

CHAPTER THIRTEEN

THE EMPIRE STRIKES BACK

The asininity of the attacks by the science media and conventional scientists upon Velikovsky was consistent with book reviewing and editorial practices generally. Sympathizers of V. had an ample data bank from 1963 onwards from which to demonstrate that V.'s critics were brash, dogmatic, imitative, narrow, selective, unprepared, precipitous, vulnerable, incomplete, pretentious, possessed, unversed, unserious, unselfcritical, prejudiced, unsystematic, inexact, unphilosophical, ideologically scatomatized, vague and irrelevant -- to say the least. Yet withal Velikovsky was said to have been "buried" not once but repeatedly, and all of his supporters with him.

In a field so broad, hundreds of major statements and thousands of details offered in over a thousand published pages somehow emerged unscathed. Several scores of statements were indicted for ambiguity or rendered more doubtful. What everyone knew ahead of time could be reasserted: the prevailing theory of celestial mechanics would only make nonsense out of data presented. In addition, planet Venus probably lacks massive clouds of hydrocarbon; if so, either such clouds were never there or they burned off over time, the latter being V.'s second line of defense.

All in all, this was so small a bag that V., when it came time to write his address to the San Francisco AAAS meeting, ended it with the words, "None of my critics can erase the magnetosphere, nobody can stop the noises of Jupiter, nobody can cool off Venus and nobody can change a single sentence in my books." He knew that last expression was bravado, but he felt like sticking it in, so unsuccessful did he consider his opposition to have been. He asked Deg's opinion: should it stay? Deg was happy for the swashbuckling septuagenarian. Besides there was enough truth in it

to let it go as the last firecracker of a speech that crackled throughout; why not? Fling it in their teeth. And so it stands. Since effectively it says nothing and says all, who can object to it?

I have given much thought to what kind of review might be tendered V. 's books, such that his supporters could not assail on substantial or moral grounds but would not please them. I consulted Professor Joseph Grace, a historian of science, and he kindly wrote a review for our pages, holding to a 700 word limit, such as is common.

"Velikovsky is a highly skilled and erudite scholar, who works comfortably in several major fields of science and the humanities. He has a style, an attack, that is primarily humanistic. By this I mean to exclude social science, which today has a format often resembling natural science, complete with jargon. He writes more like Ignatius Donnelly, a predecessor of a century ago, whose style is even more pleasurable. There can be only mild objections to such a style, considering the undefined and exotic, even occult nature of some of the areas he must venture into and the non- existence of a scientific language covering so broad an area. Of course, we would lose much in clarity and orderly communication if our students were to adopt it in all manner of writing.

Velikovsky sees prehistory and protohistory as frequented by stupendous natural catastrophes that call into question the stability of the solar system over long time periods, and therefore the gradualism of darwinism in biology. His evidence is limited and fragmentary, much of it anomalies that puzzle historians both human and natural. Most of his evidence must, and does also, serve conventional approaches, our received knowledge, although he insists upon viewing it as catastrophic.

His most radical hypotheses, which he expresses far too confidently, propose drastic erratic movements and changes of planets, particularly the Earth, Mars and Venus, not to mention the lunar satellite and the giant planets Jupiter and Saturn. The mechanics, even the electro-mechanics of such allegedly historical events are, if conceivable, quite unknown and undeveloped.

Here and there in his works one finds nuggets of valuable ore, some in history, some in legend, some natural history. One

finds these days a plenitude of studies of meteorites and comets, a few of which he cites. One finds, too, many good works on historical and stratigraphic chronology, chronometry, and it takes more than innuendo to shake the solid foundations of radiochronometry. One must be impressed, on the other hand, by Velikovsky's ability to discover anomalies and contradictions, especially in Ancient History. He may well be on the right track in discovering continuities between Pharaoh Akhnaton and Oedipus, and concordances between the Biblical Amalekites and the Hyksos conquerors of Egypt, and even is stressing a baffling absence of archeological material to fill in centuries of assigned time in Egypt, Greece, and elsewhere.

The reader will find many entertaining and suggestive pages as well. As for his general ideas, practically none of them can be fitted into contemporary scientific theory. The more heretical a theory, the more hard evidence must be found to support it, and Velikovsky's ideas of an electrically run universe, which he never develops, and his claims of planetary aberrations in early times to which he gives a great deal of attention, are, to put it mildly, bizarre; there exists, that is, no astrophysical theory to support them.

I would not recommend his books to anyone. Their pretensions will enrage the learned and confound the ordinary reader. Every age has books like them. I can mention Donnelly and Mesmer in the nineteenth century, and George M. Price and C. Beaumont in this century, but there were many more, which are best forgotten. The genre is well known to science and historians of the most ancient times, and one can judge the future of the books by what has happened to their predecessors.

The fact that a great many people read such works tells us little about their value as science or literature. No doubt, in time, such scientists as can be spared from other tasks or are involved with his specific hypotheses will build up what would amount to a total assessment. It is certainly too early to assert, as Prof. A. de. Grazia did after only a dozen years, that he is one of the great cosmogonists of the century."

What can be said for this review is that it gives a general impression of what is talked about in the books and how, and it does not challenge their right to be published, nor dismiss them as anti-scientific, nor berate the author.

When researching on the *Velikovsky Affair*, Deg stimulated V. 'S interest in the techniques of suppression, putting into a framework the host of items which protruded from V. 's archives. Deg told V. of a favorite old book, Henry Thouless' *Straight and Crooked Thinking* and explained how it might be applied to V.'s experience. V. was excited by the idea and prepared a handwritten list of "70 ways of suppressing a theory," which the two men discussed. The list that follows is largely in V. 's words and idiom. It was not included in the published work. Each item is based upon one or more concrete instances that can be documented and dated. Later on V. wished to engage Lynn Rose in fleshing out and publishing the list.

Actions of Established Scientists
and Cohorts Aimed at I. Velikovsky and his Book
Worlds in Collision (1950)

1. Refusal to read or examine the manuscript.
2. Charging it was not presented to specialists before publication.
3. Refusal to help with inexpensive tests through established facilities.
4. Accusation that work was not offered for testing.
5. Assertion that work has been disproved by tests.
6. Efforts to discourage printing.
7. Demands for censorship.
8. Engaging in censorship.
9. Boycott of the book.
10. Boycott of all textbooks of the work 's publisher.

