condemn popular astrology and provide time, talent and funds to gather together their varied literature (i.e., to compile RA). From his account



you would never know that some scientists (e.g., Cooper, Eysenck) do investigate serious astrology, that some serious astrologers (e.g., Elliott, Jayne) support popular astrology, and that the astrologers who were effectively responsible for RA (i.e., Dean and Mather) are primarily scientists. Also he describes astrology as "an extremely ancient branch of science," as if this alone is sufficient to elevate it beyond criticism, when in fact the kind of astrology being discussed is not even a century old and is certainly not a science.

His main points together with my response are as follows, and are in the order that he presents them:

He comments that much of the final draft was done by me (which is true) but implies that nobody else had a say. Also he says the book was done "in a very short period" and implies it was hurried. Both implications are incorrect. In fact RA occupied 11,000 hours of my time, 2000 hours of Arthur Mather's, and about 700 hours of the other collaborators', and every word was vetted by at least several people. It is curious that Arthur Mather's massive participation is not even mentioned, especially as much of the final shape of RA is due to him.

He criticizes RA for being negative as if it were our fault that astrologers have largely failed to make up with anything positive. He implies that we should address ourselves to making silk purses out of sow's ears, or to not telling Flat Earthers that the world is round. Presumably credibility counts for little. Also, although negativity is one of his main complaints, no examples are presented, and we have only his word for it. Indeed this cavalier attitude is evident throughout. Clearly it will not do, especially as it is the very attitude he condemns in others. He should present the evidence or hold his peace.

He says one leading scientist who examined the book characterized its style as "repellent." But until we are told who the leading scientist was, or are given definite examples of the style, this comment remains meaningless. And what about other verdicts such as the one in the book catalogue of the US distributor (Para Research) which is that the style is "stunningly clear"?

Harmonics. It is true that harmonics is given prominence (for example zodiacal harmonics has 16 pages), but this is simply on merit: the concept deserves prominence. He implies that we are not critical of harmonics and hence are biased in its favor, but this is not true: for example, of those 16 pages, 13 are

devoted to studies which show that the alleged harmonic effects are spurious. So how can it be a "rave review"?

He complains that astrologers are criticized for lack of precision while Erlewine is criticized for being too precise. Here the context is vital but has been conveniently ignored. Astrologers are criticized for being generally sloppy and ignorant of objective techniques. Erlewine is criticized for being precise to 0.0001 degree when the quantity involved (the maximum permissible inexactitude in an angle) is not defined even to the nearest degree, which is rather like giving the distance between New York and San Francisco to the nearest inch when the actual routes vary by many miles.

He says that RA attempts "to come to a conclusion on each topic." This is incorrect. It is true that some conclusions are attempted, but more usually the "conclusion" is that no conclusion is possible owing to lack of evidence. We do not make conclusions unless they are justified.

He cautions against accepting these conclusions. And with this critical philosophy I certainly agree. It is for this reason that we cite as much data as possible, backed by full references, so that the reader can see clearly how any conclusions are reached. In other words the readers are given every chance to draw their own conclusions. A fairer reviewer might have pointed out that this attitude is hardly compatible with one dedicated to bias, negativity and misquotation.

He says that many authors feel they have been misquoted etc. But how many is "many," and is what they feel actually true? Innuendo may be the media journalist's stock-in-trade, but it has no place in what purports to be a serious review. Similarly he says that the reaction of certain authors known to him has been strongly against entering into discussion with us. Since well over 1000 authors are cited, and we specifically invite their comments (page 9), his comments are meaningless until we know how many authors are involved and why they do not enter into discussion. After all, the ball is in their court. Any why should the size of the gauntlet thrown down deny proper analysis and discussion? Is it that astrologers are generally unwilling to be proved wrong?

He says that RA contains no reference to the work of Eysenck. Reference to pages 122, 124, 125 and 131 will show this to be untrue. Also he refers to replications of Eysenck's work as if all were favorable to astrology. But the latest (by Veno and Pamment at the University of Queensland involving 692 subjects) is not. (Should we now ignore it because it is "negative"?)

He says that RA should be considered retrospective, implying that we pretend it is not--a curious view since the period covered is clearly stated as 1900-1976 in the book's subtitle. And aren't all books, by definition, retrospective?

His comments on the exclusion of humanistic/symbolic/intuitional astrology etc., are covered in our response to Mr. Rudhyar.

He judges that, because symbolism etc., is excluded, the book is "certainly of little use or interest to the majority of serious astrologers, apart from its bibliography." Apart from the fact that RA does not have a bibliography (it has only references), and that symbolism is discussed at length, the same argument largely applies to his PHENOMENA. (If symbolism is so marvellous, why are the pages of PHENOMENA not flooded with it? Could it be that the same problems have defeated us both?)

He sees RA as a tactical error. He suggests that it would be better as a simple compendium sans critique, and that this would force authors to quote relevant sources, which implies that the critical element somehow obviates this persuasion. But for years there have been hundreds of uncritical books readily available for authors to quote. That they are usually not quoted indicates that, whatever RA could have been, it would remain yet another book for astrologers to ignore.

Finally he says that, because RA's conclusions are often negative, they will be largely ignored. Since most astrologers have successfully ignored Gauquelin's results on angular/cadent strength for over 20 years, Malcolm Dean is probably right. His is apt comment about the credibility of astrologers in general.

In summary, he has used his media time to attack the CSICP and to air various unsupported views well-seasoned with misstatements and innuendoes. His comments on the possible effects of RA on the astrological and scientific communities are provocative, and it will be interesting to compare them with what actually happens. But those who were looking forward to his inimical incisive comments on what actually lies between the two covers will be disappointed. This could have been the first definitive review by an astrologer. As it is, we are still waiting.

FROM ARTHUR MATHER:

Malcolm Dean's review will probably put the book (RA) into better perspective, for the readership of this journal, than of the other reviews. His reaction is basically emotional; and, in common with other astrologers, he criticizes RA outwith the context in which it was written. The book makes it quite clear firstly that we are in the business of scientific assessment, and secondly that we set out to consider the science of astrology - that is the information content of the birthchart.

The net result of our assessment is that several areas of traditional astrology appear to be valid, or at least strongly merit further investigation (most prominent amongst these being planetary characteristics, aspects and angles). On the other hand with many areas - particularly signs, rulerships and houses - the evidence is against their validity. The response to this from some quarters has been outrage, often bordering on hysteria. (Nevertheless it is heartening that there are very many individuals who appreciate what has been achieved.) Our real sin of course is that we failed to support the status quo. Had there not been this difference, RA would have been hailed as (in M. Dean's own words on first receiving a copy) "the most important book ever written in astrology." This is borne out by studying astrologers' reaction to the work of Gauquelin. Paradoxically, insofar as positive results are obtained, it is hailed as a scientific validation of astrology; but insofar as it conflicts with traditional astrology it is held to be undiscriminating, naive and (most damningly!) statistical.

Considering M. Dean's position in detail, he acknowledges us to be scientific but having erred on the side of negativity (due to overwhelming pressure of time on G. Dean), and permeating the book with a strong personal bias. In reality the neutral observer only has to read one or two randomly selected articles from the astrological press to determine where the true bias lies. The basic currency of RA is facts and facts in themselves do not permit bias. If quantities of relevant material had been omitted we would have been guilty of selection, but this charge has not been made. As it is the only guilt can lie in the emphasis put on the facts - and here of course the reader can judge for himself as all the facts (as opposed to interpretations) are given. (We would not expect the more serious astrologers to automatically accept the conclusions in RA.) The facts, often won with much difficulty, were incorporated into one of three successive drafts of the

book. The relevant sections of these drafts were normally very widely circulated amongst the 50 collaborators for comment and correction. A particular point was made of getting authors to check sections on their own work. Any reluctance to comment now is presumably because they have been left without a leg to stand on.

It is true that the amount of attention given to different areas having a similar degree of reliability varies--but this is fundamentally a literature review and quantities of material written on different topics can vary a great deal. This aside to say that the topic of harmonics has received "rave review" treatment is simply not true. Probably 80 to 90% of the claims made for harmonics research have been dismissed as not significant, and most of the rest is felt to be somewhat shaky. On the question of emphasis there is one case which we now feel was dismissed rather cursorily (the work of Erlewine) although concrete results were absent. (In this case G. Dean's feedback process with contemporary authors was frustrated due to an accidental failure in communications.)

As for cutting off positive research avenues - in the first place how can this be so if RA is being ignored? And in the second place for each avenue RA does not recommend pursuing, it positively recommends at least one other. After all any sensible research strategy should tackle the more promising research avenues first.

