6. THE SCIENTIFIC RECEPTION SYSTEM

by Alfred de Grazia

When a scientist writes a book of his controlled experiences, a publisher ponders its audience, and a colleague weighs its value, the special order of human relations called science is in being. Their patterns of motive and behaviour emerge from and return to the larger sphere of social behaviour. They are different from, yet the same as the general social order.

Perhaps then never can it be said that 'this could only happen in science': in a scientific sense science cannot follow laws uniquely its own. Also it would be exceedingly risky to reason that, though possessed of a basis of generally understood behaviour, science receives from somewhere a unique moral code that cannot be evaluated by general moral codes.

THE CONCEPT OF RECEPTION SYSTEM

There is, in every social order, a reception system. In the suborder of scientific behaviour, the reception system consists of the criteria whereby scientists, their beliefs, and their practices are adjudged by scientists as a community to be worthy, true and effective.

The importance of a reception system in every social order is manifest. The reception system shapes the character of new recruits to the order and therefore forms the product of the order. If the term itself is new, the reception processes in themselves are well known. Whenever a scientist concerns himself with the training methods and the curriculum of his field, or with its system of publications and the criteria for evaluating work, he contributes to the building or enforcement of the order. Political parties and mass movements, religious groups, business enterprises, bureaucracies, and a host of voluntary associations have

similar reception systems, and of course there is little difference between the natural and social sciences in this regard.

The principal elements of the reception system are doctrines and an operational formula with typical tactics of acceptance and rejection. Thus, 'truth according to empirical principles' constitutes a doctrine of the science reception system. It is generally believed that some criteria satisfying this goal must be extracted from those who contend for acceptance. The operational formula sets forth a number of methods by which behaviours are to be tested to determine the degree to which they fulfil the obligation of 'empirical truth.' And a set of tactics is employed to admit or reject offerings determined to have succeeded or failed according to the formula. For instance, a journal will return a manuscript with a polite note of refusal or fit an article meeting its criteria into its publishing schedule. Ultimately the social and scientific consequences of this reception system must be discovered and analyzed in order to pass judgment upon the system and to enable an applied science of science to revise and reform doctrines, formulae, and tactics.

Such a reception system may be postulated to operate when a person, belief, or practice is projected upon the perceptive and cognitive screen of scientists with an implicit or explicit demand for acceptance. We therefore view Dr Velikovsky, his theories, and his practices as a case relevant to the study of the reception system of science.

The interpretation of the science reception system may be facilitated by fitting its activity to assumed models. Models of social behaviour in a given setting can be numerous, since the construction of any single model depends only on the perception of a patterned dynamic of actions, and since the validity (and utility) of such models is theoretical and statistical, not absolute. The number of principal models may be reduced to one in the case of purely-motivated and purely-acted behaviour, or to several in the case of the usual complicated performance of social institutions. In the case of the scientific reception system the problem is to determine what postulated pattern or complex of motives and behaviour best accounts for what happens in most cases coming before the reception system

for consideration. What accounts for the favourable or unfavourable reception of men, beliefs and practices?

The historical sociology of science is obliged, in the long run, to provide materials and analysis in a large enough number of cases to verify empirically that one or several given models explain in great part and usefully the vast majority of relevant actions. A single case, as the one of Velikovsky, can contribute to an ultimate historical sociology of science, but cannot in itself prove the validity of the models used.

However, if there is support from materials already known to us, and from such writings as the preceding article by Livio Stecchini, we would be inclined to credit the hypothetical model with somewhat more validity than the single case would warrant per se. Moreover, in order for a rule of law to characterize the behaviour of social groups, justice has ultimately to be defined in relation to singular parties. Therefore a finding of injustice in a single case is sufficient to provide grounds for remedial action then and there, without resort to laws of averages, or the 'long run.' If a postulated model of the scientific reception system fits a case well, and is believed to be either personally unjust [1] or socially (scientifically) harmful, then the question will naturally arise whether the case should be reheard, as well as whether this condition is typical, this model is normal, and the public or social policies (rules) of scientific behaviour should be revised.

Four models appear to explain a good deal of scientific reception-system behaviour. They may be called the Rationalistic Model, the Indeterminacy Model, the Power Model, and the Dogmatic Model.

THE RATIONALISTIC RECEPTION SYSTEM

The rationalistic reception system is openly displayed by scientists in general as the 'scientific method.' It is considered in proto-thought [2] to be the exclusive determinant of admission policies to the corpus of science. Its goal is truth, enlightenment, knowledge, or just simply 'science.' It postulates a purity of science, namely that the propositions and methods of scientists are arrived at only by efficient, logico-empirical opera-

tions. Personal animosities, psychopathology, politics and other social conditions are ignored, reduced in importance, or denied a place in the scheme of science.

The rationalistic model, defender of the purity of science, requires that the 'scientific method' be pursued in validating fact and proposition. It demands control, prefers quantification, and honours prediction as marks of scientific work. It asserts that new material offered for scientific examination and appraisal will be fairly and openly dealt with, will be communicated freely to whoever may be in a position to judge its merits, and will, upon approval, convey credit to its author. It resembles the rule of law in court systems in that a set of procedures for arriving at truth are to be required of all men regardless of their degree of authority, their previous record, and the resources they command.

These are some of the doctrinal, procedural, and tactical elements in the rationalistic model. The socio-scientific consequences that are deemed valuable are 'truths,' by the operation of this process more and greater 'truths' will be discovered. The truth will be communicated. As its value becomes apparent, the truth will be used in all applied fields that are related.

Those who operate in the name of this model tend to deny a sociology of science. The concept of sociology implies that men are conditioned in their behaviour by social factors lying outside of the intellect. The scope of the psychology of science is similarly reduced, creating a constant tendency to believe in absolute realities. Furthermore, since those under the rationalistic spell claim that after all 'there is an objective method of testing reality and any reasonable person can see the truth when it is presented to him,' they tend to dismiss political problems as irrelevant, and to dismiss power as a factor in the building of the corpus of science.

In detailing the rationalistic model, some of the behaviour of scientists in the Velikovsky case that exemplify the use or non-use of the rules of the model can be described. To be noted first of all is that the model is itself used as a mode of attack upon Velikovsky. This is immediately apparent when articles and correspondence dealing with his work are examined. Perhaps

the most indignant published attacks against Velikovsky occur at the hand of Professor Cecilia Payne-Gaposchkin. She precedes them, however, with a statement of the rationalistic doctrine of science, for she says:

> In these days of loyalty oaths, scientists may congratulate themselves that they are not, as such, required to swear to anything. Nonetheless, every scientific man, every man who devotes his life sincerely to the advancement of knowledge, commits himself to certain loyalties. His loyalties are to principles, not to dogmas; to respect for evidence - all the evidence, not merely such as fulfills his expectations, respect for those formulations that embody the evidence. We who are engaged in research are not concerned in preserving the existing framework of theories. We spend our lives searching for the wherewithal to modify and supplant them. The discovery of discordant facts is cause for rejoicing, not consternation. If Velikovsky had adduced any real evidence that compelled a revision of the laws of celestial mechanics, astronomers would have accepted the facts, and the challenge, with delight. His supporters imagine that we are shaking in our shoes. This is partly true: we are shaking, but with laughter... Our critical faculties have not been developed only by dealing with cranks, for there is plenty of loose thinking and misinterpretation of evidence within the fold. The outsider might be surprised to learn how little mercy we have on, or ask from, our fellow scientists [3].

The *Scientific Monthly*, which was later incorporated into the magazine *Science*, also printed an article by a professor of philosophy that endeavoured to explain to the public the criteria that distinguish scientists from cranks. We quote the rationalistic doctrine as carried there:

> We have already said that there is hardly a scientific theory that is not questioned by some scientist of repute. This is so because science is

> unfinished business, an inquiry into the habits of nature where all the evidence is not in and where much of the evidence that is in has not been digested. Under these conditions there is room for minority opinions, some of which will, no doubt, turn out to be correct. There is a parallel here, though, with horse racing: long shots run in the races, and some will no doubt win. But a sports commentator who expected a long shot to win in almost every race would be open to suspicion. In the same way, the man who accepts one or two scientific 'long shots' is perfectly reasonable, but when a man accepts too many of them, his scientific standing becomes suspect. The crank is one who tries to force nature into his own selected pattern; the evidence of strain resulting from this practice is divergence from currently accepted views [4].

Harrison Brown, reviewing Velikovsky's work in the *Scientific American,* similarly asserts several rules of the science reception system:

> ...In the world of science the individual research worker usually subjects his results and theories to his fellow scientists for searching criticism and checking before making his results known to the public. If he is at a university he first solicits the criticisms of his local colleagues, following which he shows his results to scientists in other institutions. When he has thus satisfied himself that his results or ideas make sense, he submits a paper to a scientific journal. The paper is sent to anonymous referees for criticism, and if they judge it worth publishing it is published in that journal [5].

Earlier, writing in *The Saturday Review,* Brown had this to say about the Velikovsky hypotheses:

> ...Modern science can... marshal far more convincing evidence - evidence which possesses

> mathematical rigor as distinct from interpretations of what human beings may or may not have done, observed, or said thousands of years ago [6].

In each case, following upon or included in the doctrinal statements are assertions that Velikovsky has failed to fulfil the conditions. The doctrinal statements reveal how aware the scientific community is of the need to precede strong criticism by a credo.

In the rationalistic doctrine the rule of publication holds primary importance. It says that any would-be scientist should make known the result of his investigations, and, by inference, should have the right to publish his work. It also is expected that a scientist's work will be discussed before publication by those capable of evaluating it. These obligations were, of course, fulfilled by Dr Velikovsky. He consulted many specialists, among them the historian Pfeiffer and astronomers Adams and Motz. The book was examined carefully before publication. Macmillan held it for three years, and then was subjected to pressure from leading scientists not to publish or stop selling it after it was brought out. His work was subjected to double the regular scrutiny by experts prior to publication because of these pressures. It was read by at least six experts and emerged with a favourable verdict. His book was removed from one firm and transferred to another because of the threat to the publisher of loss of reputation and sales. Whereas the first article by Larrabee in *Harper's* was a responsible piece of journalism, and those of Atwater and Oursler were respectable presentations, a portion of the popular press distorted some of the features of his work, creating an image of it that many scientists could use to discourage other scientists from writing about the work seriously. The scientific journals would not subsequently publish articles by Velikovsky which adduced further proof of his thesis or responded to criticism.

A second canon of the rationalistic model is that *works will be read* before a judgment is passed. This promise is not always fulfilled. Yet the principle of reading offered material must be upheld lest the whole rationalistic model collapse. If the new work cannot be guaranteed some degree of expert reading it must naturally fail to make its mark. *Science* is a

communication system as well as a method of advancing truth. Several of the most severe attacks against Velikovsky can now be shown to have been made by scientists who had not read the book. Perhaps as many as half a million American have read *Worlds in Collision*. Among them are relatively few of the scientists - astronomers, geologists, paleontologists, historians - who are directly affected by the ideas treated in the book.

Reviewing is one step beyond reading. The review is necessary to pinpoint the audience of a book, to enlighten others as to its contents, and to suggest considerations of its truth or falsity. Hundreds of reviews were written of Velikovsky's book, *Worlds in Collision*. The popular reviewers tended to be favourable. The scientists were hostile. If there is such a thing as an ideal book review, whether favourable or unfavourable, it is not to be found in the story of *Worlds in Collision*. The question may be raised whether not only Velikovsky but also other scientists are subjected to the same inadequate treatment of their work and whether thereby this principle of the rationalistic model is continually being violated.

Another rule is that *theories offered should be tested*, not only by the author but his critics. This rule again turns out to be unobserved in many instances [7]. Velikovsky, whose behavior throughout the controversy was that of person committed to the rationalistic model, began to ask for tests of his theories four years prior to publication of his work. He reasonably claimed to have performed all tests within his power (the historical tests) but sought other tests requiring the use of equipment that he did not have access to. For instance, over a ten-year period he corresponded with several institutions - universities, museums, laboratories - trying to persuade someone to perform radiocarbon tests on Egyptian artifacts of the New Kingdom, without success. He also sought unsuccessfully to have the spectrogram of Venus analysed for heavy molecules of hydrocarbon. One wonders here, as in the case of other 'folk heroes,' whether a condition of accepting with grave seriousness the rationalistic doctrine is to be innocent of experience of the world wherein the doctrine operates. Velikovsky, having had no university appointment or foundation grant, was more tenacious in his adherence to the rationalistic myth than his detractors.

Honesty and fairness are cardinal tenets of the rationalistic credo. Unless scientists are willing to admit the source of their knowledge and theories, and willing to grant a fair hearing and test to ideas brought forth, they contribute to the collapse of the rationalistic reception system. The honesty of Velikovsky was frequently called into question by natural scientists, in a manner so strong and unbalanced as to constitute libel. Yet no single case of mis-stated fact was proven in any of the four books of Velikovsky, and it would be untrue to assert that his works are too vague to assail; they are, in fact, exceedingly detailed and specific.

The 'ruthless honesty' that both Gaposchkin and Brown asserted as the hallmark of science in relation to self-criticism and appraisal of new works was quite ruthless, it is true, but directed entirely at Velikovsky. The degree of honesty in the appraisal of Velikovsky's studies can be judged in some of the evidence presented in these papers.

The appraisal of works by specialists, we have said, is a necessary ingredient of the rationalistic model. And specialists were brought to bear upon the work of Velikovsky. However, it would appear that the specialists' functions in the Velikovsky case were primarily to proclaim their competence and to disperse the vulgar masses who claimed to see revelations of value in Velikovsky's writings. Instead of specialism being used as a positive weapon of analysis, it tended to be used as a negative weapon of destruction: 'Anything un-narrow must be bad.' Professor Boring wrote in an article on unorthodoxies of science that agreement by trained scientists is the critical determinant of truth [8]. His theory, itself unorthodox, and not part of the rationalistic model, was used to show why Velikovsky was wrong even by those scientists who were operating in the name of the rationalistic credo: since the specialists said Velikovsky was incorrect, he *must* be incorrect.

Open discussion is supposed to characterize the rationalistic model. The social setting provided for the discussion of Velikovsky's work were mostly arranged for and administered by hostile critics or intimidated moderators. He was excluded from discussions of his own work and, when he succeeded in

participating under a special dispensation, his words were not subsequently published. Several scientists and intellectuals who attempted his defence were silenced or sanctioned severely. I. Bernard Cohen, Professor of history of science (Harvard University), wrote sympathetically, almost enthusiastically, of Velikovsky's work in the advance summary of his address before the American Philosophical Society in April 1952, but changed his approach markedly in the published version of his address in the *Proceedings of the American Philosophical Society* (October 1952).