11. Threats of reprisal against publisher by not offering manuscripts or withdrawing books.
12. Threat against associated publishers without text books.
13. Appeals to the scientific community.
14. Efforts to influence reviewers in advance.
15. Appeals to mobilize hostile reviewers.
16. Efforts to suppress favorable reviewers.
17. Efforts to supplant regular reviewers with volunteer authoritative writers as reviewers.
18. Checking the allegiance of scientists and officials of scientific organizations.
19. Firing of unaligned scientists and officials.
20. Punishment of book editors and firing.
21. Demand that there be a public recantation by publishers.
22. Refusal to print author 's papers about his books in scientific magazines.
23. Return of supplementary papers unceremoniously without reading.
24. Refusal to reprint answers to distortion of facts of reviews.
25. Misquotation from the book, and quotations out of context.
26. Copying of wrong figures into a quotation used in

the book.

27. No correction of erroneous statements in reviews by anybody in the scientific community.

28. Use of knowingly false argument.

29. Dogmatic statements and accusations.

30. Setting up and knocking down "strawmen."

31. Dishonest rejoinders.

32. Defamation and discrediting abuse.

33. Promotion of antagonistic critics.

34. Appeal to religious feelings.

35. Guilt by association.

36. Treating work by association with other ridiculed or denounced books.

37. Use of fallacious statistical method to decide whether a genius or crank wrote book.

38. Writing reviews and criticisms without reading the book.

39. Copying from other reviews (even of those who had not read it themselves).

40. Innuendoes that unneeded counterarguments abound.

41. Refusal by scientific periodicals to advertise the work.

42. Warnings against readers' inability to judge work.

43. Assuring the reading (and book-buying) public the book is dull and worthless.
44. Accusing author of using methods not actually used.
45. Denials of acts of suppression, compounding perjury.
46. Omission of credit or of footnoting the work when offering "new" theories elsewhere that are contained in the book.
47. Refusal to give credit for discoveries confirmed ultimately in tests.
48. Refusal of information to author.
49. Refusal to engage in communication with author or allies.
50. Suppression of news of disputes or debates won by author.
51. Deprecating value of crucial tests favoring author's theories.
52. Concocting stories that "1000 wrong predictions" were in book.
53. Defamation in letters and intimidation of potential support.
54. Use of great names (e.g. Nobel Prize winners) for defamation.
55. Whispering campaign; private letters.
56. Intimidation of students, both undergraduates and

graduates.

57. Elimination of the name of the heretic from books of reference.

58. Removal of the book from libraries.

59. Demands to place the book on the Register of Forbidden Books.

60. Pressure on scientific supporters by bribing with better jobs to abstain.

61. Grants given to disprove the book (no grants ever given to "prove").

62. Efforts, include fabrication, to show misuse of sources by author.

63. Damaging statements put in the mouth of deceased persons of influence.

64. Heaping of accusations without substantiation in quantities making any response impossible in the same media.

65. Insinuations of profiteering and other ignoble motives for writing the work.

66. Attempts at organizing character assassination and special meetings to dispose of the challenge.

67. Dissemination of selected damaging reviews.

68. Offering the readers arguments from specialized fields that they are unable to verify.

69. Generalization and complete disapproval on grounds of a single alleged error.

70. Accusation of lack of sources by misrepresenting the term "collective amnesia."

A service to the history and science of science would occur in the expansion and testing of the list. Deg wished that he might complete the list concerning V., then move to other cases in science, and then to all occupations to display the universal prevalence of misdemeanor, not so much to scandalize, nor to stop it all (an impossibility), as to expose to light the epidemic predicament.

When asked to place them into categories (for Deg was distressed by their stringing out aimlessly) V. divided them into: suppression of publication; punishment and rewards; examination of the theories refused; ostracism of a nonconformist; rewriting of history and scientific finds; control of criticism; unfair criticism; and unfair criticism continued by unfair rejoinders. Deg in his turn divided them into logical errors, moral offenses (cheating and dishonesty); factual errors; illegitimate demands; hyperbole; personal abuse; material sanctions; etc. V. was especially pleased with what Deg called "the absent footnote technique" which with disastrous effectiveness eliminates an undesired line of ancestors, such as V.

Stecchini in the 1970's pointed out that Schiaparelli was a leading astronomer but could not get acceptance of his idea that Venus was scarcely rotating in relation to the Sun, showing an "Earth-Lock" as it comes closest to the Earth. The "Earth-Lock" was proven a century later, but although it supported V.'s position was not even mentioned, when, for example, the *Encyclopedia Britannica* (XIX, 78) connected the phenomenon with "unsolved but very significant celestial mechanical problems connected with the origins and early histories of the planets." Here is a case of partial incorporation of quantavolution with the help of the "absent footnote technique."

The tricks used against V. were all commonplace in the scientific world. Since his work was so widely publicized and since he collected evidence so carefully, the tricks were simply more completely displayed. The more basic causes of resistance and opposition, which spawn tricks, have been discussed by Bernard Barber, with a wealth of example. V. was not a sociologist.

Allegations of meanness and nonrational thought exhausted his repertoire of analysis, except for his handy notion of collective amnesia of ancient catastrophe, which, he began to think, was the essential cause of the opposition to his theories; people, including scientists, could not bear to admit to open discussion their own suppressed terror of the original events.

But, of course, resistance to new ideas occurs whether the new ideas are catastrophist or uniformitarian, and with ideas that are false as well as with true ideas, which Barber has shown in the cases of Helmholtz, Planck, and Lister, among others. As Deg has argued, the great fear of the poly-ego in the normal schizoid human determines memory at the same time as it demands forgetting (or resisting memory), and ancient catastrophes were materially grafted onto this human mechanism; but the resistance to V. 's theories can be only slightly assigned to the peculiarities of his catastrophism.

Deg prepared another list in 1978. He was making up this one out of disgust with politics: he was gloomy over the practical impossibility of finding persons in the world who were capable of organizing, agitating and contributing to beneficial and benevolent movements. But he saw that the list applied also to getting support for scientific ideas and movements.