It is possible as M. Dean claims that a critic could be left with an overall negative impression of traditional astrology from the book if he glossed over those parts he did not agree with. However he could not cite RA as disproving astrology without the material being there for a valid rejoinder. The comment about RA being out of date cannot be taken seriously either. No fresh evidence has emerged since RA went to press which makes any material difference to the conclusions. The work of Eysenck and Mayo showing the sixth and third harmonics in the zodiacal circle is in fact included - but this does not prove signs as traditionally conceived. The failure of the CSICP to disprove Gauquelin's Mars effect makes no difference as RA has (subject to corroboration) accepted the Mars effect. The argument on symbolism reflects the prevailing "fog" in astrological thought. If symbolism is truly the essence of astrology then it is all a theoretical structure (a mental construct), and we, like Abell and Agassi, would consider it unworthy of study. If however astrology has an observational basis (as M. Dean implies),

and the symbolism has developed as a non-verbal aid to understanding, then more concrete concepts need to be substituted for the symbols to permit a quantitative assessment. (After all astrologers have always done this themselves in practice.) M. Dean even appears to be criticising the book for not being irrational - we plead guilty. If RA really is "of little use or interest to the majority of serious astrologers" then this simply shows what ostriches they are. (They may shut their eyes, but it will not disappear.) However, even of those who have been critical, many use sections of RA for teaching purposes, so perhaps M. Dean is being unduly pessimistic on our account. Buckminster Fuller may well find that attacking the "current materialistic scientific model" stimulates his creativity (good luck to him), but it has in essence been around for a long time and will not cease to exist because irrationalists want it to. The initial assumptions of scientists and astrologers were not so very different until fairly recently. Some astrologers however, fearing that science was catching up with them, have backtracked very rapidly, creating a smokescreen of symbolism, inner reality, holistic understanding, etc. The inspiration for much of this stems from the popular culture of the sixties, which itself owed much to psychedelic experience. Whether this has any validity is not our present concern, which is external reality and concrete facts. We recognise that the practising astrologer can use the chart on an intuitive level (like a crystal ball) and achieve useful results. What we would dispute in this is that the correct chart has to be used.

M. Dean regrets that RA will make little or no impact in the scientific world and has (essentially through narrow-mindedness) lost a glorious chance with the astrological world. He has missed the whole point of the exercise by saying that a simple compendium of the important literature would have been preferable. (Readers will note the difference between this and Eysenck's position.) What the astrological world needed was a good dose of critical thought - not to be simply presented with every extravagant and mistaken claim of the previous three-quarters of a century. The claim that astrologers can be (or could have been) made progressively more scientific if treated with enough patience and understanding is incompatible with the spirit of his review - he is all too ready to reject the "current scientific paradigm." It is possible that, in the wake of RA, a compendium might be useful - if astrologers really do feel so strongly about bias and misrepresentation. In this case we will be happy to cooperate with M. Dean in the production of such a volume, especially if the result would be to thaw astrologers out of their supposedly frozen silence on RA. Otherwise let posterity be the judge of who or what is irrelevant.

TOWARDS A RATIONAL THEORY OF SUPERSTITION

JOSEPH AGASSI

The editor of Zetetic Scholar has challenged me to check the claims of current astrology to scientific status, by the lights of Sir Karl Popper's criterion of demarcation of science or by any other criterion. I accepted and agreed to review Recent Advances in Natal Astrology: A Critical Review, 1900-1976, compiled by Geoffrey Dean, assisted by Arthur Mather, both astrologers, prepared under the aegis of the Astrological Association and published by it in England in 1977 as a semi-official publication. The challenge of reading nearly 600 compact pages with snippets of history, astronomy, astrology, statistics, and many many other odds and ends, while finding little connections and no substance in it, was too much for me: reading it properly would be more boring and would take more than the few days I already spent reading it. If the editor rejects this review on this ground I will abide by his decision.

Another, more general challenge, was thrown to all rationalists by Paul Feyerabend, the Arch Advocate of Academic Unreason. He says we condemn superstition without really knowing much about it. And he condemns both science and rationalism very aggressively, but at least not out of ignorance. I split his challenge to two. First, the matter of condemnation. I regret all condemnation, rationalistic and irrationalistic alike. The Philosophy of Science Association accepted my proposal to have a session on the great superstitious thinker Velikovsky, simply because he was condemned by the academic establishment or its representatives. And many of us found the condemnation uncalled for; let there be no condemnation of folly, be folly rationalism or irrationalism, since the freedom of thought is, as Voltaire said, the freedom to think foolishly, considering that no one was ever against the freedom to think wisely. The second part concerns knowledge. I need not be much schooled in what I consider superstition, since I have no wish to condemn it and may ignorantly regret what I consider people's waste of time on it, perhaps also suggest that it raises false hopes and so may be undesirable. By the same token I have no objection to Feyerabend's view of science and of rationalism as a waste and as a false hope: I only object to his aggressiveness and promise to try to be gentler myself - with astrology or any other superstition-by-my-lights.

1. The Current Theory of Superstition

Superstitions abound, even in the most civilized and scientific circles. The great Niels Bohr used horseshoes for good-luck charms, and when challenged and asked whether he believed in it, he smiled wryly and said he had it on good authority, it helps even when not believed at all. No joke, this.

The current - obviously false - theory explaining superstition, its prevalence, persistence, and attraction, is due to Sir Francis Bacon in the first quarter of the seventeenth century. The mind is lazy

and mental inertia reinforces what one has learned. Society ridicules people who change their minds as admitted failures, and so there is an incentive for intellectual stubbornness - especially to teachers, more so to teachers ambitious enough to create schools of thoughts. Moreover, when one sees the world through the spectacle of one's superstition, one sees the facts distorted by it and thus in agreement with it: superstition is always confirmed by evidence. For example, evidence for the power of vows for sacrifices to Poseidon made when caught in a storm is written on the columns of the temples to Poseidon by the lucky survivors who came to pay their vows. Those who do not survive simply fail to come and record the disagreement between fact and theory.

Bacon concluded two rules: first, do not select evidence; second, begin with facts, not with hypotheses, and let the facts speak for themselves. Bacon's theory was modified by Dr. William Whewell in the second quarter of the nineteenth century. Begin with a hypothesis, he said contrary to Bacon, but not with any hypothesis: it has to be explanatory. And do select facts, facts that should test the hypothesis, probably refute it, but other wise validate it - once and for all. This once and for all clause was suspended when Einstein superseded Newton, despite the high degree of validation of Newtonian mechanics, perhaps the highest ever.

Both Bacon and Whewell, between them, rule the field: almost every scientist and almost every philosopher is likely to follow one or both of them. They share the corollary they refused to endorse, namely that religion is nothing but superstition for the gullible masses. This corollary was endorsed by Kant and by Laplace, and more openly by Ludwig Feuerbach, by Heinrich Heine - who invented the phrases "opium for the masses" and "the dying God" - by Marx, Nietzsche, and others.

What is the allure of superstition? Why is it such strong and addictive opium? Bacon's theory still is the only contender; people see the world through the distorting mirror of their superstition and cannot see their own superstition. It was Einstein who said, we do not notice our prejudices any more than a fish notices the water in which it swims. This had led to two responses, Sir Karl Popper's and Maurice Ginsberg's. Popper said, it is pseudo-scientific to marshal validating facts, as both astrology and psychoanalysis do; it is scientific to subject one's hypotheses to severe tests or honestly admit them to be metaphysical and private. Ginsburg said it is no good to offer a refutation of a prejudice, since the prejudiced will casually explain it away - as Bacon already knew, incidentally. Rather, it should be attacked simultaneously from all sides.

Clearly Ginsberg's strategy of simultaneous multiple attacks often fails. I can report from experience that a prejudice, when attacked from all directions, simply takes each attack separately and painstakingly slowly. The present volume goes even further: the authors of Recent Advances in Natal Astrology accept each criticism, denounce the gullible whole-heartedly, and remain firm and steadfast in their prejudice in favor of their superstition. Hence Ginsberg's view is false, and he can admit the refutation or defend his view and make it a

superstition. Nor is astrology alone in this respect. I had three papers of mine rejected by many editors of learned journals because in them I had followed Ginsberg's strategy, regardless of the degree of success or failure of my venture. Prejudice is common in the academic world.

Everybody knows that superstitious people are often confused, that confusion helps the clinger to a prejudice in his clinging to it despite the barrage of criticism. The reason for this is fairly obvious but hardly ever articulated. Here it is: when we reject a criticism with an excuse, our excuse can be true, and at times we can bring the evidence that it is; the more excuses, the more doubtful our view may be, but doubtful all views are anyway; yet when we make many excuses, they may contradict each other. Taking criticisms slowly, one by one, may obscure the contradiction, and confusion may cloud it.

The present volume shows no more confusion than the average, and makes no excuses. Hence it invites a new attitude from all those who hold the standard views against astrology. They may be ignorant or choose to remain ignorant of the present volume; they may find in it a defense of some superstitious opinions or another so as to have an excuse for dismissing it as a volume of prejudices in favor of superstitious views, similar to so many; they may judge the hopes it expresses in developing scientific astrology not superstitious at all, on the basis of the current doctrine that superstitions are silly opinions and not silly hopes; and they can view the book as superstitious. I need not say I think only the last two options are open. And they are both revolutionary. The first option is revolutionary in a minor way, admitting this proastrological volume as rational and scientific on account of its staunch abidance by the negative verdict of the facts. The second option is revolutionary in a major way, since rationality to date rested on theory assessment, not on program-assessment. It was the late Imre Lakatos who noticed that such a shift is a revolution in methodology, and, were he himself not so foolishly high-handed, or had he lived longer, he might have contributed something to the debate. For my part I do not see the need for this volume in order to support the shift: I had recommended this shift before I saw this volume and do so now independently of my views on what I consider superstitious. And already when discussing research programs, I have observed that a superstition can be programmatic. But this volume makes me think all sophisticated superstition is programmatic, and this has alerted me to my lack of comprehensive view about the matter. I will try to make amends towards the end of this paper. Here I will conclude with a critique of the central error of the traditional view of it.

It is clear, I think, that studies of prejudice and superstition - even of madness! - were corollaries to, or at best parts of, studies of science and rationality: so to speak, the one was the negative of the other. Two ill-effects are immediate results of this idea: first, all defects of the theory of reason were at once reflected on the theory of unreason; second, it left no neutral territory and also inverted commendation into condemnation. Let me explain.