Radical innovation, declared Dr. Gaposchkin, is no bar to the reception of new science. This is part of her testimonial to the rationalistic reception system. More in keeping with the facts of the reception of Velikovsky by herself and the scientific order is the statement by Bernard Cohen that 'Any suggestion that scientists so dearly love truth, that they have not the slightest hesitation in jettisoning their beliefs, is a mean perversion of the facts'[9].

Nor should radicalism in method be a deterrent to the recruitment of ideas. Yet one of the glaring features of the Velikovsky case is the humanistic ignorance of natural scientists. A reading of the Velikovsky record should be part of the proceedings of any group considering the revision of curriculum for students of the natural sciences. Soon a century will have passed since the beginnings of the scientific investigation of myth, folklore, and primitive psychology. It has been many years since a theory of the unconscious has found a place in the instrumentation of social science. The science of linguistics, of symbols, of the sociology of communication, has progressed. It would appear that a more broadly educated or at least philosophically trained scientific class would have been able to perceive the relevance, validity, and unique capabilities of Velikovsky's method to key problems of natural science.

But the passage of time has relegated the natural sciences principally to hardware instrumentation. The natural scientists are still dwelling mentally in the hollow rationalistic universe of the 19th century. Indeed such a statement is unfair to the 19th century, which was far richer in mental constructions than its impoverished and dependent epigoni. They were victims of the

fallacies that the present writer came to list in a previous article as common among natural scientist [10].

The rationalistic model naturally assumes that sincerity is a hallmark of scientific work. Harlow Shapley called Velikovsky a fraud [11], without having read the book. Thereupon Shapley engaged in collective action to prevent the publication and use of Velikovsky's book, actions which he then denied upon being accused of them. He declared in the *Harvard Crimson* (Sept. 25, 1950):

> The claim that Dr. Velikovsky's book is being suppressed is nothing but a publicity promotion stunt. Like having a book banned in Boston; it improves the sales. Several attempts have been made to link such a move to stop the book's publication to some organization or to the Harvard Observatory. This idea is absolutely false.

The model of rationality demands that the *populace be barred from scientific proceedings*. Sales of a work to laymen does not disprove the validity of a work yet this seems to have been indicated by critics of Velikovsky. We even note that Velikovsky was criticized negatively for having found people to buy his book, the implication being that unless a work has the previous blessings of the scientific establishment, it has no right to exist [12].

The rational model holds that *imprecision is a defeat of scientific work*. An ideal is quantification, though many of the sciences fall short of this ideal in most of their propositions. Without foundation in fact, Gaposchkin says of *Worlds in Collision*: 'It contains no scientific arguments; not a formula, not a number (save for arbitrarily assigned dates) presents itself for analysis.' Dr Donald H. Menzel's appendix to her critique, sturdily entitled 'The celestial mechanics of electrically-charged planets,' goes on to show quantitatively that a planet or sun charged to the potential demanded by equations based on Velikovsky's theory, amounting to 10 to the 19^{th} power volts, 'would be violently unstable...trying to put such an electric field on the sun resembles trying to hold back the entire mass of water in Lake Mead by a Boulder Dam made of tissue paper

sheets'[13]. Recent space probes led Professor V. A. Bailey to the conclusion that the sun must hold a net negative charge with a potential of the order of 10 to the 19th power volts [14]. The coincidence is only that, for even Menzel's arithmetic was faulty. The main point is that in astronomy and other sciences, natural and social, to make quantification a rigid condition for the admission of new theory, even in areas where qualification today rules, can promote dysfunctional rigidities.

'Reject appeals to authority,' affirm the rationalistic rules of procedure. Presumably, nothing is made true or false by the character of its supporters. However, science has not yet discovered a set of techniques for superseding authority, and the corpus of science would be a skeleton if this rule were seriously followed. We have more to say about that shortly, but meanwhile it is well to note that in no respect was the scientific movement against Velikovsky so much at variance with the rationalistic model as in its reliance upon authority.

The rationalistic model, when it is sociological at all, remembers history, warns against the blind opposition to new science, and as insurance that it can no longer happen in our secular and non-magical age, offers the assertion that *when at first, ideas are rejected, they may return with additional proof for admission and will be cordially re-examined*. On December 21, 1962, Prof. V. Bargmann of the Department of Physics of Princeton University and Prof. Lloyd Motz of the Department of Astronomy of Columbia University published a letter in *Science* magazine claiming Velikovsky's priority of prediction of the hot surface temperature of Venus, of the existence of the magnetosphere around the Earth, and of the radio noises emanating from Jupiter. We quote from their letter:

> 'On 14 October 1953, Immanuel Velikovsky, addressing the Forum of the Graduate College of Princeton University... concluded the lecture as follows: "The planet Jupiter is cold, yet its gases are in motion. It appears probable to me that it sends out radio noises as do the sun and the stars. I suggest that this be investigated."... In April 1955 B. F. Burke and K. L. Franklin of the Carnegie Institution announced the chance detection of

strong radio signals emanating from Jupiter. They recorded the signals for several weeks before they correctly identified the source.'

'This discovery came as something of a surprise because radio astronomers had never expected a body as cold as Jupiter to emit radio waves (1. see also the *New York Times* for 28 October 1962.)'

'In 1960 V. Radhakrishmah of India and J. A. Roberts of Australia, working at California Institute of Technology, established the existence of a radiation belt encompassing Jupiter, "giving 10^{14} times as much radio energy as the Van Allen belts around the earth".'

'On 5 December 1956, through the kind services of H. H. Hess, chairman of the department of geology of Princeton University, Velikovsky submitted a memorandum to the U.S. National Committee for the (planned) IGY in which he suggested the existence of a terrestrial magnetosphere reaching the moon. Receipt of the memorandum was acknowledged by E. O. Hulbert for the Committee. The magnetosphere was discovered in 1958 by Van Allen.'

'In the last chapter of his *Worlds in Collision* (1950), Velikovsky stated that the surface of Venus must be very hot, although in 1950 the temperature of the cloud surface of Venus was known to be -25 deg C on the day and night sides alike... By 1961 it became known that the surface temperature of Venus is "almost 600 degrees [K]" (4. *Phys. Today* 14, No. 4, 30, 1961). F. D. Drake described this discovery as "a surprise... in a field in which the fewest surprises were expected". "We would have expected a temperature only slightly greater than that of the earth... Sources of internal heating [radioactivity] will not produce an enhanced surface temperature. Cornell H. Mayer writes (5. C. H. Mayer, *Sci. Am.*,204, May 1961),

> "All the observations are consistent with a temperature of almost 600 degrees," and admits that "the temperature is much higher than anyone would have predicted".'

They urged 'that his other conclusions be objectively re-examined.' Following the publication of this note, Velikovsky on January 29, 1963 submitted to *Science* magazine a more complete presentation of recent empirical evidence of the correctness of some of his statements. On January 31, the article was back in his hands with a formal letter of rejection.

In connection with reports of the Venus probes, Newsweek magazine was independently developing a story about Velikovsky at the time. The Editor of *Science*, Philip Abelson, stated to the *Newsweek* reporter in the course of a telephone inquiry that he had not read the Velikovsky manuscript before returning it.

Both as a document in the present case and for its intrinsic significance, the Velikovsky note, as submitted to *Science* and rejected, is printed below (see page 215). In the months since its submission to *Science*, additional corroborative finds have occurred. The paper was written and submitted before the results of the Mariner II probe of Venus were announced on February 26, 1963. The probe further confirmed Velikovsky's claims concerning the great heat of Venus (800 deg F) and the hydrocarbons (or organic compounds) of its envelope.

It was upon an occasion shortly after reviewing the memorandum of Velikovsky that Professor H. H. Hess, Chairman of the Department of Geology of Princeton University, wrote to Dr Velikovsky:

> I am not about to be converted to your form of reasoning though it certainly has had successes. You have after all predicted that Jupiter would be a source of radio noise, that Venus would have a high surface temperature, that the sun and bodies of the solar system would have large electrical charges and several other such predictions. Some of these predictions were said to be impossible

> when you made them. All of them were predicted long before proof that they were correct came to hand. Conversely I do not know of any specific prediction you made that has since been proven to be false. I suspect the merit lies in that you have a good basic background in the natural sciences and you are quite uninhibited by the prejudices and probability taboos which confine the thinking of most of us.

For nearly a decade, Professor Hess has encouraged a hearing for Velikovsky and a testing of his ideas.

On February 15, *Science* carried a letter by Poul Anderson that lampooned Velikovsky and criticized the Bargmann-Motz letter on grounds that jokers and science-fiction writers had also made fantastic assumptions that were later verified. When Eric Larrabee, managing editor of *Horizon* magazine, protested to Dr Abelson against the exclusion of Velikovsky's article and the publication of Anderson's letter, Abelson thanked him and replied that:

Velikovsky is a controversial figure. Many of the ideas that he expressed are not accepted by serious students of earth science. Since my prejudices happen to agree with this majority, I strained my sense of fair play to accept the letter by Bargmann and Motz, and thought that the books were nicely balanced with the rejoinder of Anderson.

When the Reverend Warner Sizemore, a Philadelphia minister, wrote to *Science* to show that the very cases that Anderson cited might be construed in favour of Velikovsky he received in reply a letter from Dr Abelson that declared:

Science can exist and is useful because much of the knowledge in it is more than 99.9 percent certain and reproducible. If science were based on suggestions that were true 50 percent of the time, and all were free to make predictions which were only that reliable, chaos would result. I have repeatedly seen men of brilliance with fertile imaginations make all kinds of suggestions. Ideas are easy. They are cheap. It is the proving of a suggestion beyond a reasonable doubt that makes it valuable.

At least half of Velikovsky's ideas have been proved wrong and he has done little to substantiate the remainder. In view of this, he is not to be taken seriously.

Yet, a few months earlier, Abelson was proclaiming the role of ideas in a *Science* editorial:

> The synthesis of xenon tetraflouride and related compounds... makes necessary the revision of many chemistry textbooks...For perhaps 15 years, at least a million scientists all over the world have been blind to a potential opportunity to make this important discovery. All that was required to overthrow a respectable and entrenched dogma was a few hours of effort and a germ of scepticism. Our intuition tells us that this is just one of countless opportunities in all areas of inquiry. The imaginative and original mind need not be overawed by the imposing body of present knowledge or by the complex and costly paraphernalia which today surround much of scientific activity. The great shortage in science now is not opportunity, manpower, money, or laboratory space. What is really needed is more of that healthy scepticism which generates the key idea - the liberating concept [15].

We must question whether the P.H.A. who wrote these lines stands for Philip H. Abelson.

This was not the first time Dr Velikovsky had difficulties entering the pages of professional journals. The *Proceedings of the American Philosophical Society*, which in 1952 carried extensive attacks upon him, would not suffer his reply. In 1956, the *Scientific American* carried a strong attack on both *Worlds in Collision* and *Earth in Upheaval* by Harrison Brown. (The magazine had refused to carry advertising of Velikovsky's book.) When Velikovsky asked for permission to rebut, the Editor Dennis Flanagan, wrote:

> I think you should know my position once and for all. I think your books have done incalculable harm to the public understanding of what science is and what scientists do. There is no danger whatever that your arguments will not be heard; on the contrary they have received huge circulation by scientific standards.
>
> Thus I feel that we have no further obligation in the matter.

This quotation reveals that the Editor has picked up a common sociological misapprehension among scientists. It is that the media of the general public can substitute for the media of science. They cannot. Furthermore, most scientists, when they reflect, realize that they themselves insist upon a distinct separation of the two types of media.

Science magazine has a subscription list of 90,000. Its sponsoring body, the A.A.A.S., includes 71,000 individual members and 298 affiliated scientific societies, academies, and other professional organizations. The *Scientific American* sells a quarter of a million copies. They can reach fully the diversified audience of scientists who are concerned with Velikovsky's work. Or they can serve as a block to the admission of new material. If the *American Behavioral Scientist* prints accounts of Velikovsky's theories, it does so in the pursuance of its commitment to treat with the sociology of science and scientific freedom. If *Science* magazine carries or does not carry the developments of the substance of Velikovsky's work, it acts out of its obligation to present new scientific propositions and theories to the scientific world.

At this point the discussion of the rationalistic system of science may be concluded. Its doctrine, formulas, and tactics have been only feebly exercised in the Velikovsky case. It has furnished a poor fit. A few scientists - in conversation, by letters, and rarely by public statement - asked for the rules of rationalistic science to be observed. The behaviours of almost all scientists involved, with the expected exception of Dr Velikovsky who acted in accord with the rules of seeking

admission, must be fitted to some other model. Perhaps it will be that which is called here the indeterminacy model.

THE INDETERMINACY MODEL

The Indeterminacy Model postulates a scientific order that is not replenished according to any scheme that is instrumentally rational. Rather it almost randomly absorbs or refuses. The lightning of discovery can strike anywhere. The pattern of science forms and becomes recognizable out of a vast collection of accidents. The truth value of the scientist and his product are alleged to have very little to do with their chances of success in being incorporated into science. Nor are they kept out by skillful managers of power and arbiters of claims.

The indeterminacy model differs from the rationalistic in that it postulates deliberate activities that are distributed so as to nullify and cancel out each other, thus giving the total system an unplanned effect. Its rules therefore are not rules of conduct but rules of effects.

The very first rule of the indeterminacy model is that ‘truth’ about reality has as much chance of rejection as of acceptance. Truth is an irrelevant trait of candidates and material.

Let us pause for a moment to contemplate this radical expression. It does not say that truth is non-existent. It can still hold to the theory that statements can be distinguished as to their relative correlation with facts, patterns of fact, predictions of events, and control of events. However, for truth to exist does not imply truth will be admitted - even to its own domain of science. Like the proverbial prophet, it can be without honour in its own land.

To conceive of this situation, let us assume that all men are scientists, even if some are more so than others. They have problems that might be solved by logico-empirical procedures. Taking into account all that men allow into their body of convictions, all the statements about the world and about the future to which they grant their assents, can it be said now, or ever, that the bulk of these statements are true? Perhaps not, at least not by logico-empirical standards.

Now, moving from the common man to the scientist, can it be said that scientists take in more correct statements than incorrect ones? To affirm such, one would have to believe that they have attained omniscience. He would say, as men usually have said through history, that those who went before had mental closets packed with the shabby clothes of superstition, wrong theories, and unempirical ideas, whereas today, most of what men know is true.

If pressed, one would be forced to justify his pride by the known effects of specialization. A worn witticism says that the scientist as specialist is one who knows more and more about less and less. This may be granted, in which event one would have to resort to a collectivist theory of knowledge: knowledge is a corporate possession; apart from the question of whether most of what is known is true, more is true today than before, despite specialization, because science is a set of wonderful pools connected by communicating pipes.