"Why Doesn ' t Somebody Do Something?"

Noone wants to follow
Helplessness
Hopelessness
Incompetence
Hardheadedness
General Disbelief
Indifference
Too busy, no time
Can ' t afford to, financially
Hurts somebody
Meets opposition
Arrogant to tell someone what to do
Timidity
Fear

Fickleness
Inattention and distractedness
Leave it to the experts
The crazies you have to deal with
Hard work
Resentment against being ordered about
Ignorance of particulars
Disbelief in use of force or any form of manipulation
Hatred of those to be helped
Lack of foresight
Interested only in the moment
Can ' t believe a few voices might prevail
Things will work themselves out (laissez-faire)
Fear of being corrupted
Distaste for manners of other activists
Have to work with inferiors
Suspicious of potential collaborators
Fear of physical harm
Fear of failure
Fear of being responsible for effects

No wonder nothing ever gets done!

In 1978, Dr. Henry Bauer, later Dean at Virginia Polytechnic Institute, offered the first full-dress anti-Velikovsky manuscript and the Director of the University of Kentucky Press asked Deg to read it with reference to its possible publication. Cutbacks in funds and programming forced the Press into giving up the manuscript or finding \$5000 subsidy for its production. The University of Illinois Press was finally to have brought the work out in late 1984. Meanwhile one can have a review of it by way of Deg ' s Readers

Report of January 10, 1979:

To: University of Kentucky Press, Attn. Mr. Crouch
From: Professor Alfred de Grazia
Subject: Reader ' s report to Henry H. Bauer. Beyond Velikovsky

In my opinion, Dean Bauer's manuscript should be published. It is the first generally adverse criticism of the work of Immanuel Velikovsky by a single author. The author has researched practically all available public sources. He is aware of and also adversely critical of the failings of many of the critics of Velikovsky. The book, strangely, is a likable book, which probably reflects the author's character more than the contents, which must prove annoying to a hundred people.

The book will be controversial. There is no avoiding this. Feelings run high on the scientific and sociological aspects of Velikovsky's work. The most incisive criticism is bound to come from the supporters of Velikovsky, for they are much better informed on all aspects of the controversy than the opponents of Velikovsky. These latter are usually cut down quickly. Dean Bauer realizes, though, that it is not easy to address the issues, and has the advantage of four hundred pages to explain himself and balance his analysis.

Because of the scope of the book, not only Velikovsky but also a number of his supporters will be motivated to respond. And one cannot doubt that they will have good grounds to enter the fray. Let me take myself as an example of what may very well happen with others. On p. 236 the author mentions my "utter conviction that Velikovsky is right." Right about what? I am favorable to his general theories, his genius, and his defense against the almost invariably misplaced attacks upon him. Bauer might well stress his distinction between the "True Believers" and the scholarly supporters. Among the latter, there are many differences, the atmosphere is highly critical and, if they seem overprotective of Velikovsky, it is because the enemy outside is so massive and aggressive. It will add greatly to the clarity of the analysis if the author distinguishes the scholarly supporters and the lay supporters. (The word "public" is better but unfortunately has several meanings.) The scientific opponents of Velikovsky have also their scholarly and lay supporters. As for disputes among the scholarly supporters and Velikovsky, contrary to Bauer's statements, there are dozens, beginning with Juergens, Hess, and Stecchini and ending with the young writers in the current (Nov. 1978) issue of the *Society for Interdisciplinary Studies Review*.

At the bottom of p. 237, Bauer shoots from the hip at both Juergens as an absurdity and myself as a political scientist, while favoring physicist Kruskal's scornful attack upon Juergens. This does not accord with Bauer's many comments

upon dogmatic remarks and against extolling specialized authority. Apart from whether he understands Juergen's theory, which he does not bother to demonstrate, and whether I understand Juergen's theory as well or better than Kruskal, he takes up a vulnerable position: what qualification, one might ask, does Bauer have for writing a book of sociology, history, ethnology, and political analysis, not to mention meteorology, geology, astronomy, etc.? Does he regard himself as a greater polymath than any of us?

Then again, he contradicts my analysis of Margolis and a group of Yale reviewers, claiming that his own count in the first instance is at odds with my own. Perhaps he should reproduce, in a couple of pages, the Margolis article with my comments, adding his own. Such would be the better way to damage my conclusions. The readers might then judge.

And so on. To say only of the distinguished group of scholars who passed on the *ABS* special issue on the *Velikovsky Affair* that none was a scientist gives a completely misleading idea to the reader. Lasswell was one of the founders of quantitative method in behavioral science. Cantril was a distinguished psychologist and expert on systematic opinion analysis; etc. Nor does he stress that Harry Hess, who is sometimes regarded as having been the leading geologist of the past generation, was a thoroughly sympathetic friend of Velikovsky. Hess and I talked on two or three occasions of Velikovsky, and Hess was as eager as I to see Velikovsky's scientific ability respected. Hess recommended that his students at Princeton read *Earth in Upheaval*, for example. These are but a few of the hundreds of points of contention in the manuscript and yet I feel it should be published with only modest changes, because it might otherwise take years to redo it and I am not at all sure that the public functions of the book would be greatly assisted. Perhaps I am saying that the book as it stands invites a full rocket display and, in the process, the public, science, and students will become better educated. I doubt that any amount of revision will make it a definitive and conclusive answer to the rapidly developing body of work sympathetically or willy-willy aligned to Velikovsky's books. I have four books in process myself that are more controversial and upsetting to the established doctrines of contemporary science than those of Dr. Velikovsky. But I have the impression that I shall not encounter the same type of opposition as Velikovsky if only because the intellectual atmosphere has changed so much and in part because of the *Velikovsky Affair*.