Erroneous theories of reason are less harmful than erroneous

theories of unreason, and for an obvious asymmetry. I deem myself reasonable and so I either fail to notice my lack of agreement with current theory of rationality or will be skeptical of it; I do not deem myself unreasonable and I do not know how to view your unreasonable thinking, and so I must apply to you my theory of unreason. This way theories of unreason may be more used and so their errors may lead to ill effects. An example is the theory of rationality as proof. It excludes all reasonable disagreement, i.e., it makes all disagreement unreasonable: either both contestants are in the wrong or one; but competing views cannot both be proven, i.e., rational. This theory never applied to reasonable disagreement. No one ever said, the dispute between the field theory and the action-at-a-distance theory is a proof that at least one party is unreasonable. Yet this theory is the basis of Kraepelin's theory of paranoia that is still the official doctrine; see Y. Fried and J. Agassi, Paranoia: A Study in Diagnosis, 1976, for as much detail as is reasonably needed. And, of course, it is easier to declare a person prejudiced or superstitious than to certify him paranoiac. So much so, that historians of science have traditionally judged as prejudiced all mediaeval scientists, all eighteenth century chemists except for Lavoisier and his followers, all those who rejected Mendel and/or Darwin, and more. And many historians of science today still hold this view. For detail see my 1963 Towards an Historiography of Science, reprinted by Wesleyan University Press in 1967.

Moreover, identifying superstition and prejudice with unreason makes every decision either wisdom or folly, with no neutrality possible. Repeatedly rationalist philosophers reaffirmed that sentiment is no matter for either wisdom or folly; and sentiment means not only matters of taste, not even only of art, but also love and friendship. Now why need they repeatedly state that friendship is no matter for wisdom or folly? Because friendship includes trust, trust calls for rational test, and hence friendship becomes a matter of a foolish (superstitious) or of wise (rational) trust, i.e., a matter not outside the domain of reason. Attempts to apply the classical theory of reason and folly systematically makes one easily suffer persecution mania. In other words, it is a part of rational procedure to be critical, even at times optimally critical, but it is unreasonable to be very critical in matters of love, friendship, in relations with neighbors who differ from us in so many ways, and in attitudes towards the innocent and even the superstitious.

2. Impressions About The Volume At Hand.

Those who know no astrology will have troubles even with the meanings of the signs of the Zodiac. Everybody can guess that two waves, like the signs for rough equality, stand for Aquarius; but I know few academics who may be able to identify Virgo. Aquarius, we are told, is probably the sign of the flooding of the Nile in spring. Virgo is the Egyptian corn goddess, and her sign is in fall due to the harvest. Anyone who wants the history of the signs is advised to look elsewhere. But, details apart, surely the signs of the Zodiac are twelve mainly because of the twelve lunar periods making a solar one. Well, not quite. But then the signs do not divide the sky into equal parts either. Nor is it a good solution to have twelve equal parts in the sky and allot

to each of them 365:12 days: the solar year is not a round number of days, the months are not of equal length, and the precession of the equinox and geography make any solution too limited to space and time anyway. Besides, when does a Zodiac month start? At the sun-set, mid-night, or sun-rise appearance of the east, mid-point, or west of the sign in question? Finally, is the division along the northern hemisphere's tropical zodiac or the sidereal, since both differ from the constellational zodiac? The ancient astrologers could do a lot of juggling; modern astrologers have to juggle even more. The authors frankly admit all this, offer no guiding principle, but are optimistic, as if the difficulties they present are marginal. They are initial and insurmountable. A non-starter.

I do not mind the juggling as such, let me hasten to explain. Most writers condemn the readiness to juggle. Science, they say, is precise. When precision enters, arbitrary hunches leave and science flourishes. I reject this view. I think both science and superstition mix precision with the juggling of data, and quite ad hoc, i.e., in order to obtain a good fit. There is a theory of degrees of precision, and it is largely a new theory: Kepler still made all his calculation as precise as he could, and he had not the slightest idea of approximation. Newton was the first to develop a theory of approximation, but only marginally. Today we can correlate the limits of precision of our instruments and say when precision of one instrument is insufficient, adequate, or excessive. It depends, as always, on the purpose at hand: calendar calculations must always be precise to a day, and the degree of precision of astronomical data for calendar making is determined by the number of years ahead that the calendar projects. If we decide to reform the calendar every thousand years, then there is a limit to the required precision. In applied science, theory gives us the limits of precision: theory is always continuous, facts always discrete; it is like in motion pictures: since the threshold of vision is about one tenth of a second, we need more than ten frames per second to see motion, and we started with fourteen; the current twenty-four is the peak of comfort; but if we want to see the flutter of the wing of a humming bird, we can easily calculate the number of frames required in its speed photography: there are about sixty flutters per second, yet we can see one if it is slowed to more than two tenths of a second for each move. The rest is arithmetic. Take a horoscope. If it says, those born under the sign of Aquarius are likely to be sailors, all you need is to know your birthday in order to know whether you are likely to be a sailor. If it says, those born under Mars are likely to be soldiers, it depends what "under Mars" means. If it means the night Mars appears here or there, then you only need to know the night - and whether the day comes before or after the night in question for those born during the day. If it means when Mars arises or sets or is in conjunction with a constellation or with another planet, better knowledge is required. As for tests of theories, the more precise a theory the easier it is to refute it, but also the greater is the required precision of measurement. Yet the claim that Aquarians tend to be sailors was refuted in all possible versions of this claim. For my part, I do not need a refutation: I never suspected that a sailor's career is in any way dependent on the flooding of the Nile. The authors, however, know the refutation yet still say, on p. 47, opening of discussion of the Zodiene, "It is not impossible that both Zodiacs are valid," meaning "both tropical and

sidereal." Not the refuted theory, of course, but some other theory. Which theory? In a book so thick and full of detail there is no answer; not even a hint.

This book is still worse. It takes it for granted that somehow our luck, prescribed by the moments of our births, has some connection or another - they confess ignorance - with the very constellations that have reached us from the depth of the ancient eastern mediterranean basin. But we know now for sure that there are no constellations. This fact is known to the authors, as can easily be shown by citing certain passages in this volume. Yet the authors never take cognizance of the fact that there are no constellations, much less to raise the possibility of doing away with astrology. The apparent constellations keep changing, their apparent movement depends on time and place, and we do not quite know how to adjust our data. Did you know that in the Far East the sidereal system is used and in the Near East the tropical? Did you know that the number of signs was not universally twelve? Did you know that harmony - between a planet and a constellation - reflects Ptolemy's intuitions about how the four Aristotelean chemical elements relate to the symbolisms of the heavens? Did you know that planets were related to metals - the golden sun and the silvery moon have survived into ordinary language, as did quick-silvery Mercury. But is not iron for Mars, the God of War, better replaceable by Plutonium? Or should Plutonium go to Pluto?

But I shudder at my own proposal: the authors of the present volume take seriously every suggestion, even one which Arthur Koestler has made as a joke against astrology (p. 22)! No matter how harsh is their critique of the gullible on point of empirical evidence, they are so incredibly gullible just on point where facts are wanting and theory is fuzzy. They also at times try for maximally available precision, though from time to time they are sensitive to the limits of useful precision for any given purpose.

Can fuzzy theory be made precise? The authors notice how hopeless the situation is, yet they do not even ask whether it is not better to abort the whole project. For example, we can make a system increasingly precise in different directions. But how can we decide between competing possibilities in the absence of any guidelines, a priori or empirical? We can do things at random: we can take professions, e.g. seamanship; personal traits, e.g. courage; dispositions, e.g. homosexuality; or positions, e.g. being mentioned in the social register; or anything else. If more seamen or brave men or gay people or famous ones are born on a given month, day, time of day, conjunction, aspect, house, eclipse, harmony, if any correlation be found, then, perhaps, who knows. Yet correlations can certainly be found, since astrologers are now using computers and given enough trials, then, by Bernoulli! every correlation may be hit upon; and with longer trials some will even be repeated!

But our astrologers keep themselves busy. They not only check any suggested correlation; they also record problems to be solved. First, there are so many suggested and tested correlations, and these should be checked, catalogued, and surveyed. Why? Each step of scientific advancement raises new difficulties for astrology, ever since the Copernican Revolution. Some characteristics of astrology may be

operationally indifferent to the Revolution, especially if they only relate to the observed starry heavens alone. Others are not. For example the astrologically important modes are very different for the heliocentric system from the geocentric one. (A mode is a point at which a planet passes the ecliptic; which ecliptic?) Eclipses and conjunctions and comets and even planets multiply, not to mention metals, chemical elements, personal traits, and events at large to correlate to large heavenly events. (For, the authors do not restrict themselves, as promised, to natal astrology.) No less than all this, mathematics plays havoc. Analytic geometry has destroyed houses; spherical trigonometry and statistics destroy aspects, already viewed by Kepler as too coarse and modified by him to keep up with the times (p. 278). Kepler, incidentally, though he shared to the last much of the outlook of the present authors, was at least cleverer in deciding all traditional astrology utterly worthless and better ignored. He wanted a fresh start. They do not let go and seek kernels of truth even in refuted theories.

3. The Theory of Theory-Assessment.

The theory of scientific method traditionally has concerned itself with methods, i.e. ways, of generating theories that may be properly deemed scientific. This concern is now almost entirely ignored, and most students in the field concern themselves not with assessing methods but with assessing theories or hypotheses: given a hypothesis or a theory, i.e. any set of statements, we can decide its status vis-a-vis the body of existing factual information, and decide that it is or is not scientific. The question is, how? What is the criterion of theory assessment, of demarcating the scientific from the unscientific theory; that is. We may narrow our discussion, if we want, to theories that claim scientific status, thereby being able to seek demarcation not between science and non-science but between science and pseudo-science. In this case demarcation entails commendation or denunciation, of course.