If this is so, then everything depends upon communications. If the pipes are not working, truth is forever partial and in a worse condition than when the lesser sum of it was more generally distributed. Is this the case today ? It may be. It may be becoming so. The indeterminacy model postulates that it is so. Error is not only as common as truth; but truth is fragmented for being uncommunicated. When a truth is admitted only to a small part of the realm of science, it does not exist except for that portion of the realm.

Probably the extent of the admission of error into science is underestimated by those scientists who have high morale or rigid unconscious self-doubts. Probably also truth today does not enter a reservoir of science but only a separate pool. Therefore the indeterminacy model can affirm that truth does not enter as a matter of course not because it is deliberately excluded, but from logical, social, and psychological conditions beyond current means of control.

The model suggests that the spirit of the times and customs dictate what will and will not be science. Few or many people will acquire the habits of inquiry. They will produce results,

theoretical and practical, and they will be accepted or rejected partly by chance, partly by favour or patronage, partly by publicity, partly by the use to which their work may be put.

Scientists operate under the indeterminacy system by various myths - primarily of rationality, of causation, and of power of choice - but in fact do not know what they are seeking, what is available, or what are solutions. That their compensation, whether in esteem, position, or money, is related to performance is only an illusion. What is accepted and what is rejected are therefore only a product of chance encounters of purpose and provision.

Under these circumstances, scientists follow the laws of nonrational collective behaviour. They think in stereotypes (e.g. the eternal harmony of the spheres, uniformitarianism, catastrophism). They circulate ideas via popularization and texts [16]. Thus have Newton, Galileo, Darwin, Freud and Einstein been conveyed. Scientists are at the mercy of popularizers. Their own minds are formed by simplistic ideas, try as they will to evade their grip.

A new theory spreads as a rumour, simplified, overly precise, and success comes as a surprise. No two persons understand its extended meanings quite alike. It is resistant to rational counterargument. And it persists until it is stale and a more vibrant report originates. It seems to be specific and operational until it is shown to be blind and vague; such is the fate of most past statements about the universe.

We would expect more scientists to dislike the indeterminacy model than the rationalistic or power models. It negates the rationalistic model. And the power model, though disliked, entrusts judgments to ‘qualified authorities,’ as we shall see. The indeterminacy threatens the whole order. It can be fully expected that among various kinds of scientists, statisticians and sociologists will be least offended by it, astronomers most offended, because of their own methodology. Physics and individualistic psychology, it may be noted, have in recent years been prone to demand complicated systems of priorities in giving scientific credits. Quarrelling over datelines of reports and property in ‘findings’ has sometimes occurred. This, it may

be assumed, is in part a reaction against surrendering to indeterminacy. Much greater nervousness, verging on trauma, is approaching as scientists will consign their work to the anonymous maw of the electronic information storage apparatus of the future.

Under the indeterminacy model, in the jargon of avant-garde statistics, the man/material 'takes a random walk.' The random walk signifies that *for control purposes* (including predictive and tactical behaviour) there is no pattern except randomness. Only behaviours of a low level of typicality can be discovered, and these are too weak to determine directions. In the light of this theory, the Galileo case reads understandably. One cannot escape the feeling that the treatment afforded Galileo was produced by a host of non-rational, inconsistent incidents and intrigues leading up to his condemnation. A hierarchical or power system was at work, but its instrumental rationality was inept. The Church did not behave as a fully-aware, clearly organized, accurately aimed body. Galileo's punishment seems in retrospect almost to have been an accident, though an understandable one.

The following rules prevail:

(1) There are no prescribed scientific procedures. The rule of creative hypothesis is great and scientists 'monkey around.' *Science* fiction, magic, astrology, and half-rationalized ideas are joined to logico-empirical procedures and facts, creating an environment from which practical accomplishment emerges. There is a chaos of communication. A person working in science applies himself to whatever comes to him through his peculiar interests and *situs*, and casts forth a product whose destination and fate are unknown.

The indeterminacy model stresses the chance reception of discoveries. Poincare recites how he solved a theorem of Fuchsian functions while walking across a street [17]. Karl Gauss after working for years on proof of a theorem succeeds and writes: 'At last, two days ago, I succeeded, not by dint of painful effort, but so to speak by the grace of God. As a sudden flash of light, the enigma was solved. For my part, I am not in a position to point to the thread which joins what I knew

previously to what I have succeeded in doing.' Where is Velikovsky's method, more than one of his reviewers asks in anguish. There is a method, not highly selfconscious, *not always exposed*. It is much more clearly recognizable to social scientists than to natural scientists. Sometimes the method is concealed by an easy style that separates empirically-tied ideas while allocating them to short sentences. Of course, a number of the rational propositions, which lend the work its distinction, are only as explainable as the leaps of Poincare and Gauss. The social psychology, much less the neurology, of such events is little known.

The indeterminancy model, in this regard, offers in place of the rationalistic model a description of 'normal' science as a quasi-administrative routine [18]. It affirms the *idea* over the *process*, as in the letter from Professor H. H. Hess to Velikovsky (2, Jan. 1957) that refers to the memorandum he was sending to IGY:

> ...I will pass your ideas on to Dr Kaplan in the IGY organization.. Scientific discoveries and ideas are produced by the intuition, creativeness and genius of a man. Dollars of themselves don't produce this any more than they could be expected to produce another Mona Lisa. This is something which I believe you can readily understand...

(2) There are no rules for the form in which material is submitted, nor rules for publication. Whatever is offered is admitted or rejected for reasons largely mythical. The works of Velikovsky are actually high in the scale of adduced proof and formality, by the standards of all past useful scientific production. Much of science is passed down as lore. The procedures are habitual and not rationally and consciously prescribed or learned. Much that is communicated passes via devices and hardware inventions that elude the literature of science.

The true inventor has to be dissociated from the accredited inventor. Every famous scientist rests on the back of hundreds of unknown inventors. Even if credit were to be assigned by a laborious objective research process, it would not be well enough informed to do justice to the process of discovery.

The indeterminacy model fits the inefficiencies in maintenance and replenishment of the *corpus scientiae*. Much more is discovered and forgotten than is known. Much that is known is unused or known in a partial form. In Velikovsky's works are found numerous discoveries of the past that became essential parts of his theory. The theory that a comet created destruction of Earth was itself once propounded in various forms by distinguished scientists, as Dr. Velikovsky and Professor Stecchini have shown. Whenever a new scientific discovery or invention is made, its predecessors can be unearthed. Sometimes the ideas may be shown to be in a causal sequence. At other times they are apparently aborted and unrelated. And occasionally they are independently invented in the same ideological epoch.

(3) A work penetrates into the body of science by the machinery of publicity, through acquaintanceship circles, by accident, by unconscious exposure and the creation of frames of mind (subliminal stimulation). It enters also by parallel practical operations independently derived from the same sources or from the same, different and related sources. It joins science by 'creative misunderstanding' or by 'anticreative misunderstanding.'

(4) The rationalistic modes of presentation, as treated above, become unreliable and the scientific establishment turns out to be wicked, foolish, or ineffectual. There really are heroes, whom the people adore as the *Heroes of Science*, but the scientist does not learn from the heroes and cannot know the origins of their knowledge. The heroes are really hallucinations arising from the troubled mass mind that cannot rest with an anonymous and uncontrolled world. Subscribing to the ideal system of rational science, the public performs rituals and makes obsequies to an order which they believe to exist (but which is only fantastic and invisible) and which they believe guides the destinies of science. The representatives of the public act like the member of Parliament in J. H. Poincare's story who, when asked about the value of geodesy, would answer, 'I am led to think that geodesy is one of the most useful of sciences, for it is one of those that cost us most money.'

To conclude, a reasonably satisfying history of science and of the Velikovsky case might be written from what might be called a purely phenotypical perspective. This would decommission all the personalities of science. It would consider only the massive output of symbols. It would reveal the patterns by which certain applied operations, of considerable practical value, emerged from the nodules or clusters within this communicative system. It would conclude that there is little control over the reception of new science. It would conclude that other models for organizing and incorporating new knowledge are either practical myths sustaining the morale of scientists, and/or weak determining systems having at best a mild effect on scientific advance and almost no effect on the use to which science is put.

This set of problems is familiar to history, if not to the history of science. Did Napoleon win his battles or did the French Revolution pre-conquer Europe for him? Would science be largely the same if Newton or Galileo or Einstein had not lived? Does not the readiness of people - few in the case of science and many in the case of politics - to perceive, to believe and to use new materials, ideas and instruments constitute the deterministic, inevitable, and overpowering structural force? Are not all the actions of the powerful in the personalized drama of science, like the personalized drama of political history, a glossing upon reality, a personalizing of events not less natural for being human?

The documents of the Velikovsky case explain in this light some of the behaviours that take place. They point to the immense practical impact of science while revealing the chaotic conditions of the reception system. Scarcely any scientist appears to have read Velikovsky properly. Practically all of the mechanisms for appraisal of his work failed. Yet his findings appear to be increasingly validated, if not recognized. The science of the future may be heavily conditioned by the existence of Velikovskian natural and historical science, even though many of the sources of that science might have been incubating independently of Velikovsky.

Probably some thousands of natural and social scientists might have been among the readers of Velikovsky’s works - which are

written clearly, deal with important problems, and are controversial - were it not for the curse of superstition and fakery called down upon it. Nevertheless, through the indeterminacy system, Velikovsky's works were kept alive and read. His ideas could become part of a frame of thought among a mass of people, and to some unknown degree, help them develop a new vision of history, science, and nature.

THE POWER MODEL

Still a third reception system presents itself for consideration. It is the power model. Its pure dynamics posit as an exclusive goal the admission of scientists and their works to the establishment and corpus of science only as means to the preservation or enhancement of the power and prestige of the ruling group.

In this model science is organized as a hierarchy operating by power principles in the name of the rationalistic myth. The rationalistic doctrine is embraced, formulated, and controlled as dogma by the hierarchy, which employs it as circumstances dictate. As keepers of the sacred corpus of science, the hierarchs define ethical practices. They accept or reject men and material, and inflict sanctions, all according to their own power interests.

The power model presupposes one or more power elites. It foresees a possibility of factual conflict among elites and also of dissension through ineffective control systems. It also admits the possibility of economic and political alliances that may be employed to affect the internal power structure of a science.

In the beginning are the hierarchs of the scientific establishment. As in all political situations their existence can be proven by observation of their activity, by effects of their interventions and by correct prediction, either in the present case or by transfer of evidence in other similar situations. Thus, if Professor X, head of a famed University department and incumbent of numerous professional and public specialized offices, agitates against Dr V. and sways others to do so; if typical sanctions of non-appointment, non-promotion, non-discussion, non-publication, and negative prestige result from this for Dr V. and friends; if certain correct predictions are

made about the negative response of the establishment to projected actions of Dr V.; and if the impressive positions, connections and behaviour of Professor X in other situations are of a nature similar to his behaviours towards V.: then Professor X is a hierarch and the setting in which he operates can be said to be hierarchical and those with whom he cooperates are co-leaders and those to whom he delegates the same power tasks are subordinate hierarchs, and the whole establishment is a power structure to the extent to which all of these behaviours are typical and exclusive.

An authority-sanctioned doctrine is called dogma. It is the set of beliefs about how events occur and their rightness or wrong-ness. In science, the major dogma of method is the rationalistic model. And a minor dogma about authority is contained here in the power model, so that it is permissible to claim ‘authority’ even if authority must bow down before the ‘proof’ of the rationalistic model.

If a doctrine prevails in a social order, such as is science, it cannot be ignored by the holders of power. They must rule in its terms. They must control it. Naked power is difficult to achieve and hold. Man can no more live by power alone than by bread alone. This is especially true of ruling groups such as scientific ones, that lack the sanctions of physical coercion.

The control of dogma or doctrine rests on an original legitimacy of rule and then upon control of means. In science, appointment to leading universities, designation to honours and esteem by prior designees (co-option) confer legitimacy inside and outside the establishment.

The control of dogma enables the hierarchs to dominate a controversy in that correct dogma may be attributed to oneself and violations of dogma, hence illegitimacy, to the opposition. As indicated above, the establishment leaders were not remiss in their tasks; Gaposchkin, H. Brown, Lafleur, Stewart, et al. enunciated the code before passing judgment upon Velikovsky and his works.

At the same time, they were equally careful to state, even if without confirmation, that Velikovsky violated the code of sci-

ence in salient respects. He was accused of writing for money [19]. He was accused of a hoax. In numerous varying terms, he was labelled as incompetent to discuss his topics.

Velikovsky's detractors were vulnerable, actually, on dogmatic grounds. But only in the public press could they be attacked thereupon. *Newsweek* and *Harper's* carried the chief pro-Velikovsky statements, alleging the failure of the hierarchs to conform to their asserted belief-system.

Naked power is a shameful thing in science. Members of the establishment, realizing the vulnerability of naked power, were quick to defend themselves against accusations of arbitrariness, suppression, and censorship. One reason why their reviews and letters seemed short on literary and scientific quality was that in them they were conducting a three-fold operation - they had often to assert their control over dogma, effectuate their power, and act out the model of a rationalistic reception system, all at the same time and in the same place.

There can be no ruling group without an institutional base. The preferred situs is a university of high prestige, funds, fellowships, staffs, and expensive, collectively controlled apparatus. Holding the chief position in astronomy at Harvard is in these regards like controlling the New York State delegation at a Presidential nominating convention. From such a position come honours and other positions as well. In the 1952 *Who's Who in America*, Harlow Shapley, Professor of Astronomy and Director of the Lowell Observatory at Cambridge, listed himself as an officer or member of 41 professional associations. In this case, as happens in most power situations, the network of influence extends outward through former students, new appointments, and professional rewards, and also overlaps and is reinforced by affiliations of other kinds - sometimes of a political and ideological nature, at other times of family, of money, etc.

The tactics of power normally operate to suppress undesired opinion and manipulate favourable opinion. In the scientific reception system, this involves action in two spheres, professional opinion and public opinion. The points where control can

be exercised are in the specialized and public publishing media, and in regards to individuals.

The suppression or influencing of professional opinion in the Velikovsky case occurred in the following ways:

(A) By word-of-mouth communication before and after the publication of Velikovsky's book. This is an evanescent kind of material, now consisting largely of recollections of scientists and publishers' representatives. (It would consist of items such as: Dr. Conant, then President of Harvard, meets the Editor of *Harper's* magazine at the Century Club; he says 'I have only one thing to say about your current issue: "Really!" ')

(B) By letter and 'committee of correspondence.' Item: Before Velikovsky's book is published, Madame Gaposchkin on the basis of *Harper's* article writes a violent review at the request of *The Reporter* magazine and Dr. Shapley. This is accompanied by a hortatory message prior to publication [20].