Readers perhaps will little note the criticism directed at myself and some others in the book, but they will be alert to a number of points respecting Velikovsky, and I would suggest that Dean Bauer reconsider them. He is attacking Velikovsky in 1979 partly on the basis of a pamphlet that Velikovsky published in 1946 ("Cosmos and Gravitation") and which Bauer even appreciates is not pushed by Velikovsky himself or scarcely anyone else. True, Velikovsky hates to recant, but the pamphlet is not a necessary prologomena to the later books. Indeed, Bauer's often insightful views about Velikovsky's character and motives should make him wonder whether the pamphlet was not merely a brash preliminary exercise, which vanity demanded be published as advance claims. Further it has become fashionable now to predict the doom of the concept of gravitation, and Velikovsky's musings were in a way the fashions worn in 1946 for anti-gravitational thought. This might be said also regarding the model of the atom as resembling the solar system. Only lately has that idea become discredited. Are we to dump all scholars who early in their careers exhibited what was currently believed? Then everyone will have to walk the plank.

Bauer sometimes abuses Velikovsky, contrary to his professional aim, generally observed, of avoiding inflammatory and *ad hominem* statements. It should be easy to revise such expressions as "astonishing ignorance" (p.159), "supreme ignorance" (p.154), p.161 etc. I think that he would reap rewards if he, or an editor, were to erase fifty to a hundred non-functional adjectives or phrases.

And, in respect to Velikovsky as a knowledgeable scientist, aside from "who is a scientist besides the self-elect," Bauer underestimates Velikovsky totally. Let him ask Burgstahler (chemist), Motz (astrophysicist), someone like myself who knew Hess (geology), Hadas (linguistics), Lasswell (psychiatric psychologist), Cyrus Gordon (Near East Studies), Einstein (physics), Juergens (electricity), *et al.* Every last one will or would say that Velikovsky is not only a good scientist, but an imaginative one, and at home in a number of fields. I wonder why Bauer did not take the step to include himself in this group by interviewing the subject of his book. Velikovsky may be in error, but he is a scientist.

Also, I would recommend dropping the discussion of whether Velikovsky is a crank. Bauer admits that he himself is a crank,

about the Loch Ness monsters. It's unworthy of this book to waste itself on this unscientific concept. I would, as Dean Bauer appears to believe, devote only several necessary paragraphs to exposing the term "crank" and kicking it out of bounds.

On p. 248, I note a striking contrast between a group of pro-Velikovsky publicists and a group of anti-Velikovsky scholars of distinction. This is a "foul blow." Either let both be publicists or both be scholars.

So, I should conclude that off-hand abusive terms ought to be excised since they take away from a book some of its good air of casual and pleasant inquiry. Cut back the section on cranks. Perhaps dispense with the sections on "Cosmos and Gravitation" save for a simple statement of its inappropriateness and its inelegant foreboding of things to come. The admirably clear piece on gases should win Bauer an excellent contract for an elementary textbook in general science, but may not belong here. Perhaps other paragraphs can be removed here and there at the instigation of a generally well-educated lay reader.

The style is clear at the college level. Many, many things are said that need to be said about both sides: about how scholars are just (simply) people; about how the general public reacts to controversies in science as to political struggles, baseball games, etc.; and about the foibles of Velikovsky (though perhaps not enough, regrettably, about how these foibles have had something to do with driving him on relentlessly and with good effect). And I think that Dean Bauer might even, in the end, bite the bullet and state that on the whole it were well that Velikovsky's books were published, then bad that they were mishandled by the press, scientists, and disciples, yet good that a million people began to read into history and science. Finally take the word of the author himself (p. 366) that an astronomer's statement that "Velikovsky's scenario was impossible on grounds of celestial mechanics was just not so." That is worth something and will win the author a medal for courage, after all is said and done.

To avoid rumor-mongering or a delayed denunciation Deg told V.'s retainers of the existence of the work and of his recommendation that it be published. "Why?" he was asked, meaning why didn't he stomp it. It's not bad, he answered, you'll

see, and it will keep the dialogue going, even improving it.

Meanwhile, those who were termed by the anti-heretics "devotees," "followers," "disciples," "supporters," "sympathizers," and were consigned to the limbo of science as "benighted," "anti-scientific," "occultists," "astrologers," "fanatics," and so on, unendingly -- from these who were seriously considering his work as well as doing work of their own, came the discovery and reporting of his errors, qualification of his statements, essays at quantification, adduction of contrary materials, tempering, amending, and explaining. We need not go into the question, "Whose mass of supporters is better -- yours or ours ?" We are saying precisely that the effective scientific criticism of Velikovsky came from those who were sympathetic to his work.

It was the heretic scholars who designed alternative scenarios, in geology and astronomy, who upset V. 's chronology beyond the Eighteenth Dynasty of Egypt, who pointed out correctly evidence of pro-Biblical bias, who disputed his identification of the astronomical bodies implicated in certain legends, who pinned down the sources of numerous uncertainties, who reduced vagueness, who found and accommodated predecessors in the esoteric and difficult literature of catastrophism, far beyond the sporadic dark hints that "nothing new" was being proposed.

To be blunt, if you want to know what 's wrong with Velikovsky, ask his friends, as much as his enemies; ask his admirers, as well as his detractors. You must know the literature of quantavolution and catastrophe. It is contained by now in many books and hundreds of correctly postured articles, many old, many new, many forthcoming. One can think no longer, if ever, that by "not believing in Velikovsky" science will proceed on its customary paths; a growing parade of many different kinds of quantavolutionaries is finding its own paths. The parade cannot be dismissed by uttering an imprecation against Velikovsky.

The *Bulletin of Atomic Scientists* had been established in the triumphant days of nuclear physics following the blast at Hiroshima

and was dedicated to voicing the responsibilities felt by scientists. Like the playboy college students who excused his poor grades on grounds that his college was anti-semitic and who persuaded his father that his nose, his curly hair, and his name ought to be changed, whereupon, his grades remaining poor, he had to confess that 'us Gentiles ain't very smart,' the Bulletin did change its name for awhile and had the same old problem so it changed it back again, but at this time, around 1964, was trying to boost its popularity by exposing what Editor Rabinowitch regarded as scientific impostors, and his chosen weapon, a science publicist named Margolis, settled upon Velikovsky, whence was published a cavalier article entitled "Velikovsky Rides Again."