How, then, do we demarcate?

Classically, it was said, science is empirically validated. The idea is still immensely popular, perhaps more than astrology, since both defenders and opponents of astrology appeal to validation. For my part I think validation is a bad joke: we decide what to do with information, not information decides for us. We weigh evidence, dismiss it, endorse it, cross-examine it, defer to it, abuse it, etc., etc. But tradition is diametrically opposed to all this. Validation, the standard traditionally endorsed by the scientific community, was justificationism; indeed a variant of justificationism called these days inductivism. It says, thou shalt not advance a view unless empirically validated! Taken seriously this standard is either opposed by all church doctrines, Christian and Communist and logical Positivist, and what-have-you, or else it precludes them from rational discourse. It makes any church doctrine necessarily a dogma one way or another.

Sir Karl Popper, and more so his leading disciple W.W. Bartley, had a different proposal to make: never-mind the ground for having

advanced a doctrine, they suggested, as long as we make it criticizable and are willing to accept criticism. Thus Popper expressly declared Carnap a rationalist thinker because he presented his superstitious view (of the language of science, but this is irrelevant here) repeated in new criticizable variants and always accepted criticism and tried again.

Perhaps I am unfair to Popper. I do not know. I can discern at least three conflicting variants of his view. First is his view of science as conjectures and refutations which, to repeat, seems to me to vindicate the astrology in the present volume as well as other superstitions, such as logical positivism, as rational and even scientific. Second is his view of science as satisfactory explanations, where one of the requirements that explanation must satisfy, if not the only one, is that of independent testability. And then, neither astrology nor positivism explain and so are not rational, much less scientific. Third is his view of science not as explanations, not as mere conjectures and attempted refutations, but as conjectures, corroborations, and refutations. This third view makes it an urgent problem for all lovers of astrology (or positivism) to ask, is there any corroboration to it? The answer still is, no, but one can go on looking. The volume under consideration, which seems to me so obviously silly, fully conforms to Popper's third criterion, and its participants all agree that astrology has not come of age, is not corroborated, and so is not scientific, at least as yet. Can one try to render an unscientific theory scientific? Certainly, as long as one lives one can try. Is there a criterion for the rationality of the attempt? Does Popper discuss this question? Perhaps, yes. For, I have not exhausted here the variants of Popper's views which I have distinguished, nor will I do so here; but to answer my question I should add the fourth: Popper's latest view of science is that science is problem solving, perhaps also solving in a manner open to critical empirical assessment.

Does the present volume present a problem? Yes. It is very simple: why does astrology persist? Is there any objective reason to it? Since if validated its persistence will thereby be both explained and justified; the sifting of all the vast body of evidence in search for a validation makes sense, then!

It is here that I start feeling most uncomfortable. I, too, wish to know the immense power of astrology. And I do not see how exactly this can be done, since I cannot for the life of me compare Ptolemy's astrology with Kepler's, Kepler's with this book's authors, nor either with the gullible believers of so much nonsense. How can one view such a long tradition as one phenomenon? Even the same attitude, when exhibited in the Middle Ages and today, seems so altered.

Before discussing the power of astrology as superstition I may make two concessions to the current theory of theory assessment. The first concession is that the persistence of a view may, at first approximation, be grounded in either gullibility or facts. Contrary to the current theory of theory assessment, both these categories, gullibility and facticity, are complex, and do overlap; nevertheless, I concede: we can begin by sorting views into these two categories. Second, I concede that from time to time we may wish to test even what we consider the

most absurd hypothesis. I deem extra sensory perceptions and spiritualism superstitions sillier than astrology, yet I cannot dismiss all tests to them, not the tests by Michael Faraday and W. Gray Walter. But, I should say, be careful: do not waste your life on these matters. It is much cheaper to dismiss all superstitions on the ground of the theory of program assessment.

Yet people do cling to superstitions. And including academics, and respected and respectable thinkers. I mean not only opportunists like Feyerabend and racists like the great logician Gottlob Frege, but also people like the great social anthropologist Sir Edward Evans-Pritchard whose life-work is a stupendous, monumental defense of the rationality of magic. Until we take such matters seriously we will superficially dismiss superstition, its prevalence and perseverance, as mere gullibility. This will not do. Even if we oppose superstition - and I would rather not - we better properly assess its strength.

4. Irrationalism

I do now wish to express some moral quandary. I have met people who are tolerant towards religion, though they deem it superstitious, yet intolerant towards astrology or psychoanalysis or Marxism. I have also met people whose attitude is the reverse. The problem of the limits of toleration is traditionally focused on the toleration of the intolerant. We refuse to allow the law to assess damage caused by teaching as open to redress, and for obvious reasons, yet we put enormous efforts into education which subtly expresses our fears and hopes about what we want to transmit to the young and how. That there is damage in miseducation is unquestionable, yet we want to tolerate the miseducated even as educators. If I were not to confess my ignorance on such issues, if I were not to admit regret but toleration at the sight of the younger generation going to what I deem superstition, waste of time, loss of ability to think straight, and the like, if I were not to admit that regret requires some action whereas tolerance when not being invited requires inaction, then I would be less than honest. The word "Towards" in the title of the present essay is there to this end: I cannot begin to tackle all problems the theory of superstition ought to solve.

But let me go over familiar territory so as to start the venture of taking superstitions seriously.

I was once present at a lecture that Evans-Pritchard gave to an august group of social scientists about the rationality of the magical system of the Sudanese tribe known as the Zande or Azande, on whom he wrote his classical Magic and Witchcraft Among the Azande in 1937. He made two or three important points. First, that science is in principle unable to explain coincidence yet magic does. I find this a terribly important point: all superstitions I know of, whether perennial or ephemeral or even private, have to do with coincidence; so has Arthur Koestler seen matters in his The Roots of Coincidence, a book on extra-sensory perceptions, that is surprisingly and uncomfortably similar to Recent Advances in Natal Astrology.

Evans-Pritchard's second point was that the Zande are not uncritical and they regularly test their oracles, etc. The question is, why are they uncritical of magic? That is, they are not critical of magic as such or as a whole; why? His third point answers this: they cannot; their whole thinking apparatus is occupied by magic; magic is the whole of their transcendental apparatus: without it they have no thinking apparatus.

This is Evans-Pritchard's irrationalism, and he applied it to his own Catholic religion (he was Anglo-Catholic, though), just as Michael Polanyi did (he was a Jew, though). An increasing number of thinkers, let it be observed, is joining this irrationalist trend first reported by Bertrand Russell in his Religion and Science of 1930.

One of the reasons for the spread of irrationalism is the silly attitude so many rationalists exhibit towards it. Thus, the lecture of Evans-Pritchard that I heard was welcomed and cheered as a mere funny story. Evans-Pritchard left the lecture hall with tears in his eyes. "They will never take seriously what I say," he privately complained; "they will never believe that I mean it seriously." Of course, many did, but they were all on his side. Can there be a dialogue between rationalists and irrationalists? Both these parties seem to imply the negative answer, and if so then they are both refuted: it is possible and even happens, and even fruitfully so. Moreover, it seems to me that the irrationalists both find defects in rationalism and are also quick to apply their findings in their teaching. Thus, not only astrologers, but other superstitious groups these days recruit new believers by training them to see the world through the most general abstract principles of their sects while encouraging doubts - even strong doubts - both against specific superstition and against rationalism as such. This last ingredient, the skepticism against rationalism as such, is absent from the present volume, since it appears to be rationalistic. But, of course, the clinging to the unspecified astrological program, perhaps under the guise that any decent empirical research is laudable, can only make sense as an irrationalism - an irrationalism only reinforced by the rejection of all evidence in favor of astrology. Yet, irrationalism, is refuted by its applicability equally well to superstition, to science, religion, and to anything else.

I think Evans-Pritchard knew the difference between innocent superstition and dogmatic superstition, as well as between superstitious dogmas and other dogmas. He simply ignored the differences, even though he noticed that science pays little attention to coincidence whereas superstition centers around it. He also stressed the importance of the fact that magic - as any other superstition - injects meaning into events rather than explains them. And herein, I think, lies the peculiarity of superstition. Opponents call it wishful thinking; sympathizers call it yearning. I am a frank sympathizer here. I cannot here go into matters and conjure the atmosphere of sympathy with superstition, with the great yearning of the soul, with the quest for meaning. Let me refer the interested reader to the greatest student of superstition, Arthur Edward Waite, who wrote in the first quarter of this century. I should refer the reader especially to his book on the holy grail, especially his conclusion, where he confesses he too is in quest for a new mass. (He was a Catholic.) I think it is clear that regrettably too many

scientists are stuffed-shirted professors who insist that they are content with the state of their knowledge. They are, for some, addicted to the inverted-superstition which denies the very possibility of yearning, which denies the very existence of the puzzle, of the riddle of the universe, of the riddle of life or of the meaning of life or of history. This inverted superstition is called logical positivism; and like all inversion it is parasitic, and need not be taken more seriously than its host. Perhaps the most popular criticism of logical positivism is the claim that it survives all criticisms of it by remaining a hope, a program for research. Perhaps it is the fact that science grew out of Medieval superstition - because more than any other superstition, it was noted in the yearning for the revival of the old glory, which ended up as the Renaissance of culture and of science. Not accidentally, superstition was more common, not less, in circles that make the Renaissance of science, with numerology and astrology reigning supreme, where numerology was known as the kabbalah but later was translated to the label of Pythagoreanism since Pythagoras learned numerology from Moses in person. The long line of Pythagoreans begins with the followers of the Christian kabbalist Giovanni Pico della Mirandola whose "Oration on the Dignity of Man" is viewed by many as the opening of the Renaissance of science because it distinguishes between the evil magic that Moses forbade and the good magic that we now view as the roots of the scientific tradition. It is hard to say who was the last Pythagorean; perhaps Newton. Yet whatever Pythagoreanism meant for Galileo, he opened his first great book with an admission of guilt and the promise to clear the Pythagorean house of all mumbo-jumbo.