(C) By seeking recantations. Shapley asked his colleague at Harvard, Dr. Robert H. Pfeiffer, to confirm the genuineness of his statements supporting Velikovsky's *Ages in Chaos*: Pfeiffer, Lecturer in Semitic Languages, did so. Atwater was asked by professor Otto Struve in a menacing letter to reconsider and perhaps clarify his favourable disposition towards Velikovsky. At an A.A.A.S. meeting called especially to deal with problems of publishing ethics growing out of the failure to suppress completely the Velikovsky book, the Macmillan company was permitted to recant and state a safe position. (Boards of review for scientific publishing were suggested and considered by the panel.)

(D) By depriving opposing persons of positions. Their support of Velikovsky's right to be heard and/or of his theories appears to have played a significant part in the forced resignation of Gordon Atwater, Chairman of the Astronomy Department of the American Museum of Nature History and Curator of the Hayden Planetarium, and of James Putnam, a Macmillan editor for 26 years. The converse, promoting the useful allies, is found in Lafleur, of whom *Scientific Monthly*, in heralding a second article a few months later, reported that he had been appointed

to a new university and promoted to a departmental chairmanship following his article on Velikovsky.

(E) The techniques of denying and avoiding public discussion, of refusing access to scientific fora and a denial of access to scientific publications - via articles or letters of reply, or even advertising - are amply illustrated elsewhere in these pages.

In additions, the power model of the reception system operates to *restrict* credentials. Velikovsky did not possess orthodox credentials. This was made clear in the review of his work. He was of course, well trained in many fields as, one by one, his readers came around to admitting.

At that time, he had few friends, although among them was Albert Einstein. Shortly after Einstein's death, Professor Bernard Cohen reported that Einstein had spoken in humorous disparagement of Velikovsky. Einstein could not respond, but a number of personal meetings and a good deal of reading by Einstein of Velikovsky's material would refute the surmise. (Cohen himself retracted. *Cf.* the Cohen letter above, p. 15.) We note a handwritten letter in German from Einstein to Velikovsky, 30 days before the former's death, in acknowledgement of a gift of *Ages in Chaos*.

> I look forward with pleasure to reading the historical book that does not bring into danger the toes of my guild. How it stands with the toes of the other faculty, I do not know as yet. I think of the touching prayer: 'Holy St Florian, spare my house, put fire to others!' I have already carefully read the first volume of the memoirs to '*Worlds in Collision*,' and have supplied it with a few marginal notes in pencil that can be easily erased. I admire your dramatic talent and also the art and the straightforwardness of Thackeray [Thackrey] who has compelled the roaring astronomical lion to pull in a little his royal tail yet still not showing enough respect for the truth.

Velikovsky made attempts to conciliate the powers, partly in conjunction with his attempts to satisfy the demands of the

rationalistic model of the reception system. He appreciated that Shapley and Einstein, along with others, to be sure, were two heavily influential figures on the scientific scene. Einstein was a source of comfort, if not of theoretical support. Shapley was approached in the typical honest manner of 'cranks,' that is, in the course of a public forum, without introduction, and then by letter assuming naively the rationalistic operational code that 'to test a theory, you go to a testing specialist who has the required apparatus.'

It may be inquired why Velikovsky chose Shapley and Einstein, and why he engaged in other actions directed at impressing the gatekeepers of science. This behaviour is in the first place 'normal.' It indicates only that he himself was no enemy of authority, but remained throughout a naive and quixotic believer in the symbiosis of the rationalistic and power models. One might pursue farther the psychology of this set of incidents. The strongly controlled but nevertheless necessarily and typically great self-confidence of Velikovsky, which enabled him to be a 'normal' man who could still pursue tremendous hypotheses through many thousands of hours against many adversities, had a side of unconscious intellectual presumption: 'The Lodges speak only to the Cabots.'

The establishment has a final weapon against hostile innovators. It is the *concealed incorporation of their ideas*.

The best-known manifestation of the techniques of secret information is sometimes called the *'silent footnote techniques.'* Credit is given in sources, footnotes, and forewords only to those who are members of the establishment in good standing. Also there is a rule of the highly specialized to not cite anyone less highly specialized for fear of being thought too general, too popular. As a clique device, selective footnoting costs an aspirant nothing (except possibly self-respect) and shows that he *belongs* to the group, and he is 'advanced.' It also lets him grace the patronage chiefs and the powerful. It is a vote. A less expensive, less discernible, and more vitriolic tactic is hard to imagine.

To this day, despite a great deal of corroborative evidence and the passage of thirteen years, no scientist has admitted in a

work of his own that any glance that he may have given towards the skies, nor any peek into ancient documents, has been provoked by an objective and calm desire to examine Velikovsky's evidence. When relevant findings have occurred, they have not been associated with the name of Velikovsky.

Then, too, using the partially respectable and partly true doctrines of the indeterminacy model, the leaders claim that the innovator plucks his ideas and facts from the air of the times. Examples are the 'ideas are cheap' statement of Philip Abelson. Or Harrison Brown's assertions that 'Velikovsky apparently looks upon himself as an original thinker...' and 'He quotes some data which we know to be true, some which we know to be dubious and some which we know to be false.' Brown gives not a shred of evidence for this statement. It is baseless, yet a widely circulated canard among scientists is that Velikovsky made so many predictions that some are bound to be true.

Or, using the rationalistic dogma, the establishment propagandists claim that 'there are predictions *and* predictions,' meaning that correctness is not the hallmark of good predictions. Science works only on proper, methodical, laboratory work, it is declared. This mysterious science is, of course, only the power and indeterminacy procedures at work. So Velikovsky's catastrophes 'do not upset' scientists: Madame Gaposchkin goes out of her way to express the attitude, 'See how we have accepted the much greater catastrophes recently demonstrated empirically and mathematically by members of the establishment!'

ECONOMIC AND POLITICAL NETWORKS

The tactics used to enhance power within the scientific establishment include bringing in power from the outside. The most obvious external networks activated in the Velikovsky case were the economic and the political.

Here is Dr Velikovsky's description of the fatal interview in May 1950 with the President of Macmillan Company, when the latter requested him to free Macmillan from its obligation to continue publishing *Worlds in Collision*. Mr Brett said:

> Seventy per cent of the business of this company is in textbooks; it is the real backbone of our firm. Therefore we are vulnerable. Professors in certain universities have refused to see our salesmen. We have received a series of letters declaring a boycott against all our textbooks. Please realize how it works. *(Here Mr Brett picked up a pencil and drew some circles.)* Academic circles are not isolated groups; they are united in local organizations, or in professorial associations that are incorporated or represented in larger national organizations. *(And he drew larger circles.)* The American Association for the Advancement of Science in Washington, The American Philosophical Society, and the National Academy of Sciences are groups of national importance where scientists in many field are represented. In this way the academic pressure may become widespread.

The conversation is pursued and becomes difficult. Velikovsky notes again:

> Mr Brett, though very polite and trying to be pleasant, was definitely committed to his decision to free his house of a book that was arousing wrath among the powerful of the textbook world, and he began again to draw a pattern of circles to show me how the scientific groups are interlocked; how they are centred, and how they can damage a publishing house.

The most readily available economic instrument of the scientific establishment is the ‘boycott.’ It is well-known but not sufficiently appreciated that the leaders of the scientific field wield a triple influence over publishers. They are authors or sponsors of the leading works in the field. They influence opinion about books; this in turn affects purchasing. And they and their subordinates and followers in other colleges purchase an important part of the books and materials sold in the field and used as texts and required reading. When a publisher’s contact men find the doors to the mighty suddenly closed to them, this is more than pressure - it can be a mortal blow.

The establishment moved with speed and vigour to block professional support for Velikovsky's book and to boycott it and its publishers. The following occurs in a letter from Shapley to Macmillan Company *prior* to the publication of the book.

> And frankly, unless you can assure me that you have done things like this frequently in the past without damage, the publication must cut me off from the Macmillan Company.

And on February 20, one month later, and still before the book was printed, in a letter to Ted Thackrey, Editor of *Compass*, Shapley writes:

> In my rather long experience in the field of science, this is the most successful fraud that has been perpetrated on leading American publications... I am not quite sure that Macmillan is going through with the publication, because that firm has perhaps the highest reputation in the world for the handling of scientific books.

The book was published after clearing the hurdle of a board of censors instituted by Mr Brett but pressure continued. Macmillan prevailed upon Velikovsky to release it from its contract with him, presenting him with a contract with Doubleday (the book was already on the top of the best-seller list and over 50,000 copies of it had been sold) and making clear that he had no other course to take if his book were to be promoted and marketed. Indeed, the company had already stopped publicizing the book. As every bookman knows, this could be construed as a breach of faith with the author.

Subsequent correspondence indicated the nature of Operation Boycott. D. B. McLaughlin, University of Michigan astronomer, in a letter of June 16, 1950 to Fulton Oursler, *Reader's Digest*, said in part:

> *Worlds in Collision* has just changed hand, from Macmillan to Doubleday. I am frank to state that

> this change was the result of pressure that scientists and scholars brought to bear on the Macmillan Company. It is our duty to the public to prevent such fraud insofar as we can.

Paul Herget, Professor of Astronomy at the University of Cincinnati and Director of its observatory, wrote to the columnist Sokolsky, early July 1950:

> I do not believe he [Shapley] was in any sense the leader in this campaign. I was a very vigorous participant myself...For your information I enclose copies of some of my correspondence.

After the transfer was made, pressure was brought upon the Doubleday Company.

On June 30,1950, David C. Grahame, Associate Professor of Chemistry at Amherst, wrote:

> Macmillan company abandoned it [*Worlds in Collision*] because of the storm of protest it aroused among informed persons, and you, too, may find yourself kept busy answering letters of indignation from scientists the country over. Scientists are now engaged in an active boycott of the Macmillan books, their opinion should be heeded by any publisher who intends to publish a book which purports to be science. I trust that you can be dissuaded.

The Harvard University group was relentless. Professor Fred L. Whipple, who had been Shapley's chief assistant and had relieved him as Director of the Harvard College Observatory, took up the cudgels with Doubleday. On June 30, 1950 he wrote to the Blakiston Company, which was the publisher of his book, *Earth, Moon, and Planets*. Commenting on an article that *Newsweek* magazine had just published on Velikovsky's case (called 'Professors as Suppressors') he says:

> *Newsweek* has unwittingly done the Doubleday Company a considerable amount of harm. They

> have made public the high success of the spontaneous boycott of the Macmillan Company by scientifically minded people. This in turn amounts to organizing a boycott of the Doubleday Company by the thinking people who buy books. My guess is that Doubleday Company will never publish Volumes 3 and 4 [21]... In any case, since I believe that the Blakiston Company is owned by the Doubleday Company, which controls its policies as well as the distribution of its books, I am now then a fellow author of the Doubleday Company along with Velikovsky. My natural inclination, were it possible, is to take *Earth, Moon and Planets* off the market and find a publisher who is not associated with one who has such a lacuna in its publication ethics.

He would instead, he declared, give the royalties to charity and bring out no new edition. Indeed the entire popularly-written Harvard series on astronomy was soon withdrawn from Blakiston.

Whether a political network became engaged along with the scientific and economic ones is quite unclear. It may even be questioned whether so controversial a subject should be raised. (Perhaps if mere Democrats and Republicans were the participants, one might not hesitate.) And yet, the evidence suggests that an informal left-wing network might well have been in operation. This would help explain the intensity of emotion and activity exhibited by Professor Shapley and various supporters. The political affiliations of Dr Shapley during this period were under scrutiny by official agencies. The ‘normal’ threats posed by the Velikovsky work might have been intensified by the political attacks Shapley was undergoing. Velikovsky could have been a convenient, fairly helpless target of displaced aggressions.

Yet Shapley was not alone. He was supported by others who were under the same kind of political attack, for example, Kirtley Mather and Edward U. Condon. Were they all displacing aggressions? Was the British evolutionist, J. B. S. Haldane, several thousand miles away, subject to the same

collective disorder? In Britain and on the Continent of Europe, *Worlds in Collision* was received differently. Not accepted in many quarters, neither was it vilified. On the other hand, Haldane, an old friend and political ally of Shapley, wrote an exceptional diatribe against Velikovsky, even associating the book with those in America who wished to use Britain as a base for atomic warfare.

If a political network theory were to be assumed, the reasons might be several. The work of Velikovsky could be assumed to defend Jewish nationalism. It could be assumed to defend fundamentalism. It could be considered anti-materialist, anti-determinist, and obscurantist. An attack on it might also give a political apparatus, with its associated branches, some needed exercise, and, what is more, a needed victory at its lowest moment in history. The conflict could moreover serve to bind to the group unsuspecting sympathizers in a common cause of science.

This is conjectural, yet it would be improper to eliminate it entirely from consideration, even at the cost of arousing hostility in readers who, until this page, might have been in full sympathy with our presentation. To illustrate further, there occurred a strange incident that can perhaps be best understood as a network problem.

Shapley was among a group of progressives and more extreme left-wingers who, when the New York newspaper *PM* failed, backed its successor, *Compass*. On February 19, 1950, it reprinted the original *Harper's* article on Velikovsky's book, the very article which, appearing before book publication, caused an immediate hostile outburst from the Harvard group. On February 20, Harlow Shapley, on the stationery of the Harvard College Observatory, wrote to Ted Thackrey, Editor of the *Compass*. 'Dear Ted,' he began, 'Somebody has done you dirt.' The rest of the letter was smoothly persuasive to Thackrey and derogatory to Velikovsky. He referred to *Worlds in Collision* as 'a successful fraud,' 'rubbish,' and 'astrological hocus-pocus.' Einstein was later to read his letter and call it 'miserable' in a marginal notation.

However, Thackrey, far from cringing, sent back a stinging retort. He stated well the rationalistic ideal, and accused Shapley of trying to suppress Velikovsky's work. Another exchange followed. The *Compass* was not long for this world, however. Thackrey's views on issues such as the Korean War threw the communists and fellow travellers into deadly opposition to him. Eventually, key backers withdrew their financial support, and *Compass* folded.

But the main struggle over *Worlds in Collision* was not waged in the associated arenas of business and politics. It occurred within the ramparts of science. Furthermore, it was a fairly clear engagement of the one with the many. The hierarchs were not riven by dissent. There has been no revolt. The natural resort of the denied and dispossessed in a power system, factionalism, was not exercised. No faction within science attempted in the name of rationalism to substitute its interest, theories and facts for the prevailing ones.

A different kind of power behaviour within the dynamics of the model is visible. Dr Velikovsky has been more of the hermit scientist than of the hierarch, cabalist, or rebel. The model of this behaviour has the gates of scientific recognition being forced by the single-minded dedicated scholar and a small group of disciples. They create a disturbance that cannot be ignored. The whole picture is one of a power struggle where the odds against innovation are great but the addiction of the innovator to truth is supreme.