Deg's larger and more detailed refutation of the offensive article is reproduced in *The Burning of Troy*. So here I may introduce a letter in the same vein from Eric Larrabee, a publicist and early supporter of V., later head of the New York State Arts Council.

April 21, 1964

To the Editor:

The "Report from Washington" by Howard Margolis in your April number is a mixture of intemperate accusations and misstatements of fact. Margolis dismisses as "hokum" the work of Immanuel Velikovsky, which he has demonstrably read without care and judges without experience. He claims there is "no scientific way to examine" books which abound in references to physical fact. Their author had furnished specific scientific tests of his theory and on all of them to date, according to Professor H.H. Hess of Princeton, he had been vindicated. Margolis brushes off Velikovsky's successful predictions as "science fiction" and offers instead the results of his "few hours" reading in philology and history.

He can apparently read neither French nor Hebrew. If he could read. French he would not speak of the "actual" inscription at el-Arish in words from the

outdated English translation of 1890 instead of the modern French translation of 1936, which is plainly cited in Velikovsky's footnote. The French translation gives the name Pi-Khirote. Margolis is flatly wrong in stating the Velikovsky "alters" the text, either here or in the case of the biblical pi-hihiroth (so spelled by Velikovsky in *Ages in Chaos*, p.44). If Margolis had read even the English translation attentively he would have found "King Tum" (The French gives "*le roi Toum*"). This is the text: "*Voici que Geb vit sa mere qui l'aimait beaucoup. Son coeur (de Geb) était négligent après elle. La terre -- pour elle en grand affliction.*" It goes on to describe "upheaval in the residence" and "such a tempest that neither the men nor the gods could see the faces of their next." The inscription is shown to be historical by the fact that the King's name is written with the royal cartouche.

Velikovsky's reasons for suggesting that *bkhor* (firstborn) in the Hebrew text might be a misreading for *bchor* (chosen) are given at length (*Ages in Chaos*, p. 32-34) and are not essential to his argument that Exodus and the Egyptian sources refer to the same natural catastrophe. He uses the word "obvious" in proposing that the phrase "to smite the houses" refers to an earthquake in view of the fact that Eusebius, St. Jerome, and the Midrashim all confirm this interpretation. Margolis' sarcastic repetition of the word "obvious" is wholly without justification.

Margolis accuses Velikovsky of saying that St. Augustine puts the birth of Minerva at the time of Moses whereas Augustine "says the opposite." This would be a serious charge if true but it is doubly untrue, both as to Augustine and Velikovsky. The relevant passage in *The City of God* (Book XVIII, Chapter 8) reads that Minerva was born in the time of Ogyges and Velikovsky quotes it (*Worlds in Collision*, p.171) in those precise words. In support of

the damaging assertion that Velikovsky alters evidence Margolis alters the evidence from both sources.

Margolis cannot even read Velikovsky correctly. He says that Velikovsky "can cite no description" of Venus growing larger in the sky despite the fact that on pages 82-83 and 164-65 of *Worlds in Collision* it is so described from Western ("an immense globe"), Middle Eastern ("a stupendous prodigy in the sky") and Chinese ("rivalled the sun in brightness") sources.

The sociological interest of the Velikovsky case lies in the willingness of scientists to dismiss the work of a serious scholar as "hokum" on the basis of slipshod, inaccurate, and abusive criticism. Margolis had proved once again that the interest is justified.

Eric Larrabee

Deg was in an ornery mood and had threatened the *Bulletin* with a suit for slander. V. was all for the idea consulted his friend, the libel expert, Philip Wittenberg. Deg also consulted Herbert Simon and adopted Simon 's view, as expressed in the letter below:

Dear Al,

I have read the materials you sent me about the Velikovsky matter. (Incidentally, I lunched with Velikovsky last week, and we are going to have him back to the campus next autumn for a lecture.) I have a few comments to offer on the matter of strategy.

As I am sure you know, there is a doctrine in the law of libel known as "invitation to comment." Anyone who performs publicly -- and that includes publishing a book -- invites critical comment, and has no recourse if he gets it unless he can show actual malice. The critic does not, in general, have to sustain the burden of proving truth. (I may have forgotten details, but your lawyer will tell you that that is the general idea.) Two consequences follow from this: (1) one should not publish books -- or issues of the *American Behavioral Scientist*

devoted to the *Velikovsky Affair* -- unless one has a thick skin; (2) when one is flayed by a critic, one should almost never threaten legal action, however righteous one 's feeling.

The opponents of Velikovsky are not malicious, they are indignant. Nothing about the Margolis article seems to me libelous, however much I disagree with it. We certainly do not want to imply that *we* wish to suppress his right to hold, or even publish, these opinions, however much anguish they cause us. Hence, if I were editor of the *Bulletin of the Atomic Scientist*, I would politely but firmly reject your request that I "withdraw my support" from the article. He might even point out that to an anti-Velikovskyite, some of the language in the September *American Behavioral Scientist* might seem quite as offensive as Margolis ' language did to you. C ' est la vie.

When you receive the refusal from the editor -- as I am sure you will -- I would advise that you then request an opportunity to have three pages in BAS to reply to Margolis (perhaps offering the same number of pages in *ABS* for a rebuttal to the September articles). There is nothing to be lost by a public discussion of the issues, especially the issue of freedom to publish, and nothing to be gained by defending that through threats to suppress it.

With best regards,

Cordially Yours,
Herbert A. Simon
Professor of Administration
and Psychology

After much deliberation and testing of the winds, Rabinowitch wrote to Deg:

25 June 1964

Dear Mr. de Grazia:

In answer to your letter of May 12, I do not see why, and in what form, the *Bulletin* should "withdraw its support from the article of Mr. Margolis." I do not understand what you mean by "your contributors and advisors urging you to take action to remedy the wrong done us." The responsibility for the contents of the articles published in the *Bulletin* rest (sic) with

authors of the articles. It must be obvious, of course, that the magazine cannot disclaim legal responsibility for any defamatory statements, but I do not see in the article by Mr. Margolis any statements of such nature with respect to yourself or to the contributors of your journal. If all polemics over matters of scientific competence would end in court, this would be bad indeed for the climate of free discussion in this country. In our society, the enemies of evolution can call scientists, espousing this theory, ignoramuses, or heretics; the enemies of fluoridation can call the medical authorities supporting it whatever like names they might choose -- short of character assassination -- and the proponents of fluoridation can do the same to their critics. This is as political processes should be in a democratic society.