The irrationalism of Evans-Pritchard and of Michael Polanyi and others still keeps the mumbo-jumbo out as inferior stuff; of all contemporary academic writers on the subject, Feyerabend is perhaps alone in inviting all mumbo-jumbo, old and new, and a priori legitimizes it all. Irrationalists pretend that any system is a priori as good as any other, since thinking begins and ends within a system. Bacon believed we can live without a system; Kant believed we all must share one system; modern irrationalists recommend all systems equally. What we need is some discrimination. And we begin with the ousting of superstition as too elusive to be any good, though as historically of great importance.

5. Superstition, the Illusive.

The demarcation Popper offers is between science and pseudo-science, or between science and non-science, but in either case between two sets of hypotheses. The demarcation, I must say, gets involved. It is the proposal to view as scientific refutable hypotheses, i.e. ones that may be refuted by some conceivable observation reports if these were made as true observation reports. For example, when we say, as we do, all living things include proteins, then we can easily imagine a living thing with no protein in it, with another element, perhaps a rare earth, replacing all nitrogen in all amino-acids in a living cell without killing it. No report of such a cell having been observed has ever been made. But we can imagine it made, and so the hypothesis is scientific. Yet Popper notices at once something missing here: we can change the meaning of our hypothesis to exclude any refutation of it in its present sense as soon as such a refutation is made. He therefore requires, in addition, that we

avoid such moves, that such moves are juggling and deprive the hypothesis of its scientific status. This means that Popper discusses only hypotheses of fixed meanings: once you change the meaning of a hypothesis you simply have another hypothesis, and that new one may be unscientific though it looks the same as the old scientific one.

We must, then, notice that language does not always behave like that. Not only do sentences often shift their meanings gradually and surreptitiously. At times they do so against our will and despite the care we take to keep meanings constant. But at times we like meanings to shift; at times we make half a sentence in the hope that our interlocutors hear a whole sentence, and different ones hear different completions. It sounds clever, but it is silly, and it is a subtle mode of cheating. When people are willing to project meanings into the world - as Evans-Pritchard says magically-minded people do - then it may be profitable and easy to tell them suggestions of sentences that they hear as sentences, often quite naturally and effortlessly as if what was said was clear! These are the gullible, and there are the ways to cheat them.

If so, then the attacks of the present volume on the gullible are misplaced. The astrological hypotheses it refutes are clear-cut; its authors themselves believe in suggestions, not in clear-cut hypotheses; what they refute, then, is pseudo-astrology, and what they believe in is superstition. Popper's claim that astrology is a pseudo-science is false, then, though he follows a rationalist tradition. Astrology tantalizes; it is no meat for thought, but it is no stones instead of bread either; it is the smell of bread, perhaps promising bread, perhaps arguing that bread is there somewhere, perhaps something else. Yet just as the superstitious puts meaning into magic, so he puts meaning into astrology. Sir James Frazer, of The Golden Bough fame, said that magic is elusive and hence essentially pseudo-science; he too was fusing pseudo-science with superstition.

I do wish to correct all this. Magic as pre-science cannot be pseudo-science since it preceded the advent of science. Also a pre-science may juggle honestly even today, as a heuristic device. All in all, we must be careful to distinguish the innocent from the regressive and the parasitic. Now as parasitic on science a superstition may become pseudo-scientific when it is fixed. As regressive it is a part of a more complex affair, and is certainly very shifty.

Let me stress that I speak now of the regressive, not of the innocent. The regressive is infantile, fixational. In infants, as in lower animals, we can see what Watson considered the three elementary feelings. Put in modern terms they are, first, clinging or fixation, which is quite common even in adult animals - in times of stress, great needs, etc.; second, aggression or fight or hostility; and third, fear or anxiety, or flight. Of course, combining these three, or any two of them, may be terrible and cause much suffering. And, of course, the great stress or need may come from an objective condition and from a state of conflict, internal or external - we need not go into it; we can just notice that anxiety and regression go well together. As Konrad Lorenz notices, anxiety causes confusion in all animals. Humans can, even under stress, make efforts to stick to a point, focus their attention on it, and be clear within it. When they see nothing but what is at the center of their

attention, they are prone to some sense of magic, like children ignorant of their environment, and like readers of some science fiction literature. The situation is conducive, however, not only to superstition, but also to ambulatory paranoia, to fanaticism, or to mere dogmatism. There are variants, but not given to grading on a scale since diverse factors are present here. It seems to me to be something - I am juggling the facts and do not wish to sound precise since I am not - something like this. Dogmatists are clear in a center of attention, but do not notice a periphery, cling to the center of their attention and aggressively so. Fanatics are confused about the center of their attention, but likewise do not notice a periphery and are more aggressive than clinging. Ambulatory paranoiacs are clear about the center of their attention, increasingly confused about the periphery, cling, and very anxiously so. The superstitious are either totally confused or else they allow themselves to be superstitious only in the periphery of their thinking; and act more out of anxiety than out of clinging. We can see that it is far from clear, by this characterization, whether ambulatory paranoia is better than regressive superstition, in that its fixation is clear; or worse in that it is fixated. It is a fact, however, that many ambulatory paranoiacs are superstitious - indeed the literature defines paranoia as an erroneous *idée fixe* logically sustained. But I cannot take encouragement from the literature as it confuses all four cases of regression I have distinguished and confuses, perhaps deliberately, all cases with the regressive cases. I cannot abide: I cannot see the dogmatism of Newton as regressive; or the naive superstition of Giovanni Pico delle Mirandola and of Kepler; or the paranoia of so many prophets, lay and religious; or the fanaticism of ever so many leaders, military or political or even religious. Notice also that in my characterization I have omitted prejudice altogether. It is too coarse a category to take seriously. The interested reader may be referred to the various works on prejudice by Michael Banton.

The confusions mentioned in the previous paragraph are all rationalistic. The confusion of the irrationalists is almost identical and is illustrated by Evans-Pritchard's identification of the Zande magic system, the Communist dogma, Nuer religion, and even his own Catholicism, all as self-imposed intellectual systems. This idea is called by Karl Popper the myth of the framework. Frameworks, he says, can be and at times are composed and discussed and replaced.

The most relevant role of frameworks for the present study - not in general - is their use as generating research programs, good and bad ones or, as Lakatos called them, progressive and regressive or degenerative. What makes a program progressive? Popper does not really discuss this question and Lakatos says it can be discussed only in retrospect, not in prospect. The owl of Minerva, he reminds us, flies only at dusk. The present volume illustrates the weakness of Lakatos' answer: when is it dusk? Has the sun of astrology sunk four centuries ago as I think, or not yet arisen as the volume's authors say? How do we decide? Shall we cast a horoscope for astrology, perhaps?

As I say, astrology has a glorious past which cannot be revived; and no one has said why it should have a future. There are more interesting programs and perhaps more promising. The astrology presented in the present volume is not a set of claims that can be refuted, it is a

silly hope that has not even been stated and that one has to be very superstitious to take them at all seriously.

As a coda to my suggested typology of regressive superstition and other regressions, I wish to relate these to irrationalism in general, to Zande magic, to the present volume, and to Feyerabend - all the odd ideas mentioned here.

Now irrationalism need not be regressive; superstition likewise need not be regressive: the former can be a sophisticated confusion, the latter a naive one. Zande magic, I think, is regressive, but I may be in error; I do not think Evans-Pritchard has given us enough material. That dogmatism can be either - sophisticated or naive - as well as regressive, is also clear. The only thing hard to characterize is Feyerabend's support of superstition. The aspect of his writings that does not ring true is the enormous aggressiveness it contains, the fanatic hostility to rationalism. But then Feyerabend may be an opportunist - a mere pseudo-superstitious. He claims in his debate with me that he is honest, since he goes to quacks and acupuncturists for his health. I simply do not believe this, and I do not think it suffices as a bona fide; he is, I think, a phony phony. So we have a phony astrology that is phony because refuted yet not hopeless, and a phony superstitious because he is aggressive.