In the end, it is the outcome of the power struggle that determines whether the truth is admitted, not the rationalistic tests. Just as a soldier or a bureaucrat will exclaim in amazement over the gargantuan capacity of the collective organism to ingest irrationality and inefficiency, the scientist with any degree of historical perspective must often be shocked at the frequency with which power determines what the laws of human and natural behaviour 'are' and how a corpus of science survives.

THE DOGMATIC MODEL

A final model, the dogmatic, requires exposition. Professor Stecchini has given ample reason to believe that the resistance

to the astronomical theories of Velikovsky was motivated by sheer ideology, a dislike of challenge to an orderly universe. Much evidence can be brought forward from other fields of knowledge - archaeology, biblical studies, paleontology, geology, physics and biology - to the same effect: the theories of Velikovsky operating against the prevailing dogma are repulsed vigorously. Every weapon is brought into play against the new ideas - authoritative denunciation, arguments *ad hominem*, tricks of logic and evidence, suppression, denial of rewards, and stony silence.

By the rules of the dogmatic model, what happens is explained solely and adequately by the fact that all believers in the state of present knowledge unite to resist the innovator. New material and men are accepted in the proportion to which they conform with prevailing theories and norms.

Several tests of the dogmatic model may be proposed.
(1) Is there a universal agreement against a work on grounds other than rationalistic? If so, a dogmatic model may fit the case. The spontaneity and generality of denunciation of Velikovsky's work is compelling. The power apparatus is simply not strong enough to explain it. The rationalistic model certainly does not. Nor does the indeterminacy model. Yet the concept of a collective obsession spread among a great many persons on all scientific levels and in all scientific fields would fit the dogmatic mould.

(2) Does the power elite reject new and correct ideas even though the effects of the ideas may be expected to enhance their power? If the answer is an unambiguous 'yes,' then the dogmatic model fits. The Velikovsky case is here ambiguous, however. Partly this is owing to the lack of agreement over the correctness of his theories. But other factors could cloud the issue too. In 1950 the throne of astronomy, the queen of sciences, was shaky. It could have been bolstered by consideration of the Velikovskian theories. The weakness of classical studies was evident. They could have been rejuvenated. Biology was not in such a poor condition, but it too could have been aided by vigorous re-examination of evolutionary theory. Geology was vigorous, physics too. They

needed no great prestige. All rejected the ideas. Thus power (prestige) was not a determinant, it would seem.

However, power outside is not the same as power inside the disciplines. Time after time in history, power elites succumb because they are more intent of gaining or holding internal power than in maintaining or extending the scope and intensity of their power vis-a-vis the outer spheres. Cavalry generals have been known to risk their country's safety in order to protect the power of their outmoded arm within the military establishments. An authority in the classics might readily sacrifice the chances of his discipline to retain his personal position within it.

We do note a perceptiveness of the larger power issues among fundamentalists and other belief-groups that held a fringe position with respect to modern science. They could see a movement back into science from which they had long been displaced by evolutionary and anti-scriptural doctrines in science.

(3) Do conflicting power factions within the power elite take the same attitude towards plausible innovation? If so, then the dogmatic model is indicated. In the Velikovsky case, whatever general scientific leadership could be said to exist was either antagonistic or silent towards him. If factions existed, then dogmatism can be assumed. The answer is in doubt. The factions may not have existed or perhaps they did not perceive their 'objective interests' (indeterminacy) or perhaps they were in fact dogmatically opposed.

Going into the autonomous fields of science, the situation is somewhat clearer. In no scientific field, of the half dozen involved, did a faction seize upon the issues. In astronomy, for instance, Struve, who might have opposed Shapley, took a dogmatic position in opposition to Velikovsky. The West Coast empires of astronomy were less unanimous in opposing him. Again, the query: indeterminacy? A cancelling effect between dogmatism and factionalism ?

(4) Is there in fact a high correlation between opposition and novelty, where truth is a constant ? If so, then the dogmatic

model fits. The Velikovsky case alone cannot serve for this test. The measure of truth of the numerous theories is not yet agreed to. The opposition has treated the books wholesale; hence, opinions of one proposition are intertwined with opinions of another.

(5) Where there is awareness and interest in a work among several disciplines that are autonomous power groups, and where the rationalistic code is not applied, is agreement in the appraisal of the work conditioned by the degree to which its theories and approach are novel to the individual fields? If so, then dogmatism, rather than other behaviours, is manifest.

Here again, a sure answer is impossible in the Velikovsky case. Several fields were interested, but each suffered radical assaults. The only group that might have received the findings of Velikovsky without shock would be psychoanalytically-oriented anthropologists of folklore. But there are few of these, and they seem scarcely to have been alerted (again the indeterminacy model).

(6) Are statements purporting to be empirically proven propositions of science bluntly made and repeatedly hammered home? If so, the dogmatic model would apply. Time after time, the same simple assertions were made against Velikovsky. This is a well-known rhetorical and propagandistic device, and would fit the power model as such, but it is likely that the assertions were sincerely meant as facts. Examples:

The earth cannot stop suddenly without disintegrating. (Literally true but the affirmative was never asserted by Velikovsky.)
The sea levels did not change in historical times. (Incorrect)
Temples and dwellings from before 1500 B.C. are still standing. (Incorrect)
Excavations in Ur show no signs of flooding. (Incorrect)
Eclipses are checked to 3000 B.C. (Incorrect)
Clear records of Venus as a planet with orderly movements exist from before 1500 B.C. [22](Incorrect)
Velikovsky is not scientific.

(7) Is the language of the reviewers and commentators heavily dogmatic and authoritative rather than rationalistic? If so, then the dogmatic model is operative. In fact, this is the most obvious aspect of the Velikovsky case. In the *New Haven Connecticut Register* of June 25, 1950, there appeared a collective review of *Worlds in Collision* by four Yale professors who were shortly to republish the same review in the *American Journal of Science.* I attempted a crude analysis of the contents of the four successive reviews. Putting aside the question of the validity of empirical statements made by the authors, I attempted to discover the proportions of various kinds of formal statements that appeared in the reviews. Using the sentences as the unit of measure, I fitted each statement into one of five categories by its form: a descriptive statement purporting to carry information about the contents of the work; an empirical statement presenting a factual proposition about the scientific material; a logico-empirical statement containing a prosition of factual or conceptual relations; a dogmatic-authorative statement affirming a belief or a consensus of experts; and miscellaneous statements dealing with the personal motives of the author and publisher.

I emerged from this little exercise with 27 statements purportedly descriptive of the work, 4 purportedly empirical statements, 12 purportedly logico-empirical statements, 27 dogmatic-authoritative statements and 8 statements dealing with the character of the author and publisher. A separate summing-up of the evaluative loading of each statement resulted in a total of 2 favourable sentences, 31 neutral sentences and 46 negative statements about the work. In the Velikovsky case, then, rationalistic criticism was heavily subordinated to dogmatic-authoritative criticism of a negative character. This kind of material, if pursued through the Velikovsky case and also through many other scientific case studies, might lead to a complete overhaul of the machinery of scientific evaluation. At the very least, it would position the review function on a low level in the order of merit for the rationalistic appraisal of science.

The language of the academic reviewers is unequivocally harsh, strident and hostile. The question arises, however, whether this might not also be an indication of the power system at work.

The language of power and the language of dogmatism are often similar: established power is conservative.

Furthermore, we note that the popular reviewers, numbering into the hundreds, are more disposed to rationalistic argument with the Velikovsky ideas than the scientists. This might indicate power, not dogma, to be the issue. The conclusion may be that motives of dogmatism and power are both in evidence. An unnecessary excess of abuse reveals that *Worlds in Collision* struck at dogmatic and moralistic defences as well as at existing power structures.

REFORM OF THE SYSTEM

The documentation of the Velikovsky case cannot be completed here. Much remains to be said. It is enough, however, to the immediate tasks, if it is shown that the Power Model, the Dogmatic Model, and the Indeterminacy Model describe and explain far more of the behaviours observed in the Velikovsky case than the Rationalistic Model.

In the early stages of the Velikovsky case, numerous ‘wrong’ cues were given. Lacking a conscious, regular system for the reception of new materials, the scientific establishment was governed by intrusive psychological forces organized irrelevantly by ideological and power networks. The frequent, remarkable misreadings of plain textual material are merely one of various indications of a perceptual system operating psychopathologically.

An original spate of publicity was the red flag to the bull. It warned the authorities that an outsider was seeking entrance with strange credentials. In some scholars and scientists, a high level of political anxiety (this was the period of McCarthyism) could join with intellectual anxieties produced by ‘strange’ and ‘discredited’ forms of data and proof to form a highly combustible mixture.

The rationalistic system was suppressed and the power system and dogmatic systems were activated. Once events had taken this course, little could be done to evade the conclusion. All involved were fully committed. There was no higher court of

scientific appeal, or other checking or remedial agencies. The adjustment thereafter had to occur through unmobilized elements - young, sceptical students (from time to time Velikovsky mentions the young as his justifiers) or dissident scientists or outside intellectuals. Interestingly, the engineering profession is one of the best represented among Velikovsky's adherents.

The problem that many thought had been solved ages ago - that of recognition of new contributions - turns out to be ominously present. Actually little was solved by the great historical cases of Copernicus, Bruno, Galileo and Pasteur, for the problem has always been conceived as one of improving rationality rather than as one of the applied sociology of science and institutions. It is in every respect like the central problems of political and governmental organization; there, however, a long history of scientific attention focuses on the need for more than personal goodwill and sweet reason to preserve and promote desired behaviours.

Also like the political order, the scientific order consists of a set of sub-universes each with its own goals, routines, organization, and, hence, particular problems. Generalizations about science as a whole and subsequent policies must be based on averages and parameters, and *a priori* could provide less than the total need for policies governing the individual disciplines. That is, a few policies may work for all fields, but each field needs its own; and all such policies should be based upon extensive behavioural research.

Few scientists can be immediately useful in the policy process of science. Most are uneducated to the tasks. They do not understand the nature of ideology. They seem not to know their own psychology or their patterns of social behaviour. They do not know how their organization works or what its policies are. In the end, how can scientists be trusted to fashion solutions to a wide range of social problems to which their special 'hardware' competence must contribute? The answer is that they cannot. Unless and until there is the equivalent of a Copernican revolution (or a Velikovskian revolution) in the form of a sociological revolution in science, natural scientists as

a group will constitute a dead weight in public and professional policy, or worse, a potential force of evil.

The beginning of such a revolution must be in scientific self-knowledge. At present, scientists appear to study everything but themselves. An institute for research in scientific procedure is needed to initiate and conduct a wide variety of research projects on the behaviour of scientists. This institute should be amply supported by numerous individuals and groups and should be beholden to none. In its own structure it should predicate the goals that brought it into being. Its activities might be based on the recommendations for reform that are put forward in the passages that follow.

ON THE EDUCATION OF SCIENTISTS

The education of scientists must be broadened to include a knowledge of the aims and methods of the humanistic and behavioural disciplines.

The average scientist needs to know more of the history of science, but especially of an analytic sociological history of science. Unfortunately, the history of science is largely old-fashioned chronological recitation and rationalistic technical analysis.

The sociology of knowledge, epistemology, and pragmatic logic should be regular instruments of all of the sciences and philosophy.

The education of scientists should include ethical training. The cynicism normally provoked by analysis of the type undertaken in the present article can have a destructive effect upon creative and sustained work unless there appear to be social and professional forces working towards rationalistic ideals. The rationalistic model of science itself needs reformulation and reinforcement. Despite its failures in the Velikovsky case, it remains the most acceptable of the model reception systems of science presently conceivable.

The more frequent employment of psychiatric techniques to give specialists insight into their motives and behaviour would

help to prevent destructiveness, exclusiveness, and other unconsciously provoked behaviours.

Efforts at unified interpretations of science should be promoted. Presently, the rewards for scholars who work on bridges across the sciences are unattended chairs in philosophy. The largest expenditures, and professional prestige go to the masters of disciplinary secrets.

ON REPORTING ABOUT SCIENTIFIC BEHAVIOUR

Periodic surveys, assessments and agendas of scientific work in every discipline are needed. A clear and frank set of observations about what is and is not going on in science can help prevent a slump into the chaos of indeterminacy and into the evasive and irrelevant actions of the power-hungry. For science and all of its parts, regular reports should be prepared on the costs of maintenance, and on any imbalances between scientific and other social costs and among the various sub-sciences.

The sociology of science should focus upon the new communication systems that are rapidly developing, including linguistics, information storage and retrieval, mechanical translation, and rapid large-scale publication. The invention and control of these systems will soon force decisions that will critically affect power relations within science and society. The existing organizational structures of the sciences are inadequate to deal with such issues.

Most scientific journals are organized along lines of power; scientific controversies are often conducted like political campaigns. The journals lack serious intellectual goals; and they command few resources and skills for the massive tasks of providing free and easy communication. Their reviewing procedures need reform. Professional reviewers' associations might be set up within each scientific association; their members would engage to improve the science of scientific reviews and to use explicit agreed-upon procedures in reporting on new works. Their reviews would carry their associational 'trademark.'

ON THE CONTROLS OF SCIENCE

The associations of science are still among the primitive and puerile mechanisms of modern life. The annual convention of the American Association for the Advancement of Science has perhaps as much to do with the advancement of science as a state fair with the advancement of agriculture, but not more. Yet it is not atypical of the associative activities of science.

At present, and perhaps indefinitely, awareness of the non-rationality of scientific behaviour should favour old-fashioned means of promoting scientific freedom. For instance, the semi-independent scientific establishments that have resulted from nationalistic separateness may be preferable to an international establishment with semi-coercive powers.

On the same grounds, a pluralism of support of scientific endeavour is desirable. A multiplicity of foundations, associations, well-equipped universities and other supportive agencies may appear costly, but brings about a larger efficiency through increased initiative and varied development. In this connection, the role of non-governmental companies engaged in research and development, and of independent publishing firms, should not be understated.

It would be well to inquire whether existing institutions have any inherent capacity for trying and sanctioning unprofessional practices among professionals. Two types of problems occur: those of ethics and those of non-rationality. Most contemporary scientists, and the public, perhaps believe that scientific freedom is achieved when outside lay authorities are forbidden to rule on questions of functional ethics and scientific truth. Inquisitions are scorned. Legislative investigations are hateful. The considerable powers of lawyers and medical practitioners for self-government are regarded as inappropriate to scientific affairs.

Is there then no recourse for the scientist who has been damaged by the means detailed in these papers? Perhaps Harvard University has within its authority the right to inquire into the scientific behaviour of its faculty. Its officers might make a determination ‘on the merits’ that one or more members of the faculty were so irrelevant and destructive in their

scientific work as to violate plain standards of scientific competence. They might as a result take remedial action, as, for example, to require apologies, re-tests, re-examinations, discussion in open forums, suspension, reprimand, resignation, or dismissal. Lacking any of these forms of action, can a University be said to be responsible to its own and to the greater community for the quality of the particular activities it performs in the name of the community and of knowledge?