In his article Mr. Margolis, after dealing briefly with the astrophysical difficulties of Velikovsky's theory, expanded on the interpretation of ancient texts. From the point of view of the *Bulletin* the physical and astronomical evidence is crucial, and the considerations of what Velikovsky calls "experience of humanity," can only be subsidiary. Physical evidence is simpler and more unambiguous; while interpretations of old texts and hieroglyphic inscriptions is a tentative and often controversial matter.

Since Mr. Margolis brought up the paleographic evidence in his article, we must in all justice, permit Dr. Velikovsky (or a spokesman for him) to point out the errors, if any, in his argument. This should be done by someone with first-hand experience in the field -- either Dr. Velikovsky himself, or even better, some independent recognized authority in Biblical history and ancient languages. We are willing to publish such a letter in one of the forthcoming issues (giving Mr. Margolis the opportunity of answering it, if he desires); but, we will then terminate the discussion, since Egyptology or Old Testament studies do not represent a field of the *Bulletin*'s major interest.

As far as physical possibility of the events suggested by Velikovsky is concerned, I mention the names of Menzel and Shapley because I remembered that they did analyze Velikovsky's theories at the time of their publication. I would be glad to have any other recognized astrophysicist or geophysicist (including the Princeton and Columbia astronomers who have pointed out in *Science* the correctness of some of Dr. Velikovsky's specific predictions), to present in

the *Bulletin* briefly what they think of Velikovsky ' s theory as a whole.

I believe it is a mistake to accuse modern science of intolerance to the theories which destroy its accustomed frame of reference and force it to revise its foundations. Einstein proposed a revision of Newton ' s conceptions of time and space; for a few years, there was some resistance of the type suggested by you, but it was silenced by Einstein ' s explanation of the precession of the perigee of Mercury, and his prediction of the bending of stellar light in the neighborhood of the sun. If the correct predications by Velikovsky, pointed out by Hess and others, do not change the general rejection of Velikovsky ' s theories by scientists, it is because changes in the laws of celestial mechanics and revisions of well-established facts of earth history, required by Velikovsky, are quite different from the subtle, but logically significant and convincing changes in the scientific world picture suggested by Einstein (as well as by Mac[sic] Planck, when he postulated the atomic structure of energy, or more recently by Lee and Yang when they postulated a physical difference between a right and left screw, object and mirror image). Modern science has learned to be open-minded to revolutionary suggestion, *if* they are brought up with strong scientific or logical evidence. Reluctance to go along with Velikovsky ' s *Worlds in Collision* is, in my eyes, evidence not of stubborn dogmatism of "official" science but of the physical and logical implausibility of his theories.

Your letter and its request misinterprets the position of the *Bulletin*. To conclude, since Mr. Margolis brought up paleographic evidence, fairness requires the *Bulletin* to give space to a letter disputing this evidence (provided this letter is not more abusive than Mr. Margolis ' criticisms). If Dr. Velikovsky can suggest a recognized authority in astrophysics or geophysics willing to discuss his theory as a whole in the light of recent verification of some of his predictions, I would consider giving space in the *Bulletin* for a brief discussion of this kind.

It is in this spirit of scientific argumentation that the whole problem should be resolved.

Sincerely yours.

Eugene Rabinowitch
Editor

During the next few weeks Deg drafted a brutal reply to Margolis 's article and prepared a letter to accompany the critique. However and meanwhile, V., ever hopeful of access to and acceptance by the authorities of physics, prevailed upon Harry Hess to submit on his behalf to Rabinowitch an article he had prepared on his Venus theory in the light of new findings. It would serve as a counter weight to the Margolis article, without reference to the libertarian and legal issues involving the *Bulletin*.

In September Rabinowitch wrote to Hess, returning V.'s manuscript without having read it and saying, "the *Bulletin* is not a magazine for *scientific* controversies -- except on rare occasions (e.g. in the field of genetic radiation damage) when they are directly related to political or other public issues... Neither is it the function of the *Bulletin* to provide an outlet for scientific theories not recognized by professional authorities in the field." He explained the Margolis article as an attempt to undo the work of "behavioral scientists" in aid of V. whom, he said, they "championed in the most violent way."

In October, the *ABS* published Deg 's critique of Margolis, and Deg sent it to Rabinowitch along with the letter that he had drafted three months earlier.

November 12, 1964

Dear Mr. Rabinowitch:

Please permit me to answer frankly your letter of June 25, which asks *why* and *in what form* you should "withdraw your support from Mr. Margolis 's article about us."

The *why* should be apparent in the attached analysis of Mr. Margolis ' writing, entitled "Notes on 'Scientific' Reporting." This explains in detail the errors, the malice, and the legal offenses of Mr. Margolis. Unless you can by the use of evidence and reason erase those 54 notes, you are bound scientifically, morally, and legally to "withdraw your support."

In what form should you "withdraw your support"? You should "withdraw your support" by expressing in seven

columns of space in your magazine (1) your acknowledgment of the excessively large number of factual errors contained in Mr. Margolis' article, and (2) your regret for the incorrect unjustified slurs upon the character and motives of Dr. Velikovsky and the contributors and editors of *THE AMERICAN BEHAVIORAL SCIENTIST*, together with your hope that your reader should join you in repairing in the course of time such damages as was caused by this article.

My present letter could now end, as might have your own at the same point. However, you go on to make further comments that require answer.

You say that it would be "bad indeed for the climate of free discussion in this country" if "all polemics over matters of scientific competence would end in court." I answer that "all polemics" are not at issue, but only *one* polemical action. (You are of course, at liberty to universalize its meaning.) Moreover, "the climate of free discussion" that you mention has been clouded and cannot be logically cited as a reason for staying out of court. It is precisely to get people out from under this cloud that the law and courts are built. The courts enable an objective determination to be made of a matter in certain cases where free discussion is impossible. They permit and require the calling and interrogating of witnesses under just conditions. They prevent and remedy the abuses that you have presumably endorsed. The law of evidence and the rule of law, Mr. Rabinowitch, are the grandparents of the scientific method. They are not its antithesis.