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RESPONSE TO PROF. AGASSI FROM GEOFFREY DEAN & ARTHUR MATHER:

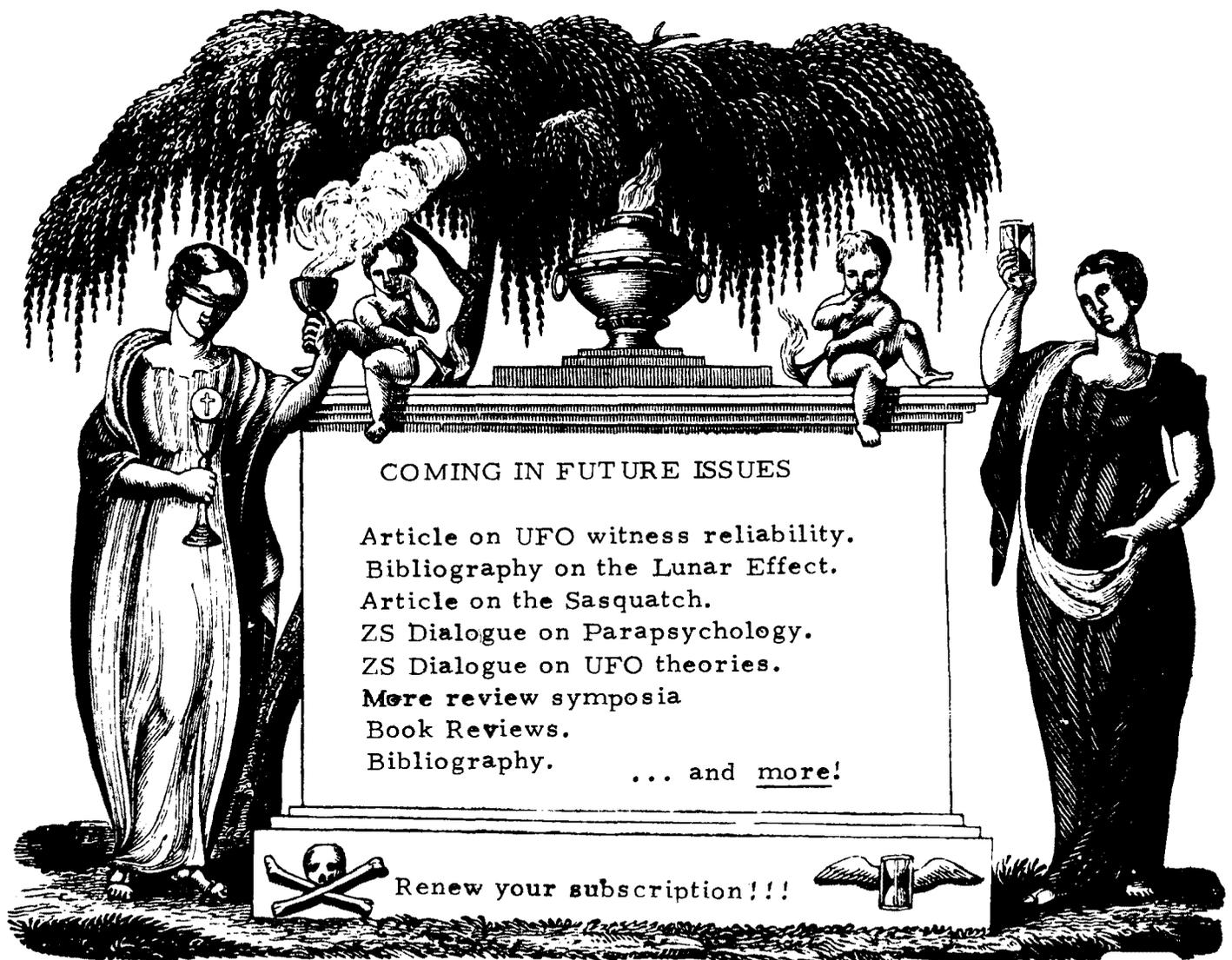
On Prof. Agassi's main topic of superstition we are not competent to comment. Hence we shall confine our response to those parts of his essay which involve our book, namely scattered snippets comprising about 14% of the total.

Briefly, his verdict is that our book is no more confused than average but has few connections, no substance, is boring, incredibly gullible, and obviously silly. However we may note the following:

1. Agassi rejects astrology out of hand as superstitious nonsense. This is bad news for those expecting a balanced opinion. Also it is hardly in keeping with this journal's policy of objectivity and fairness. But as he himself says, prejudice is common in the academic world.
2. He admits not having read the book properly. Hence his comments on what it does or does not contain are worthless.
3. He has a poor understanding of modern astrology. For example, he states erroneously that signs do not divide the sky equally, he gives constellations an importance they have not held for millenia, he is ignorant of modern attitudes towards precession, and he implies erroneously that there is more than one ecliptic. But presumably such incompetence does not matter, since it is all nonsense anyway.

4. He fails to recognize that we are merely reviewing the astrological literature, not generating a set of claims susceptible to refutation. Thus he claims we are prejudiced in favour of astrology (interestingly many astrologers claim the opposite), and condemns us for taking every crazy superstitious idea seriously. In his view it would be better to abort the whole thing. Under the circumstances his comments are meaningless.

According to Agassi, the original editorial request was not for a review but for a check on the claims of astrology to scientific status in the light of Recent Advances. This was unfortunate as Recent Advances does not claim scientific status for astrology. A scrutiny of the claims of astrology using modern scientific techniques is not the same thing. Unless of course this very scrutiny - along with the proposals for further work - constitutes a science by some definitions. In this case, Professor Agassi seems to be saying, the effort was wasted on a body of superstition. In other words the body of science may grow out from itself but not make a leap into the dark. In reply we would simply say that the amount of effort put into (wasted on?) astrology worldwide today surely justifies a much smaller effort spent on assessing the underlying assumptions.



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-- Thanks to I.W. Kelly

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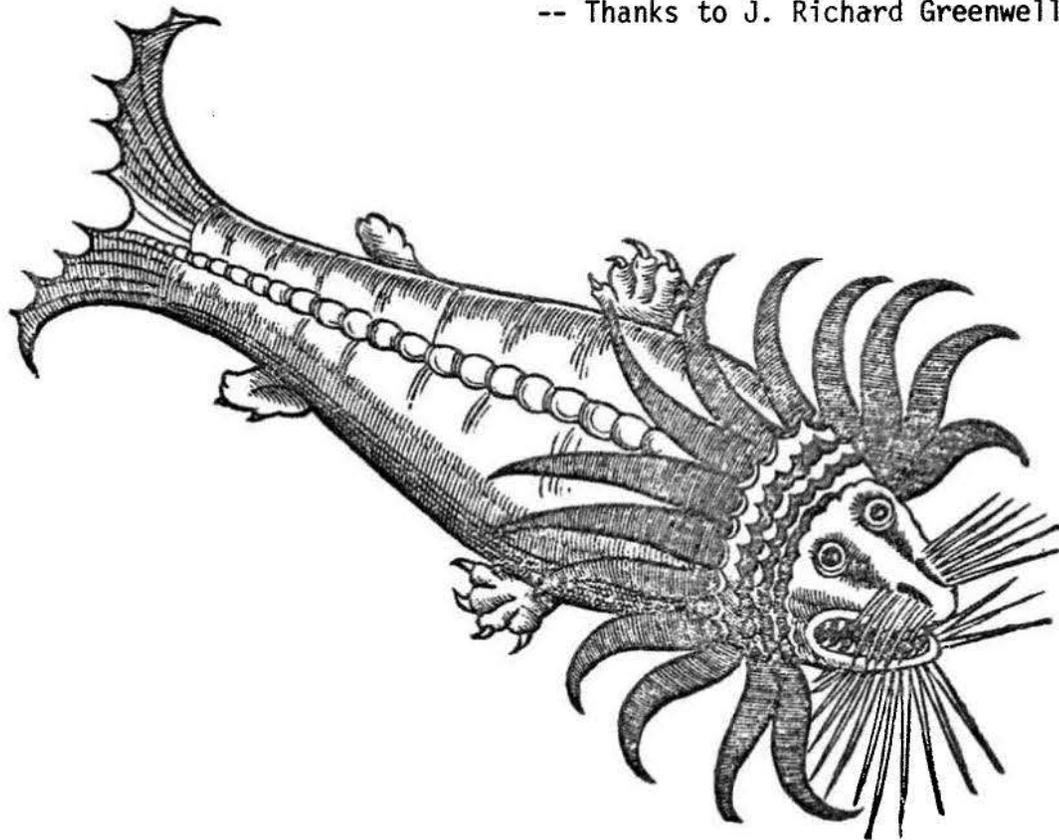
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-- Thanks to J. Richard Greenwell





BOOK REVIEWS



Astrology Disproved. By Lawrence E. Jerome. Buffalo, New York:
Prometheus Books, 1977. 233 pages. \$14.95.

Reviewed by Anthony Standen

I take it that many readers of the Zetetic Scholar will applaud the appearance of a book attacking astrology. But Mr. Jerome's book, although aiming in the right direction, contains a number of mistakes on the way. Most of these fall into three groups, as follows.

First, Mr. Jerome does not seem quite clear as to what astrology is. He shows some most interesting pictures of prehistoric bones, with notches carved on them, evidently recording the number of days that elapse between the phases of the moon. Now, if there were any evidence that these men said "People born during this time are strong, successful hunters; those born in some other time are weaklings," or anything resembling that-- that would have been astrology. But they were simply trying to find the number of days in a (lunar) month, and fit this into a (solar) year--a tricky job. This is astronomy, of a very practical kind, and as I see it has nothing whatever to do with astrology. So why mention it?

I suspect that not all readers of ZS will go along with me in my next objection, which springs from the fact that I do not share Mr. Jerome's anticlericalism. When he speaks of St. Augustine as [p.38] "head of the Eastern portion of the Holy Roman Church," that is utter nonsense. Speaking of the Renaissance, he says, "Both the public and certain Christian theologians were demanding that a measure of free will be allotted to man" [p. 106]. He does not seem to realize that in Catholic philosophy, from the days of Augustine right through to the present, a very important point has been the free will of man. Indeed, it was the Church's insistence of human free will that led Augustine to his conversion from his earlier Manichaeism.

Lastly, I have a criticism of Jerome's whole approach to the disproof of astrology. He first establishes the proposition "Astrology is a form of magic." He then goes on "and magic is false." If this proposition is granted, then it follows that astrology is false. But is it? Perhaps we do live in a world where magic comes in, once in a great while. The possibility of occasional very queer things happening cannot be ruled out so easily. As I see it, the refutation of astrology has to be done far more carefully than just that. To call it "magic," and then say "and magic is no good," is trying to prove something by means of a word.