Scientific associations might conduct the same kind of inquiries. Their sanctions might be lighter; their responsibilities, however, are no less heavy. They could extend their authority to questions of apology, hearings, open forums, open journal pages, and suspension or withdrawal of membership.

The machinery and practices so envisioned might be self-defeating. The unorthodox voice is likely to end as the defendant, not the plaintiff, in most proceedings. The rank and file are likely to follow their leaders more than the dissident. Research is needed, therefore, into the conditions under which a hearing procedure and its consequences can be structured independently of the organization as a whole, very much as an independent court system operates in civil law.

The question arises also whether the larger society should ever take a hand in professional affairs. The investment of the public in the Velikovsky case is not inconsiderable. The scope and importance of the knowledge involved are great. Beyond them lies the public concern in how scientific scientists are. And the education being conveyed to the young is of public interest. Nor is it immaterial that a part of the nation's resources is being spent each year to solve technological problems, some of which are connected with national survival. If the public concern is present, what public machinery is to be brought into play - congressional investigations, a national science board to hear and investigate complaints, a congress of scientific associations with a judicial branch?

Such questions warrant intensive study followed by new policies. It is this writer's belief that independent hearing and reporting mechanisms should be invented for use by associations and by joint scientific-public-governmental organs.

Legislative and executive machinery should be avoided as far as possible, but quasi-judicial machinery encouraged. Scientists have on the whole tender sensitivities. A mild exposure and embarrassment usually have great corrective value for them.

These then are the conclusions reached. They are as far from the original incidents engendering the case of Dr Velikovsky as were his astronomical, geological, and historical conclusions from his early thought that Freud misjudged Akhnaton.

Immanuel Velikovsky propounded a synthetic theory of the highest order. He reordered classical chronology. He derived important truths from ancient sources that science had abandoned. Profound experiences of man's ancestors are revealed anew. He therefore has given us new understanding of man's nature.

He has shown that the present order of the solar system is quite new and that unaccounted forces help govern it. He has struck at a great part of the Darwinian explanation of evolution. He has upset several major theories of geology and offered substitutes therefore. He found space a vacuum and has made of it a plenum.

A great many of his truths are to be found scattered in the historical and contemporary byways of science. As bits of information and fragmented theories, they meant little or nothing to the many scholars and scientists who may have glanced at them and turned away. With rare imagination and consummate skill, he fashioned them into theories of great scope, compactness, and integration. While his ideas are not at all beyond criticism, as a cosmogonist he appears in the company of Plato, Aquinas, Bruno, Descartes, Newton and Kant. What would therefore be only the duty of the critics of science - to defend ordinary or even mistaken scholars - becomes, by accident, an occasion to defend a great savant of the age.

Notes (References cited in "The Scientific Reception System")

1. A person may be *favored* ‘unjustly’ by the reception system Thus, many irrelevant elements may enter into rewarding undeservedly a scientist for his behaviors. Whatever principles may be established to correct ‘unjust unacceptance’ should also be observedly operative in cases of ‘unjust acceptance.’ It also may occur that ‘unjust acceptance’ is correlated with ‘unjust unacceptance.’

2. Proto-thought is a level of assumptive prejudiced thought midway between unconscious ‘thought’ and self-controlled thinking. It is prominent in ideological and stereotyped thinking.

3. Cecilia Payne-Gaposchkin, ‘*Worlds in Collision*,’ *Proceedings of the American Philosophical Society*, Vol. 96 (October 15, 1952), pp. 519, 523.

4. Laurence J. Lafleur, ‘Cranks and Scientists,’ *The Scientific Monthly*, Vol. LXXIII (November, 1951), p. 285.

5. In a review of *Earth in Upheaval*, *Scientific American*, Vol. 194 (March, 1956), p. 127.

6. Harrison Brown, ‘Venus and the Scriptures,’ *The Saturday Review*, Vol. XXXIII (April 22, 1950), pp. 18, 19.

7. In a recent article in *Science*, M. King Hubbert has shown how an erroneous formula existed in various books over a half century without detection. ...the equation cited was for twenty-five years the most widely used equation in the petroleum industry ... it was ruefully discovered that the equation in question was neither physically correct nor a valid statement of a result established a century earlier by a Frenchman named Henry Darcy. (*Science*, March 8, 1963, p. 8856.)

8. Edwin G. Boring, ‘The Validation of Scientific Belief,’ *Proceedings, Op. cit.*, pp. 535-39.

9. 'Orthodoxy and Scientific Progress,' *Proceedings*, *Op. cit.*, p. 505.

10. *American Behavioral Scientist*, Vol. VI, December, 1962.

11.*Harvard Crimson* (September 25, 1950), p. M2, and *infra*, p.59.

12.*Cf.* James V. Conant, in *Science and Common Sense* (1951), Preface and p. 278, and in New York *Herald Tribune*, February 16, 1951.

13. *Proceedings, Op. cit.*, p. 525.

14. *Nature*, May 14, 1960; January 7, 1961; March 25, 1961.

15. *Science*, Vol. 138, October 12, 1962.

16. T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962), p. 164.

17. *Science and Method* (London, Nelson, n.d.),p. 54.

18. *Cf.* A. de Grazia, *Science and Values of Administration* (Indianapolis, Bobbs Merrill, reprint series, 1962), on science as administration; T.S. Kuhn, *Structure of Scientific Revolutions*, p.10 seq.

19. A fair estimate of Dr Velikovsky's wage rate considering his total royalties from writing and his total research time on his books, including *Worlds in Collision*, would be $1.35 an hour. He held no university or foundation appointment at any time. The typical Harvard professor could be said to be paid the equivalent of royalties on sales of 30,000 books *every year*.

20. It was in the transition from the mimeographed to the printed version that a clear ethical test was presented and failed by Dr Gaposchkin. We quote here the passage from the mimeographed text and that of the printed text:

The mimeographed version: 'If the biblical story which Mr Velikovsky seeks to establish is to be accepted at its face value, the rotation of the earth must have been stopped within six hours. All bodies not attached to the surface of the earth (including the atmosphere and the ocean) would then have continued their motion, and consequently have flown off with a speed of 900 miles an hour at the latitude of Egypt.'

The printed version (later): 'Let us assume, however, that Dr Velikovsky is right - that the earth did stop rotating. In that case all bodies not attached to the surface of the earth (including the atmosphere and the ocean) would have continued the motion, and would have flown off with a speed of nine hundred miles an hour at the latitude of Egypt.'

Nota Bene. If the earth, as she says first, decelerated within six hours, the inertial push in objects on the earth's surface would be 500 times smaller than their weight. A man of 160 lbs would experience a forward push of 5 ounces. Dr Gaposchkin now had a clear choice: Someone had called the quantitative error to her attention. She might choose to recalculate the inertia of the slower stop. She chose the latter. She took out the reference to the six hours and all other qualifications Velikovsky had introduced and kept the 900 m.p.h. reference.

21. An incorrect prediction. Doubleday Company has published, in addition to *Worlds in Collision*, *Ages in Chaos*, *Earth in Upheaval*, and *Oedipus and Akhnaton*. A fifth volume, forming a sequel to *Ages in Chaos*, is in page proofs.

22. We note such phenomena as the following triple play among reviewers: Dr Edmondson of Link Observatory obviously copies in a review from Kaempffert of the New York *Times* who had copied in his review from Gaposchkin's preview that (1) the Venus tablets from before 1500 B.C. describe regular motions of this planet 'exactly as we see it,' and that (2) Velikovsky suppressed both this fact and the very existence of the tablets. Both statements are untrue. The tablets describe very erratic motions of Venus, and Velikovsky presented the Venus Tablets in his book to support his concept.

7. ADDITIONAL EXAMPLES OF CORRECT PROGNOSIS

by Immanuel Velikovsky

In 1950 - as it is still largely today - it was generally accepted that the theory of uniformity must be true and that no process which is unobservable in our time could have occurred in the past. It was also believed that celestial bodies, the Earth included, travel serenely on their orbits in the void of space for countless eons. In *Worlds in Collision* (1950), however, I offered these theses: '(1) there were physical upheavals of a global character in historical time; (2) these catastrophes were caused by extraterrestrial agents; and (3) these agents can be identified' (from the Preface). These claims were termed a 'most amazing example of a shattering of accepted concepts on record' (Payne-Gaposchkin).

The consequences of the theory affected almost all natural sciences and many social disciplines. Especially objectionable was the assertion that events of such magnitude took place in historical times.

Worlds in Collision describes two (last) series of cataclysmic events that occurred 34 and 27 centuries ago. Not only the Earth, but also Venus, Mars, and the Moon were involved in near encounters, when the Morning Star, then on a stretched elliptical orbit following its eruption from the giant planet Jupiter, caused turmoil among the members of the solar system before settling on its present orbit.

The description was derived from literary references in the writings of ancient peoples of the world. The archaeological, geological, and paleontological evidence for the theory was collected and presented separately in *Earth in Upheaval* (1955).

In order to explain how certain phenomena could have taken place - how, for instance, Venus, a newcomer, could obtain a circular orbit, or the Earth turn over on its axis - the theory envisaged a charged state of the sun, planets, and comets, and extended magnetic fields permeating the solar system. This appeared even more objectionable since celestial mechanics had been solidly erected on the notion of gravitation, inertia and pressure of light as the only forces acting in the void, the celestial bodies being electrically and magnetically sterile in their inter-relations. *Worlds in Collision*, in its Preface, was acknowledged as heresy in fields where the names Newton and Darwin are supreme.

The only quantitative attempt to disprove one of my main theses was made by D. Menzel of Harvard College Observatory (1952) [1]. He showed ('if Velikovsky wants quantitative discussion, let us give him one'), on certain assumptions, that were I right the sun would need to hold a potential of 10 to the 19th power volts; but, he calculated that the sun, if positive, could hold only 1800 volts, and, if negative, it follows from the equation, no more than a single volt.

In 1960-61, V.A. Bailey calculated that to account for the data obtained in space probes (Pioneer V) the sun must possess a net negative charge with the potential of the order of 10^{19} volts [2].

In 1953 Menzel wrote: 'Indeed, the total number of electrons that could escape the sun would be able to run a one cell flash-light for less than one minute.'[3] My affirmation of electromagnetic interactions in the solar system became less objectionable with the discovery of the solar wind and of magnetic fields permeating the solar system.

My thesis that changes in the duration of the day had been caused in the past by electromagnetic interactions was rejected in 1950-51 [4]. In February 1960, A. Danjon, Director, Paris Observatory, reported to l'Académie des Sciences that following a strong solar flare the length of the day suddenly increased by 0.85 millisecond. Thereafter the day began to decrease by 3.7 microseconds every 24 hours [5]. He ascribed the fluctuation in the length of the day to an electromagnetic cause connected with the flare. His announcement 'created a

sensation among the delegates to the General Assembly of the International Union of Geodesy and Geophysics' that year in Helsinki [5].

V. Bargmann of Princeton University and L. Motz of Columbia University claimed for me the priority of predicting radio-noises from Jupiter, the existence of a magnetosphere around the earth, and the high ground temperature of Venus [6]. They stressed also that these discoveries later came as great surprises, though I have insisted in my published works, in my lectures, and in my letters that these physical conditions are directly deducible from my theory.

These claims were not made casually or in a veiled form. Some of my arguments for Jupiter sending out radio-noises can be learned from my correspondence with A. Einstein. I could add that if the solar system as a whole is close to neutrality, and the planets possess charges of opposite sign to that of the sun, Jupiter must have the largest charge among the planets. Rotating quickly the charged planet creates an intense magnetosphere.

In the last chapter of *W. in C.* ('The Thermal Balance of Venus') I insisted that ' Venus is hot' and 'gives off heat' as a consequence of its recent origin and stormy history before settling on its orbit. In 1954, R. Barker suggested that a layer of ice on the night side of Venus is responsible for the ashen light [7]. It is more probably a visible sign of incandescence. When in 1961 the temperature of Venus was found to be ca. 600 deg K, it was admitted that neither radioactivity nor greenhouse effect suffices to explain why Venus is so hot.

Several of the sensors of Mariner II were beyond their capacity to report temperatures before the nearest point to Venus was reached, 'because temperatures beyond their designed scale were encountered,' as reported by C. W. Snyder to the meeting of the American Geophysical Union, December 28, 1962 [8]. On December 15, 1962, a day after Mariner II passed the point of closest approach, the 'temperature had inexplicably started to drop'[9].

It is interesting also to know why the temperature of the upper cloud layer of Venus measured in the 1920's by Pettit and Nicholson (-33 deg C for the dark side, -38 deg C for the bright side)[10] was found in the 1950's by Stinton and Strong to be a few degrees lower (ca. -40 deg C for both sides)[11]. Could it be that Venus cools off at this rate ? It would point, too, to its youth as a celestial body.

In 1950 the critics of *W. in C.* emphatically objected to the notion that Venus is a young Planet or that it erupted from Jupiter.

R. A. Lyttleton (1959-60) showed why the terrestrial planets, Venus included, must have originated from the giant planets, notably Jupiter, by disruption [12]. W. H. McCrea (1960) calculated that no planet could have originated by aggregation inside the Jovian orbit [13].

R. M. Goldstein and R. L. Carpenter reported to the meeting of the American Geophysical Union at Palo Alto, the last week of December 1962, that radar probes from Goldstone Tracking Station between October 1 and December 17, 1962, confirmed earlier indications that Venus rotates very slowly and retrogradely. According to the press, this led to the following surmises: 'Maybe Venus was created apart from other planets, perhaps as a second solar explosion, or perhaps in a collision of planets.'[14] To this, compare *W. in C.*, p. 373: 'The collision between major planets... brought about the birth of comets. These comets moved across the orbits of other planets and collided with them. At least one of the comets in historical times became a planet - Venus, and this at the cost of great destruction on Mars and on the earth.'

In the section 'The Gases of Venus' in *W. in C.* (1950), I concluded that Venus must be rich in hydrocarbons. This theory was termed 'surprising' (H. Shapley, 1946) when, a few years in advance of the publication of my book, I requested that Harvard College Observatory make a spectral search for hydrocarbons in Venus's atmosphere [15]. In 1955, Fred Hoyle proposes, on theoretical grounds, that Venus is covered by oceans of oil and that its atmosphere is clouded by hydrocarbon droplets [16]. I, however, wrote: '...as long as Venus is too hot

for the liquefaction of petroleum, the hydrocarbons will circulate in gaseous form.' (*W. in C.*, p. 169).