You say that in our society, disbelievers in evolution can call scientists espousing evolution ignoramuses or heretics. You say enemies of fluoridation can call medical authorities supporting it like names and *vice versa*. You are defending your magazine evidently for assuming the privilege of such name-calling as opponents of fluoridation and evolution employ. Very well. Your reader must judge you for that.

"Character assassination", you say, is *not* permissible, however. The issue here is of course just that. I call to your attention the numerous instances, well-noted in the aforesaid memorandum on "54 ways", in which your magazine is guilty of character assassination, slander, and libel.

Your next paragraph is logically queer, for you say that the *Bulletin* is largely concerned with the astrophysics of

Velikovsky and not with the humanistic evidence.(I will not tarry with your incredible distinction between physical and humanistic evidence.) But then you go on to admit that the *Bulletin* reversed itself and abandoned its chosen field in this case. (Apparently, any and every policy can be reversed to get at Velikovsky. How true we were!)And you say you want to get the historical evidence argued. Argued -- but not too much you state, for you have to get back to your major interest! Like UN affairs? Like scientific freedom? You may go back to your affairs, Mr. Rabinowitch, but not before we are done with the matter.

Now you would graciously permit Dr. Velikovsky or an "independent authority" of the classics to answer Mr. Margolis by a letter, to be followed by a reply from Mr. Margolis, and then *stop!* Two-to-one is bad enough. But how does Mr. Margolis deserve this reply? By his own expertness as a biblical scholar, specialist in ancient languages, and classical historian? I submit that this exchange might be equal and appropriate if I might delegate my daughter who is majoring in archaeology at Bryn Mawr to take up your invitation to reply.

A general appraisal of Dr. Velikovsky 's theories in your paper would be a good idea, as your suggest, and I think you should find a set of scientists to make such an appraisal. I would not go to Drs. Menzel or Shapley, whose participation in the Velikovsky case, as documented in *Harper's* and *The American Behavioral Scientist*, has been most unbecoming Your hazy remembrance of their posture is scarcely a firm basis for risking the reputation of your magazine and colleagues. Besides the balance of evidence has continued to shift between 1950 and 1964. Do read that document; you must take the time : you and your writer cannot decently continue to ignore all the factual record of the case.

Still, all of this is not the central point, which is the behavior of scientists, and you do well to return to it in your last two paragraphs. There you first say that modern science is not intolerant of unorthodox theories. This is not so; even the case you cite, Einstein, was in your own words victim of "some resistance" of the type the *ABS* described. But even if it were so *generally*, why would you unscientifically and dogmatically refuse to recognize an "unusual" case of resistance when it loomed before you?

How can you say that the actions taken concerning Velikovsky

and his theories was tolerant? Please state one procedure, whose value you would defend, for that reception and consideration of new scientific material, which was followed by the leadership of science in the Velikovsky case. Show us that he was given one key to the kingdom. I believe, as you seek to do so, you will gradually eliminate from consideration all the decent and rational procedures that are supposed to govern the behavior of scientists. In the end you will either be indignant or a cynic. You will not be the Rabinowitch whose letter I am replying to.

I must end in laughter, which I hope you will forgive. For you conclude by permitting Dr. Velikovsky to answer by letter "provided this letter is not more abusive than Mr. Margolis' criticisms!" I am not clear whether you are here defining the outer limits of abuse, or whether you suggest pursuing scientific truth by balancing two sets of slander.

Go back to my beginning, sir; you will find our two requests to be generous offers made in the veritable "spirit of scientific argumentation" that you appeal to.

Sincerely yours,

Alfred de Grazia

Dear Mr. de Grazia:

Thank you for your letter of November 12th. I can only add my appreciation that you published the full Margolis article in *The American Behavioral Scientist*. Your readers may judge.

Sincerely,

Eugene Rabinowitch
Editor

December 3, 1964

Dear Mr. Rabinowitch

We acknowledge your appreciation of our fairness. Does your appreciation mean that you, too, will be fair to us and present

our rebuttal before your readers?

Sincerely yours,
Alfred de Grazia

The rebuttal was *not* carried by the *Bulletin*. A great many scientists had their prejudices reinforced at the expense of V., Deg, and the *ABS*. In the final analysis and many year later, Deg's indignation seems overdone, and it is doubtful that he ever had the intention of suing, but he was up to his typical game of driving home contradictions and pounding away at the basic homology between legal and scientific procedure. Furthermore, while discounting his rhetoric, I should also call attention to specific instances of the damage caused by irresponsible behavior in scientific circles tied directly to the *Bulletin* article: one on the matter of fluoridation, on an exchange between Urey and Deg, and two to be treated in chapter 15 on "The Knowledge of Industry" involving the Sloan Foundation, Moses Hadas, and a project of Deg in economics.

July 17, 1996

Dear Professor de Grazia:

Since writing you earlier in connection with my review of "A Struggle With Titans, " I have been reading the various documents cited in "*The Velikovsky Affair*."

One that particularly "struck" me was the article by Howard Margolis in the April 1964 issue of the *Bulletin of the Atomic Scientists* that you ably dissected in the October 1964 issue of the *American Behavioral Scientist*.

What came as an even greater surprise, however was the article written by Margolis about fluoridation in the June 1964 issue of the *Bulletin of the Atomic Scientists*. By failing to take note of published reports of toxic effects from fluoridated drinking water, he constructs a very favorable case for fluoridation and makes his opponents appear to have no scientific grounds on which to oppose it! Since you were able to show that Margolis is not a good philologist, I thought it might be worth pointing

out that he also has not read the fluoridation literature very thoroughly. The major documents he cited to support his view are guilty of omission just as he is. The one that was prepared in 1955-1956 is hardly relevant to "current" findings, while the "Select" bibliography is no more than a compilation of proponent research, with virtually no mention of contrary results reported by others, especially in relation to clinical findings.