And I find other mistakes along the way. Altogether, I find myself in disagreement with Mr. Jerome in almost everything -- except the bottom line!

The Lunar Effect: Biological Tides and Human Emotions. By A.R. Lieber.
New York: Anchor Press, 1978. 168 + x pages. \$7.95.

Reviewed by Ivan W. Kelly

In The Lunar Effect, Arnold Lieber, a psychiatrist, presents a case for serious consideration of the belief that the moon exerts a strong influence on human activities. Lieber provides empirical research to support the notion of a lunar effect on human behavior and suggests a theory of "biological tides" to account for this influence. I contend that neither the research nor the theory support Lieber's claims.

Lieber's empirical case for a lunar effect is weak. Lieber seems to have combed the scientific literature for any remote information to make his theory appear plausible. Every new theory or discovery in science that looks even remotely relevant, no matter how tentative are seized upon, including those that may be far removed from any direct causal impact on human behavior. Consequently, Lieber brings in studies dealing with meteorological and biometeorological variables and even includes hearsay and gossip about policemen and ambulance drivers who purportedly expect to see "more action" during full moon nights. However, the basic question is What is the status of the scientific evidence for a lunar influence on human behavior? Two articles have recently reviewed the central studies in this area: Campbell and Beets, "Lunacy and the Moon," Psychological Bulletin, 1978, 85, 5, 1123-1127 and Cooke and Coles, "The Concept of Lunacy: a review," Psychological Reports, 1978, 42, 891-897. These articles examine a total of 20 studies dealing with lunar phase and several classes of behavior including psychiatric hospital admission, suicides, and homicides. Both articles conclude that there is no unequivocal support for belief in a relationship between lunar phase and human behavior. In fact, Campbell and Beets argue that the few positive findings are examples of a Type I statistical error. It is also noteworthy that there are few reports of successful replication in the majority of studies that report a significant relationship.

Just as Lieber's empirical case for a lunar effect is weak so is his "biological tide" theory. Lieber argues that (1) The human body is constituted of 80% water and 20% solids; (2) The human body is like the earth in that both are causally affected by the physical forces of neighboring planetary systems; (3) The tides of the earth's oceans are caused by the gravitational pull of the moon in conjunction with other forces as well; therefore, (4) It is likely that "high" human biological activity and "low" human biological activity are caused by the gravitational pull of the moon in conjunction with other physical forces; (5) Human action is determined largely by certain inner biological processes; therefore, (6) It is likely that instances of "high" and "low" human activity are governed to some extent by "high" and "low" biological tides and that what influences both terrestrial and human biological tides are gravitational forces

of nearby planetary bodies.

Lieber's analogy fails in that the analogy is too weak to warrant the inference he wants to draw. The fact that the surface of the earth and the human body contain the same proportion of solids and liquids seems a poor reason for talking of tides in the human body. It is like saying "Dogs have four legs; cats have four legs; therefore, cats are dogs." The fact that dogs and cats have four legs in common is a poor reason for identifying dogs and cats. The same can be said of Lieber's argument--in both cases the analogy isn't sufficiently strong enough to warrant the inference.

Lieber is also assuming that like causes always have like effects. This isn't necessarily so. The presence of other factors may create a different effect.

Finally, the establishment of a correlation between terrestrial ocean tides and human activity (even if it existed) would not justify the belief that whatever causes the ocean tides also causes tides in the human body. For example, suppose corns on my toes are correlated with the presence of forest fires in the interior of California and never occur when fires are not present; it does not follow that whatever causes forest fires also causes my corns.

The cover of Lieber's book proclaims: "Ours is a killer moon, and it is a lover's moon. As a link between man and the cosmos, the Moon affects everything we do." Lieber's book does not justify this claim.

BOOKS BRIEFLY NOTED*

* Listing here does not preclude later full review.

Boschke, F. L. (translated by Jan van Heurck), The Unexplained: The Unknown World in Which We Live. N. Y.: Pocket Books, 1978. 273 pp. \$1.95 paperback. A remarkable survey of scientific questions and history concentrating on many speculative areas and bringing in numerous anomalies of interest to ZS readers.

Briggs, Katherine M., The Vanishing People: A Study of Traditional Fairy Beliefs. London: B. T. Batsford, 1978. N. Y.: Pantheon, 1978. 218 pp. \$8.95. An outstanding study in folklore by the expert in this area. Highly recommended. Fascinating reading.

Bronowski, Jacob, Magic, Science, and Civilization. N. Y.: Columbia University Press, 1978. 88 pp. \$6.95. An enlightening but some what eccentric presentation (especially in his definition of magic) which contains many nuggets of wisdom but strung together somewhat oddly at times. Worth reading but rather overpriced.

Chitrabhanu, Gurudev Shree (edited by Leonard M. Marks), Realize What You Are: The Dynamics of Jain Meditation. N. Y. : Dodd, Mead & Co., 1978. 125 pp. \$7.95. A general introduction to the basic techniques and philosophy of Jain meditation which includes elements of "positive thinking" along with its concern for a transcendent experience. Nicely done and remarkably modern viewpoint presented.

Davis, Mikol, and Earl Lane, Rainbows of Life: The Promise of Kirlian Photography. N. Y. : Harper & Row, 1978. 95 pp. \$6.95, paperback. Pretty pictures; terrible science. Along with ignoring the negative evidence, the authors accept everything from Odic Force to N-Rays.

de Mille, Richard, Castaneda's Journey, Second Edition, Revised. Santa Barbara; Cal. : Capra Press, 1978. 201+xiv pp. \$4.95, paperback. This new edition of the highly acclaimed debunking of the ethnographic claims made for the don Juan narratives updates and corrects the record on what many believe to be the biggest anthropological hoax since the Piltdown man. Those unconvinced by de Mille's arguments in the first edition may be persuaded by his account of recent events which go far towards reinforcing his case for fraud. Highly recommended.

Ebon, Martin, editor, The Signet Handbook of Parapsychology. N. Y. : New American Library, 1978. 519 pp. \$1.95, paperback. A bargain compendium of over two dozen significant review articles by parapsychologists. Though very much a pro-parapsychology anthology, many of these reprinted papers are indispensable for anyone seriously interested in psi claims; and this may be the best economy package of such papers available. Martin Ebon does his usual excellent editorial job in introducing the pieces, and his own article of historical review is particularly valuable. Highly recommended.

Edelson, Edward, Who Goes There? The Search for Intelligent Life in the Universe. Garden City, N. Y. : Doubleday, 1979. 196 pp. \$8.95. A very useful survey of the SETI (Search for Extraterrestrial Intelligence) movement. Its coverage of Ufology (chapter 7) seeks to be fair but is uncritical of the opponents of Ufology, which distorts unduly, particularly in its characterization of Ufology as a cohesive perspective when it is not so. Nonetheless, an impressive and generally well informed presentation that is an excellent introduction to the current state of the SETI.

Edmonds, L. G., D. D. Home: The Man Who Talked to Ghosts. N. Y. : Thomas Nelson, 1978. 192 pp. \$7.95. A new and welcome biography of the great 19th century medium whose feats were never exposed and still remain a mystery. Edmonds is also a conjuring

historian and, though he remains noncommittal on the question of Home's actual powers, offers normal means by which Home might have accomplished his effects. Though reference to key Home scholars like Eric J. Dingwall are surprisingly missing, this new look at Home is a welcome addition for Edmond's speculations on Home's methods.

Ellis, David J., The Mediumship of the Tape Recorder. West Sussex, England: D. J. Ellis (Fernwood Nightingales; West Chiltington; PULBOROUGH; West Sussex RH20 2OT), 1978. 161 pp. \$5.00, paperback. This is a detailed examination of the (Jürgensen, Raudive) phenomenon of voice extras on tape recordings, done as part of the Perrott-Warrick Studentship at Trinity College, Cambridge. The thoroughness of Mr. Ellis is most admirable and his conclusions are basically negative. Anyone interested in this topic should most certainly read this valuable commentary. Highly recommended.

Fontenrose, Joseph, The Delphic Oracle: Its Responses and Operations With a Catalogue of Responses. Berkeley, Cal.: University of California Press, 1978. 494 pp. \$25.00. A remarkable historical study which largely debunks some of the mythological impressions many of us have of the "cold readings" which came from the oracle at Delphi. An outstanding piece of historical scholarship.

Foster, Robert, The Complete Guide to Middle Earth: From the Hobbit to the Silmarillion. N. Y.: Ballantine Books, 1978. 575 pp. \$10.00. The updated guide to the Tolkein world of fantasy;

Froud, Brian, and Alan Lee (edited by David Larkin), Faeries. N. Y.: Harry N. Abrams, 1978. No pagination, 185 illustrations. \$17.50. A gorgeously illustrated work, unfortunately sometimes inaccurate in its depictions and with text heavily reliant upon (but without credit given to) Katherine Briggs. Since I am an extreme admirer of Froud's artwork, it more than compensates for its errors.

Gauquelin, Michel, Dreams and Illusions of Astrology. Buffalo, N. Y.: Prometheus Press, 1979. 158 pp. \$14.95. A translation of one of Gauquelin's earlier books debunking traditional astrology and outlining his own work in astrobiology. Highly recommended.

Gawr, Rhuddlwm, editor, Pagan/Occult/New Age Directory. Atlanta, Ga.: Pagan Grove Press (P. O. Box 49285; Briarcliff Station; Atlanta, GA 30359), 1978. 37 pp. \$3.95, paperback. A very useful listing of addresses of and information on the many major pagan religious and related organizations. A supplement to the directory has also been announced.