The extraterrestrial origin claimed in my book for at least part of the petroleum deposits, notably those of the Mexican Gulf area, was scorned (C. R. Longwell, 1950)[17], and it was asserted that petroleum is never found in recent sediments (J. B. Patton, 1950).[18] However, soon thereafter, P. V. Smith (1952)[19] reported the 'surprising' fact that the oil of the Gulf of Mexico is found in recent sediment and must have been deposited during the last 9,200 plus or minus 1,000 years.

Hydrocarbons were subsequently found on meteorites, a fact termed by H. H. Nininger (1959)[20] also 'surprising': 'These resemble in many ways some of the waxes and petroleum products that are found on the earth.' Several months ago, A. T. Wilson (1962)[21] postulated an extraterrestrial origin of the entire terrestrial deposit of oil. In *W. in C.* (p.55), presence of hydrocarbons on meteorites was anticipated. The experiment in which high molecular weight hydrocarbons were compounded from ammonia and methane with electrical discharges (Wilson, 1960) [22] supports the view that the planet Jupiter (rich in ammonia and methane) was the source of the hydrocarbons on Venus, on meteorites, and in some of the earth's deposits (*W. in C.*, 'The Gases of Venus').

My contention that Mars's atmosphere must be rich in argon and neon and possibly nitrogen was made early in my work (lecture titled 'Neon and Argon in the Atmosphere of Mars'). A few years later, Harrison Brown, on theoretical grounds and independently, arrived at the same conclusion concerning argon: 'In the case of Mars, it might well be that argon is the major atmospheric constituent.'[23] But he thought that rare gases 'are essentially non-existent' on meteorites. In recent years neon and argon have been repeatedly discovered on meteorites (H. Stauffer, 1961)[24], as anticipated in *W. in C.* (pp. 281 ff, 367).

Concerning the Moon, I asserted that its surface had been subjected to stress, heating (liquefaction) and bubbling activity in historical times. 'During these catastrophes the moon's surface flowed with lava and bubbled into great circular formations,

which rapidly cooled off ...In these cosmic collisions or near contacts the surface of the moon was also marked with clefts and rifts' (*W. in C.*, 'The moon and Its Craters'). H. Percy Wilkins (1955) described numerous domes that might be regarded as examples of bubbles which did not burst.'[25].

Signs of tensional stresses have been detected on the Moon (Warren and Fielder, 1962)[26]; volcanic activity has been unexpectedly discovered by Kozyrev (1958)[27]. Sharp outlines of lunar formations could not have persisted for millions of years in view of the thermal splintering due to great changes in temperature, over 300 degrees, in the day-night sequel and during the eclipses. H. Jeffreys (1959)[28] drew attention to this evidence for the youth of the surface features, but made it dependent on the presence of water in the rocks. Since there seems to be volcanic activity on the Moon, water is most probably present in the rocks.

Assertions that the Earth's axis could not have changed its geographical or astronomical position constituted one of the main arguments against *Worlds in Collision* [29]. They gave place to the theory of wandering poles. Th. Gold (1955ff)[30] shows the error in the view of G. Darwin and Lord Kelvin on the subject, and stresses the comparative ease with which the globe could - and did - change its axis, even with no external force applied.

Confirmed is also the conclusion that advanced human culture would be found in the today uninhabited area 'on the Kolyma or Lena rivers flowing into the Arctic Ocean' in northeastern Siberia (*W. in C.*, p. 329) in the region where herds of mammoths roamed. Already in 1951, A. P. Okladnikov [31] making known the results of his research in northern Siberia, wrote: 'about two to three millennia before our era, neolithic races...spread to the very coast of the Arctic Ocean in the north and the Kolyma in the east.' Twenty-five hundred years ago copper was worked in the taiga of Yakutsk.

Under the heading 'The Reversed Polarity of the Earth' (*W. in C.*, pp. 114ff.) is written: 'In recent geological times the magnetic poles of the globe were reversed.' The phenomenon that could cause it was described, and the question was asked

'whether the position of the magnetic poles has anything to do with the direction of rotation of the globe.' Complete and repeated sudden reversals of the magnetic poles were postulated by S. K. Runcorn (1955)[32] and P. M. Blackett (1956)[33]. Runcorn wrote: 'There seems no doubt that the earth's field is tied up in some way with the rotation of the planet. And this leads to a remarkable finding about the earth's rotation itself...The planet has rolled about, changing the location of its geographical poles.' Complete reversals would change the rising and setting points, west becoming east, as described in many ancient sources collated in *W. in C.* The pioneers in paleomagnetic studies, G. Folgheraiter and P. L. Mercanton [34], found a reversal of the earth's magnetic field in the Central Mediterranean area in the 8th century before the present era, recorded in the magnetic dip of the Etruscan and Attic vases; their position in the kiln is learned from the flow of glaze. This find is in harmony with the events described on pp. 207-359 of *W. in C.*

Radiocarbon analysis, besides disclosing that some petroleum is of recent origin and deposit, verified also the claim (*W. in C.*, 'The Ice Age and the Antiquity of Man') that the last glacial period ended less than 10,000 years ago. One of the first and most important results of the new method was the reduction of the time of the last glaciation. 'The *advance* of the ice occurred about 11,000 years ago... Previously this maximum advance had been assumed to date from about 25,000 years ago,' reported W. F. Libby and Frederick Johnson in 1952 [35]. Later this figure was still more reduced; furthermore, it refers to the advance, not the end of the retreat of the ice cover.

Possibly the most clear-cut case of vindication concerns the antiquity I assigned to the Mesoamerican civilizations (Mayas, Toltecs, Olmecs). G. Kubler of Yale University wrote (1950)[36]:

> The Mesoamerican cosmology to which Velikovsky repeatedly appeals for proof did not originate and could not originate until about the beginning of our era.

Kubler showed a discrepancy of over 1,000 years and asserted that events I ascribed to the 8th-4th centuries before the present era could not have taken place until rather late in the Christian era. But on December 30, 1956, the National Geographical Society, on its own behalf and that of the Smithsonian Institution, announced:

> Atomic science has proved the ancient civilization of Mexico to be some 1,000 years older than had been believed. The findings basic to Middle American archaeology, artifacts dug up in La Venta, Mexico, have been proved to come from a period 800 to 400 or 500 A.D., more than 1,000 years later. Cultural parallels between La Venta and other Mexican archaeological excavations enable scientists to date one in the terms of the others. Thus the new knowledge affects the dating of many finds. Dr Matthew W. Sterling, Chief of the Bureau of American Ethnology at the Smithsonian Institution, declared the new dating the most important archaeological discovery in recent history.

P. Drucker and his co-workers have elaborated on the subject in *Science* (1957) and in the report of the excavation (1959)[37].

H. E. Suess, because of an accumulation of certain discrepancies in the radiocarbon dates, assumes that natural events caused a radical change in the intensity of the magnetosphere and in the influx of cosmic rays sometime in the second millennium before the present era. Several other researchers came to the same conclusion [38]. This is also in harmony with the story related in my book.

Oceanographic research brought several confirming data. H. Pettersson of Goteborg found so much nickel in clay of the oceanic bed that he inferred that at some time in the past there had been a prodigious fall of meteorites [39]. In *W. in C.*, the descent of enormous trains of meteorites and meteoric dust and ash (pp. 51ff) of land and sea is narrated, with reliance on ancient sources. In 1958, J. L. Worzel found a layer of white ash, 5 to 30 cm thick, very close to the bottom, evenly spread

over an enormous area of the ocean bed in the Pacific, and he thought of a 'fiery end of bodies of cosmic origin'[40]. M. Ewing cites evidence that the same ash layer of 'remarkable uniformity of thickness' found by Worzel in the Pacific underlies all oceans and assumes 'a cometary collision'[41]. It could hardly be without some recorded consequences of global extent,' Ewing concluded. To this a line from *W. in C.* ('the Darkness') can be quoted: 'The earth entered deeper into the tail of the onrushing comet' with its 'sweeping gases, dust, and cinders' and 'the dust sweeping in from interplanetary space.'

In 1950 a past collision of the earth with a comet was denied, and comets were also regarded as very tenuous and light masses incapable of causing much damage [42]. R. Wildt claimed that the largest comet would have a mass equal to one millionth of that of Venus [42]. But N. T. Bobrovnikoff (1951)[43] Director of Perkins Observatory, took a different view. Several comets seen in the 19th century moved in very similar orbits and 'in all probability, are the result of decomposition of one single body.' He estimated that: 'If put together' these comets 'would make something like the mass of the moon.'

Before Ewing, a cometary collision was postulated in 1957 by H. Urey to explain the tektites and their distribution [44]. G. Baker insists that Australian tektites (australites) have lain in place no longer than 5,000 years [45].

3,500 years ago the oceans suddenly evaporated and the water level dropped about twenty feet, a fact first noted by R. Daly and later confirmed by Kuenen [46]. Rubin and Suess found that 3,000 years ago glaciers in the Rockies suddenly increased in size [47]. Scandinavian and German authors date *Klimastürze* at 1500 and 700 B.C. - the very period of great perturbations described in *W. in C.* [48].

In the ocean floor B. Heezen discovered (1960)[49] a ridge split by a deep canyon, or 'crack in the crust that runs nearly twice around the earth.' He wrote: 'the discovery at this late date of the midocean ridge and rift has raised fundamental questions about basic geological processes and the history of the earth and has even had reverberations in cosmology.'

Prof. Ma (Formosa) claims that there was a sudden and total shift in the crust only 26 and 32 centuries ago, as evidenced by the shift of marine sediments (1955) [50]. It was argued that in global catastrophes of such dimensions no stalactites would have remained unbroken, but within one year after the atomic explosion, stalactites grew in the Gnome cavern, New Mexico: 'All nature's processes have been speeded up a billionfold.'[51]

Claude F. A. Schaeffer of College de France, in his Stratigraphie Comparée [52] on which he worked not knowing of my simultaneous efforts, came to the conclusion that the Ancient East, as documented by every excavated place from Troy to the Caucasus, Persia, and Palestine-Syria, underwent immense natural paroxysms, unknown in modern annals of seismology; cultures were terminated, empires collapsed, trade ceased, populations were decimated, the earth upheaved, the sea erupted, ash buried cities, climate changed. Five times between the third and the first millennia before the present era the cataclysms were repeated, closing the Early and the Middle Bronze Ages in their wake. The number of catastrophes and their dates relative to historical periods coincide in Schaeffer's estimate and in my own. From source material of a different nature - archaeological - he found that the greatest catastrophe terminated the Middle Kingdom in Egypt (Middle Bronze). Thus we are in agreement to a day. The catastrophe that ended the Middle Kingdom in Egypt is the starting point of *Worlds in Collision* (and *of Ages of Chaos,* my reconstruction of ancient chronology).

The recent finds in astronomy, especially in radioastronomy (sun, Venus, Jupiter), have given confirmation from above; oceanography, radiocarbon, paleomagnetism, and archaeology have carried their shares from below.

Notes (References cited in "Additional Examples of Correct Prognosis")

1. D. Menzel, *Proc. Amer. Philos. Soc*. (October, 1952).

2. V. A. Bailey, *Nature*, May 14, 1960; January 7, 1961; March 25, 1961.

3. Menzel, *Flying Saucers* (Harvard University Press), 1953, p. 236.

4. J. Q. Stuart, Princeton University Observatory, in *Harper's*, June, 1951.

5. A. Danjon, *Comptes rendus des séances de L'Académie des Sciences*, 250 #8 (February 22, 1960); 250 #15 (April 11, 1960).

5a. New York *Times*, July 30, 1960.

6. *Science*, December 21, 1962.

7. R. Barker, *J.B.A.A.*, 64, 60 (1954).

8. New York *Times*, December 29, 1962 (West coast ed.).

9. U.P.I. dispatch from Washington, D.C., in Philadelphia *Inquirer*, December 16, 1962.

10. E. Pettit in Hynek (ed.) *Astrophysics*, McGraw-Hill, 1951, revised by Pettit and Nicholson, *Publ. Astr. Soc. of the Pacif.* 67 (1955), p. 293.

11. Sinton and Strong, *Science*, 123, 676 (1956).

12. R. A. Lyttleton, *Monthly Notices*, Royal Astr. Soc. 121 #6 (1960); *Man's View of the Universe*, 1961, p. 36.

13. H. H. McCrea, *Proceedings*, Royal Society, Series A, Vol. 256 (May 31, 1960).

14. *The National Observer*, December 31, 1962.

15. H. Shapley to H. M. Kallen, May 27, 1946.

16. F. Hoyle, *Frontiers of Astronomy*, 1955, pp. 68-72.

17. C. R. Longwell, Am. J. of Science, August, 1950.

18. J. B. Patton quoted by F. E. Edmondson, *Courier-Journal*, Louisville, Ky., April 23, 1950.

19. P. V. Smith, *Science*, October 24, 1952.

20. H. H. Nininger, *Out of the Sky* (Dover Publ.), 1959, pp. 89-90.

21. A. T. Wilson, *Nature*, October 6, 1962.

22. *Ibid.*, December 17, 1960.

23. H. Brown in *The Atmospheres of the Earth and Planets*, ed. Kuiper, 1949, p. 268.

24. H. Stauffer, *J. Geoph. Res.*, 66 #5 (May,1961).

25. H. P. Wilkins, *The Moon*, 1955, p. 42.

26. B. Warren and G. Fielder, *Nature*, February 24, 1962.

27. N. A. Kozyrev, November 3, 1958. *Cf.* Z. Kopal, *The Moon* (1960), p. 96.

28. H. Jeffreys, *The Earth*, 4th ed.(1959), p. 377.

29. C. Payne-Gaposchkin, *Popular Astronomy*, June, 1950.

30. Th. Gold, *Nature*, 175, 526 (March 26, 1955); *Sky and Telescope*, April, 1958.

31. A. P. Okladnikov, in *Po Sledam Drevnikh Kultur*, Moscow, 1951; Russ. Transl. series of the Peabody Museum, 1, #1(1959).

32. S. K. Runcorn, *Scientific American,* September, 1955.

33. P.M. Blackett, *Lectures on Rock Magnetism,* Jerusalem, 1956.

34. G. Folgheraiter, *Archives des sciences physiques et naturelles* (Geneva), 1899; *Journal de Physique*, 1899; P. L. Mercanton, *Archives des science phys. et nat.*, 1907 (t.xxiii).

35. F. Johnson in W. F. Libby, *Radiocarbon Dating* (University of Chicago Press), 1952.

36. G. Kubler, *Am. J. of Science*, August, 1950.

37. P. Drucker, R. F. Heizer, R. J. Squier, *Science*, July 12, 1957*; Excavations at La Venta* (Smithsonian Institute, 1959).