- Halifax, Joan, Shamanic Voices: A Survey of Visionary Narratives. N. Y.: E. P. Dutton, 1979. 269 pp. \$6.95, paperback. A general survey essay with excellent edited first-person narratives. A very well done and useful collection.
- Johnson, Fred, H., The Anatomy of Hallucinations. Chicago: Nelson-Hall, 1978. 240 pp. \$16.95. An excellent new survey on the psychology and neurophysiology of hallucinations, particularly auditory hallucinations. Highly recommended.
- Khalsa, Parmatama Singh, editor, Spiritual Community Guide #4. San Rafael, Cal.: Spiritual Community Publications (Box 1080; San Rafael, CA 94902), 1978. 256 pp. \$5.95, paperback. This book's subtitle is "The New Consciousness Source Book," and it represents a massive compilation of names, addresses, and descriptions of just about every reasonably-sized spiritual development center and humanistic psychology organization around. Handsomely printed and with an introduction by Buckminster Fuller, this little directory may be the most complete now available. Recommended.
- Leith, Harry, Bibliography of Books and Articles on the Relationship between Science and Pseudoscience, 2nd edition. Toronto, Ontario: York University, Dept. of Natural Sciences, Atkinson College, 1978. 61 pp. \$3.45, paperback. An extremely useful bibliography put together by Professor Leith, presumably for use by students in his classes, since citations for articles do not include names of the authors and many of the books are referenced by library call number rather than by publisher. Should prove invaluable for anyone similarly involved in teaching courses on the science-pseudoscience demarcation problem and includes excellent references to the literature in many specialized paranormal areas.
- Mauskopf, Seymour H., editor, The Reception of Unconventional Science. Boulder, Col.: Westview Press/American Association for the Advancement of Science, 1979. (AAS Selected Symposium #25.) 137 pp. \$13.25. The papers presented at the 1977 AAAS panel of the same title. Includes papers on acausal physics, continental drift theory, acupuncture, parapsychology and statistics, and the general issues involved in radical scientific change. Highly recommended.
- Nash, Carroll B., Science of Psi: ESP and PK. Springfield, Ill.: Charles C. Thomas, 1978. 299 pp. \$16.95. A first-rate survey of parapsychology intended as a text book. As a survey of the pro-parapsychological literature, the book is excellent since it synthesizes a tremendous amount of material. Though much too uncritical, as evidenced by chapters on "questionable" phenomena that seem no more or less questionable than many of the topics presumably not considered questionable,

the book represents a massive and generally well-done effort. The book needs more balance--perhaps a solid chapter on the major reasons for skepticism towards psi phenomena--but I still have no hesitation in recommending the book for use in courses dealing with parapsychology and we should be grateful for this very useful survey and text.

Neely, James, and Eric Tarkington (edited by Malcolm Dean), Ephemeris of Chiron 1890-2000. Toronto, Ontario: Phenomena Publications, (Box 6299, Toronto A), 1978. 112+xvi pp. \$10.00, paperback. Many astrologers will welcome this remarkable product of computer technology.

Ridpath, Ian, Messages from the Stars: Communication and Contact with Extraterrestrial Life. N. Y.: Harper and Row, 1978. 241 pp. \$10.00. A very readable book the first parts of which discuss modern space exploration and contact attempts and the third part of which concentrates on attempting to debunk various claims of past and present contact. Like Edelson's volume (which I generally preferred to this one) the debunking chapters of this book are useful but far from definitive.

Robbins, Rossell Hope, Witchcraft: An Introduction to the Literature of Witchcraft. Milwood, N. Y.: KTO Press, 1978. 121 pp. \$7.95, paperback. A volume introducing the catalog of the superb witchcraft collection of Andrew White at Cornell University. The author of the respected Encyclopedia of Witchcraft, Professor Robbins has worked for many years with this collection and was certainly the person best suited to author this highly important survey. Those of us who used the White Collection prior to its being cataloged will especially appreciate the labors involved in this great effort.

Roberts, Nancy, with photographs by Bruce Roberts, Appalachian Ghosts. Garden City, N. Y.: Doubleday, 1978. 78 pp. \$5.95. Essentially a book of ghost stories for young readers.

Robertson, R. Macdonald (edited by Jeremy Bruce-Watt), Selected Highland Folktales. North Pomfret, Vt.: David and Charles, 1977. 212 pp. \$9.95. A very welcome reprinting of the 1961 classic collection of Scottish folklore orally gathered by a popular collector.

Rogo, D. Scott, and Raymond Bayless, Phone Calls from the Dead. Englewood Cliffs, N. J.: Prentice-Hall, 1979. 172 pp. \$8.95. A rather fascinating book for its very presence quite aside from its content. The authors do not claim to present a careful scientific case for what I have come to think of as OOB-scene phone calls. Fun reading but if the case ever gets validated for these phenomena, I hate to think about the long-distance phone bills (at least the calls don't seem to coming "collect").

- Sebald, Hans, Witchcraft: The Heritage of a Heresy. N. Y. : Elsevier 1978. 262 pp. \$18.95,hardbound; \$7.95, paperback. A wonderful ethnographic and historical work on the witchcraft practiced in Franconian Switzerland. Though I found myself in disagreement with the author's conclusions about occultism and its relation to science in the modern world(chapter 16), the work as a whole represents excellent scholarship and consistently intelligent perspective on a complex topic. Recommended.
- Stover, Leon E., and Bruce Kraig, Stonehenge: The Indo-European Heritage. Chicago: Nelson-Hall, 1978. 212 pp. \$19.95, hardbound; \$9.95, paperback. A very informative and fascinating account of the history and meaning of stonehenge from an anthropological and historical perspective. Speculative in many parts but well anchored in the existing research, this book is a welcome addition to the already large literature on standing stones and their meanings.
- Van Over, Raymond, Total Meditation: Mind Control Techniques for a Small Planet in Space. N. Y. : Collier Books, 1978. 207 pp. \$4.95, paperback. A very compact but well done survey of meditation and its major techniques including Zen, Yoga, T'ai Chi, and TM among others. An excellent self-instruction volume including 26 exercises for the meditator.
- Vogt, Douglas, and Gary Sultan, Reality Revealed: The Theory of Multidimensional Reality. San Jose, Cal. : Vector Associates, 1977. 466 pp. \$12.95. A very ambitious attempt to create a kind of unified field theory to account for an incredible amount of real and alleged phenomena and apparently much indebted to the earlier ideas of Nikola Tesla. A controversial theory expounded to explain facts which are themselves controversial as to their existence, e. g. , psi, kirlian aura, mythological-historical events, etc. An imaginative voyage through a very wide ocean of ideas.
- Walker, D. P., Spiritual and Demonic Magic: From Ficino to Campanella. Notre Dame, Ind. : University of Notre Dame Press, 1975. 244 pp. \$5.95, paperback. An important historical work showing a deep influence on Renaissance Neoplatonism from magical and mystical traditions. A major and influential reinterpretation.
- Wambach, Helen, Reliving Past Lives: The Evidence Under Hypnosis. N. Y. : Harper & Row, 1978. 200 pp. \$8.95. A very unconvincing and scientifically naive but nonetheless entertaining attempt to validate reincarnation through a report on past-life recall by over 1000 subjects under hypnosis.
- Weigle, Marta, Brothers of Light; Brothers of Blood; The Penitentes

of the Southwest. Albuquerque, N. M.: University of New Mexico Press, 1976. 300 pp. \$12.95. Probably the definitive anthropological-historical study of the Brothers of Our Father Jesus, an Hispanic lay organization often noted for its Lenten processions that included cross-bearing and self-flagellation and a general impression of fanaticism and secrecy. Includes a superb bibliographic essay and a thorough documentation. An excellent scholarly work.

White, John, editor, Kundalini, Evolution and Enlightenment. Garden City, N. Y.: Doubleday Anchor Press, 1979. 479 pp. \$4.50 paperback. A large compendium of essays on just about every connection possible between Kundalini and other matters including UFOs, stigmata, neurochemistry, etc. I think the book probably contains more than I am likely to ever want to know about Kundalini.

Wilson, Colin, editor, Dark Dimensions: A Celebration of the Occult. N. Y.: Everest House, 1977. 236 pp. \$7.95. A collection of essays by Wilson and others on Rasputin, Gurdjieff, H. P. Blavatsky, Nikola Tesla, Aleister Crowley, Sir Francis Dashwood, Uri Geller, F. A. Mesmer, and Nostradamus. A rather sensationalistic and uncritical set of accounts but well written and with some new (but poorly documented) information for those familiar with the persons discussed.

Wilson, Colin, Mysteries. N. Y.: G. P. Putnam's Sons, 1978. 676 pp. \$15.00. In many ways a sequel to the author's very popular earlier large book The Occult, this book takes off where that one left off, this time with the works of T. C. Lethbridge. Since Wilson's writings do not pretend to scientific validation for most of the extraordinary claims being made in them, one needs a broader philosophical perspective to evaluate them. Like his earlier tome, this study of "Faculty X" is frequently flawed as an act of careful scholarship, but its grand sweep and general intelligence make it delightful--and often enlightening--reading. After reading Wilson, my own reaction is that I don't believe in Faculty X, but I rather wish I did.

Wylder, Joseph, Psychic Pets: The Secret World of Animals. N. Y.: Stonehill, 1978. 163 pp. \$9.95. Another of the many books (e. g., Bill Schul's The Psychic Power of Animals and Maurice Burton's The Sixth Sense of Animals) claiming to illustrate the presence of psi in animals, this book concentrating heavily on such alleged communication between man and animal. The author is allegedly a scientist writing under a pseudonym to protect himself from the scorn of his fellows. I can see his point.

-- M. T.

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