38. *Cf.* a series of articles by V. Milojcic, *Germania*, 1957 ff, E. Ralph, *Am. J. of Sc., Radiac. Suppl.*, 19559 (note to samples p-214, p-215, p-216).

39. H. Pettersson, *Scient. American*, August, 1950; *Tellus*, I, 1949.

40. J. L. Worzel, *Proc. Nat. Acad. of Sc*., Vol. 45 #3 (March 15, 1959).

41. M. Ewing, *Proc. Nat. Acad. of Sc*., Vol. 45 #3.

42. R. Wildt, *Amer. J. of Science*, August, 1950.

43. N. T. Bobrovnikoff, in *Astrophysics*, ed. Hynek, McGraw-Hill, 1951, pp.310-311.

44. H. Urey, *Nature*, March 16, 1957.

45. G. Baker, *Nature*, January 30, 1960.

46. P. H. Kuenen, *Marine Geology*, 1950, p.538.

47. Rubin and Suess, *Science*, April 8, 1955.

48. H. Games and R. Nordhagen, *Mitteil. der Geograph. Ges. in Munchen*, XVI, H. 2 (1923), pp. 13-348. R. Sernander, *'Klima-verschlechterung, Postglaciale'* in *Reallexikon der Vorgeschichte*, VII (1926); O. Paret, *Das Neue Bild der Vorgeschuchte* (1948), p.44.

49. B. Heezen, *Scient. American*, October, 1960.

50. T.Y.H.Ma, *Alterations of Sedimentary Facies on the Ocean Bottom*, 1955.

51. Dispatch by W. Hines of the Washington *Star* from Carlsbad, N.M., in *The Evening Bulletin*, December 18, 1962.

52. C. F. A. Schaeffer, *Stratigraphie comparée*, Oxford University Press, 1948; Im. Velikovsky, Theses for the Reconstruction of Ancient History *(Scripta Academica Hierosolymitana)*, 1945.

APPENDIX I

ON THE RECENT DISCOVERIES CONCERNING JUPITER AND VENUS

In the light of recent discoveries of radio waves from Jupiter and of the high surface temperature of Venus, we think it proper and just to make the following statement.

On October 14, 1953, Immanuel Velikovsky, addressing the Forum of the Graduate College of Princeton University in a lecture entitled '*Worlds in Collision* in the Light of Recent Finds in Archaeology, Geology and Astronomy: Refuted or Verified?' concluded the lecture as follows: 'The planet Jupiter is cold, yet its gases are in motion. It appears probable to me that it sends out radio noises as do the sun and the stars. I suggest that this be investigated.'

Soon after that date, the text of the lecture was deposited with each of us [it is printed as supplement to Velikovsky's *Earth in Upheaval* (Doubleday, 1955)]. Eight months later, in June 1954, Velikovsky, in a letter, requested Albert Einstein to use his influence to have Jupiter surveyed for radio emission. The letter, with Einstein's marginal notes commenting on this proposal, is before us. Ten more months passed, and on April 5, 1955, B. F. Burke and K. L. Franklin of the Carnegie Institution announced the chance detection of strong radio signals emanating from Jupiter. They recorded the signals for several weeks before they correctly identified the source.

This discovery came as something of a surprise because radio astronomers had never expected a body as cold as Jupiter to emit radio waves [1].

In 1960 V. Radhakrishnah of India and J. A. Roberts of Australia, working at California Institute of Technology,

established the existence of a radiation belt encompassing Jupiter, ‘giving 10 to the 14th power times as much radio energy as the Van Allen belts around the earth.’

On December 5, 1956, through the kind services of H. H. Hess, chairman of the department of geology of Princeton University, Velikovsky submitted a memorandum to the U.S. National Committee for the (planned) I.G.Y. in which he suggested the existence of a terrestrial magnetosphere reaching the moon. Receipt of the memorandum was acknowledged by E. O. Hulburt for the Committee. The magnetosphere was discovered in 1958 by Van Allen.

In the last chapter of his *Worlds in Collision* (1950), Velikovsky stated that the surface of Venus must be very hot, even though in 1950 the temperature of the cloud surface of Venus was known to be -25 deg C on the day and night sides alike.

In 1954 N. A. Kozyrev [2] observed an emission spectrum from the night side of Venus but ascribed it to discharges in the upper layers of its atmosphere. He calculated that the temperature of the surface of Venus must be + 30 deg C; somewhat higher values were found earlier by Adel and Herzberg. As late as 1959, V.A. Firsoff arrived at a figure of + 17.5 deg C for the mean surface temperature of Venus, only a little above the mean annual temperature of the earth (+14.2 deg C) [3].

However, by 1961 it became known that the surface temperature of Venus is ‘almost 600 degrees (K)’[4]. F. D. Drake describe this discovery as ‘a surprise... in a field in which the fewest surprises were expected.’ ‘We would have expected a temperature only slightly greater than that of the earth... Sources of internal heating (radioactivity) will not produce an enhanced surface temperature.’ Cornell H. Mayer writes [5], ‘All the observations are consistent with a temperature of almost 600 degrees,’ and admits that ‘the temperature is much higher than anyone would have predicted.’

Although we disagree with Velikovsky’s theories, we feel impelled to make this statement to establish Velikovsky’s

priority of prediction of these two points and to urge, in view of these prognostications, that his other conclusions be objectively re-examined.

V. BARGMANN
Department of Physics, Princeton University,
Princeton, New Jersey

LLOYD MOTZ
Department of Astronomy,
Columbia University, New York

Notes (References cited in "Appendix I - On recent Discoveries Concerning Jupiter and Venus")

1. See also the New York *Times* for October 28,1962.

2. N. A. Kozyrev, Izv. *Krymsk. Astrofiz. Observ*. 12 (1954).

3. *Science* News 1959, 52 (Summer 1959).

4. *Phys. Today* 14, No. 4, 30 (1961).

5. C. H. Mayer, *Sci. Am*. 204 (May, 1961).

APPENDIX II

VELIKOVSKY 'DISCREDITED': A TEXTUAL COMPARISON

The various writings of Harvard astronomer Cecilia Payne-Gaposchkin against *Worlds in Collision* (*The Reporter*, March 14, 1950; *Popular Astronomy*, June, 1950, *Proceedings of the American Philosophical Society*, Vol. 96, October, 1952) provided a convenient reservoir of damaging testimony from which her colleagues as well as lesser critics drew freely in formulating their own opinions and in preparing further commentaries on the book.

Reproduced below are passages from Gaposchkin's paper that appeared in the *Proceedings of the American Philosophical Society* and the material in Velikovsky's book that she purportedly discredited. The reader may judge for himself who is guilty of faulty scholarship and purposeful misrepresentation.

THE CRITICISM: I

Gaposchkin:

The thesis of the book is scientific, but the evidence is drawn from an immense mass of biblical evidence and Hebrew tradition, myth and folklore, classical literature and the works of the Church fathers. A critic is faced ... with the herculean labour of laying a finger on the flaws in an argument that ranges over the greater part of ancient literature. [But] when one examines [Velikovsky's] sources, his argument falls to pieces...He has not only chosen his sources; he has even chosen what they shall mean.

Let me give one example. [Gaposchkin quotes from *Worlds in Collision*:] 'One of the places of the heavenly combat... was on the way from Egypt to Syria. According to Herodotus, the final

act of the fight between Zeus and Typhon took place at Lake Serbon on the coastal route from Egypt to Palestine.' *But Herodotus says nothing about the battle, or even about Zeus*, in the passage quoted. [The dots denoting an omission and the italics are Gaposchkin's. She next quotes Herodotus in Greek and translates:] 'Egypt begins at the Serbonian shore, where, they say, Typhon is hidden.'

[Gaposchkin makes it appear that Velikovsky invented the battle and its participants, because Herodotus speaks only of Typhon's place of burial, not of a battle.]

THE TEXTS: I

Velikovsky (*Worlds in Collision*, pp. 78-81):

[The quoted sentence in *Worlds in Collision* follows almost three pages of a description of the battle between Zeus and Typhon, quoted from Apollodorus: 'Zeus pelted Typhon at a distance with thunderbolts...'] The Egyptian shore of the Red Sea was called Typhonia (Fn: Strabo, vii, 3, 8). Strabo narrates also that the Arimi (Syrians) were terrified witnesses of the battle of Zeus with Typhon... 'who... when struck by the bolts of lightning, fled in search of a descent underground.'

[Restituted in full, the passage quoted by Gaposchkin reads as follows:] One of the places of the heavenly combat between elementary forces of nature - as narrated by Apollodorus and Strabo - was on the way from Egypt to Syria. (Fn: Mount Casius, mentioned by Apollodorus, is the name of Mount Lebanon as well as of Mount Sinai. *Cf.* Pomponius Mela, *De situ orbis*.) According to Herodotus, the final act of the fight between Zeus and Typhon took place at Lake Serbon on the coastal route from Egypt to Palestine. (Fn: Herodotus ii, 5. Also Apollonius Rhodius in the *Argonautica,* Bk. ii, says that Typhon 'smitten by the bolt of Zeus... lies whelmed beneath the waters of the Serbonian lake.') [Actually, the Harvard University edition of Herodotus (Loeb Classical Library) connects the quoted sentence about the place where Typhon is entombed with his defeat by Zeus.]

THE CRITICISM: II

Gaposchkin continues:

A cosmic encounter, we read, was responsible for the destruction of the army of Sennacherib by a 'blast of fire.' But none of the three biblical accounts of the event mentions a blast: each one ascribes the defeat of the enemy to an angel. (Fn: II Kings, xx, 35; II Chronicles, xxxvii, 2; Isaiah, xxxvii, 36). We do find a blast in the prophecy made by Isaiah *before* the event: 'Behold, I will send a blast upon him, and he shall hear a rumour, and shall return to his own land.' (Fn: II Kings, xix, 7). But the Hebrew word used here means 'wind or spirit' rather than 'fire.'

[Thus Velikovsky is accused of suppressing the 'angel' as the agent of destruction in the story of Sennacherib's debacle; of incorrectly interpreting 'blast of fire,' which words do not appear in the biblical narrative]

[Next, Gaposchkin implies that Velikovsky suppressed Herodotus's version of Sennacherib's defeat:] Herodotus gives a very different account of the defeat of Sennacherib's army, which does not suggest any catastrophe on a cosmic scale. [The passage in Herodotus is printed in Greek, and a translation follows it (Gaposchkin's dots):] Afterwards...Sennacherib, king of the Arabians and Assyrians, marched his vast army into Egypt.... As the two armies lay here opposite one another, there came in the night a multitude of field-mice, which devoured all the quivers and bowstrings of the enemy, and ate the thongs by which they managed their shields. Next morning they commenced their flight and great multitudes fell, as they had no arms with which to defend themselves.(Fn: *History*, iii; Rawlinson translation.)

[Gaposchkin concluded:] If all readers had complete classical libraries, and could read them; if every man were his own Assyriologist and habitually studied the Bible in the Hebrew and Septuagint versions, Dr Velikovsky would have had short shrift.

[When Velikovsky submitted to the editors of the *Proceedings of the American Philosophical Society* evidence that he *had not*

misquoted the Biblical passages, had not ascribed 'blast of fire' to a Biblical text, and *had not* suppressed Herodotus's version, he was refused access to the pages of that journal for a rejoinder. As a result, more than one irresponsible writer was misled into echoing Gaposchkin: 'Thus when Velikovsky quotes Herodotus about a battle between Zeus and Typhon and Isaiah on the destruction of Sennacherib's army by fire, you have only to turn to the books cited to learn that Herodotus... and Isaiah said nothing of the sort' - this from an article by L. Sprague de Camp ('Orthodoxy in Science,' *Astounding Science Fiction*, May, 1954.)]

[As late as the fall of 1962, the reader information service of the *Encyclopedia Britannica*, in answer to inquiries about the validity of Velikovsky's theories, mailed out a five-page-long compilation of excerpts from critical reviews of *Worlds in Collision*. More than three pages were filled with Gaposchkin passages in the same vein as, and including, those set forth here for comparison with Velikovsky's text.]

THE TEXT:II

Velikovsky (*Worlds in Collision*, pp. 230-231):

The destruction of the army of Sennacherib is described laconically in the Book of Kings: 'And it came to pass that night, that the angel of the Lord went out, and smote in the camp of the Assyrians a hundred four score and five thousand; and when the people arose in the morning, behold, they were all dead corpses. So Sennacherib king of Assyria departed, and went and returned, and dwelt in Nineveh.' It is similarly described in the Book of Chronicles: .'..And the Lord sent an angel which cut off all the mighty men of valour....'

What kind of destruction was this?... It is explained in the texts of the Book of Kings and Isaiah that it was a 'blast' sent upon the army of Sennacherib. 'I will send a blast upon him... and [he] shall return to his own land,' was the prophecy immediately preceding the catastrophe...

The Talmud and Midrash sources, which are numerous, all agree on the manner in which the Assyrian host was destroyed:

a blast fell from the sky on the camp of Sennacherib. It was not a flame, but a consuming blast: 'Their souls were burnt, though their garments remained intact.' The phenomenon was accompanied by a terrific noise. (Fn: Tractate Shabbat 113b; Snahedrin 94a; Jerome on Isaiah 1: 16; L. Ginzberg, *Legends of the Jews*, vi, 363.)

Another version of the destruction of the army of Sennacherib is given by Herodotus. During his visit in Egypt, he heard from the Egyptian priests or guides to the antiquities that the army of Sennacherib, while threatening the borders of Egypt, was destroyed in a single night. According to this story, an image of a deity holding in his palm the figure of a mouse was erected in an Egyptian temple to commemorate the miraculous event. In explanation of the symbolic figure, Herodotus was told that myriads of mice descended upon the Assyrian camp and gnawed away the cords of their bows and other weapons; deprived of their arms, the troops fled in panic.

[Velikovsky also drew attention to the neglected fact that both versions - in the Scriptures and in Herodotus - include a story of a disturbance (reversal) of the sun's movement in immediate sequence with the above narratives.]

[In a chapter dealing with the folklore of the American Indians, Velikovsky relates a tale preserved by the Mnemoni tribe of the Algonquin nation. The sun had been caught in a noose and restrained from proceeding on its path:] .'..The Mouse came up and gnawed at the string...the Sun breathed again and the darkness disappeared. If the Mouse had not succeeded, the Sun would have died.' (S. Thompson, *Tales of the North American Indians*, 1929)... The image of the mouse must have had some relation to the cosmic drama...Apparently the atmosphere of the celestial body that appeared in the darkness and was illuminated took on the elongated form of a mouse...This explains why the blast that destroyed the army of Sennacherib was commemorated by the emblem of a mouse...Thus we see how a folk story of the primitives can solve an unsettled problem between Isaiah and Herodotus.

========

End of **The Velikovsky Affair** **Home**

========