I realize your interests lie primarily in the area of the "sociological" aspects of a subject like fluoridation, but the strong scientific evidence against fluoridation has been kept so heavily suppressed that there is a close parallel to "*The Velikovsky Affair*." Our own local public library, I might add, has refused to accept or acquire a copy of "A Struggle with Titans" on the grounds that the standard reviewing media have ignored it -- just as they are ignoring "*The Velikovsky Affair*"!

Sincerely yours,

Albert W. Burgstahler
Professor, of Chemistry
The University of Kansas
Lawrence, Kansas

June 2, 1964

Dr. Alfred de Grazia
The American Behavioral Scientist
80 East 11th Street
New York 3, New York

Dear Dr. de Grazia:

I am sorry to see that you have gotten mixed up in the Velikovsky case. Velikovsky was a charlatan. There is just no doubt about it at all. It is not true that outstanding astronomers would not welcome a truly original man with constructive ideas. We would put him on the staff of the University of California San Diego. I do think that you should try to withdraw from this controversy as gracefully as possible and not continue it. I assure you that every physical scientist of my acquaintance will rise to defend the *Bulletin* against anything you do.

I am terribly concerned at present about the lack of control in

scientific publication. Science had always been aristocratic. Not everyone could get his ideas published in effective journals. Articles to the scientific magazines have been carefully edited, and unless they conformed to reasonable scientific standards they were refused. Today anyone can publish anything. In the first place, very second-rate scientists can get jobs somewhere - - with industrial companies, government agencies, the space program, etc. They all have their private printing press in the back room, namely a reproduction device. As a result, papers of all sorts are sent out. Also there are new journals springing up with no decent editorial control whatever. The result is an enormous amount of confusion. In fact, as I have stated and I now repeat, there is often so much noise that one cannot hear the signals.

With best regards,
Very sincerely,
Harold C. Urey

Deg' s Journal, June 29, 1964

...Velikovsky had palpitations last week. For several days his pulse was irregular. He has gone into a three day period of rest and is taking a little tranquilization by drugs. He has been traveling too much and spending too much time trying to direct strategy in his scientific defense. A letter I received from Harold Urey depressed him greatly. Identifying as he does with authority, V. is hurt when a Noble Prize winner for chemistry refers to him as a charlatan. What can he be expecting? I have not been able to educate him to the sociology and political science of science. He believes in rationalism and that other experts only by odd mistake "because they haven't read his works," treat him so contemptuously and with hostility. V. wrote what he thought should be my reply. (Sometimes his presumption becomes arrogant.) It was a strange letter, full of pathos and humble remonstrance. I could not and would not use it. It is an interesting document about V. himself. It would do him no good even if I were to use it. Yet he was deeply perturbed when I informed him I was sending my own letter of reply. He claimed that his was a perfect letter, which he was proud of, and felt must be sent. It was then I learned of his palpitations. The thought occurred: the strangeness of this letter goes with a nervous disturbance. He desperately wanted me to send his letter; he mailed it by special delivery to New York where I was and phoned to press me about it. In a week or two, when his illness is passed, he may be secretly pleased

that I went by own way.

I spoke later to his wife. She seemed displeased with me too. She, too, will come around. She confirmed how "hurt" he was by the Urey letter. Urey is a -----! What better could come from him. His letter to me is a disgrace and I mean to call it that.

July 8, 1964

Dr. Harold C. Urey
School of Science and Engineering
University of California, San Diego
P.O. Box 109
La Jolla, California 92038

Dear Dr. Urey:

Thank you for your letter of June 2. I appreciate your concern that I may "have gotten mixed up in the Velikovsky case." Since everyone whose attention is called to the case has gotten mixed up in it, in one way or another, I guess that I am in good company.

Your second sentence is that "Velikovsky was a charlatan." He neither "was" nor *is* a charlatan. Resort to your nearest dictionary will satisfy you on that score. If you insist that you have not made a linguistic error, then you must give me *one*, just *one*, bit of evidence to support your allegation. Indeed, your next sentence is "There is just no doubt about it at all." Since you are a scientist and know the nature of proof, you must have a great many pieces of evidence, adding up to certainty. If you cannot cite such evidence, then you must apologize to Velikovsky, or you become yourself a charlatan and slanderer.

You may refuse this challenge. Very well. We do not usually carry substantive discussions of factual theory in the *American Behavioral Scientist*, but if you will honor us with one significant error of fact or logical contradiction in Velikovsky's works we will print it and let it go at that, for we are not concerned to solve the problems of physics and astronomy, or politics and economics in our pages. I know that you will have no trouble with this small matter; I could probably manage it myself; that Mr. Margolis could not succeed, nor some others who tried, does not prove that the works are flawless.

Then you say, "It is not true that outstanding astronomers would not welcome a truly original man with constructive ideas." I am afraid, Dr. Urey, that you will be hard put, in the light of the history of science, to maintain this statement also, unless you would again resort to evasive semantics, defining the words "truly original" and "constructive" to suit your ends. Your saying that "we would put him on the staff of the University of California, San Diego" could be regarded as an idle threat if it were not for the well-known anxiety of certain California colleges to discover warm bodies wherever they may be.

You thereupon urge me to withdraw from the controversy. Actually, I had done so; but the stupid brazenness of the *Bulletin of Atomic Scientists'* article brought to me a sharp realization that many of your kind simply will not learn. "Ontogeny recapitulates phylogeny:" every error of the scientific mind and spirit in the history of the Velikovsky case was by almost preternatural skill recomposed into a few columns of the *Bulletin*. This you ask me to swallow!

The controversy will continue. You say the "every physical scientist of my acquaintance will rise to defend the *Bulletin* against anything you do." Perhaps you will not have as many acquaintances as you claim and they will not be willing to act as your troop if they, or at least several of them, were to read the pages of the *American Behavioral Scientist* and compare them with the article of the science correspondent of the *Bulletin*. (Isn't it interesting that the scientists' *Bulletin* should have to hire a non-scientist to write about science for them?)

You have, it is clear, a rather horrifying vision of science. You gently threaten me, you promise to bring in your gang, and then you begin to reveal the utopia that occupies your mind. "I am terribly concerned at present about the lack of control in scientific publication," you write; "Science has always been aristocratic. Not everyone could get his ideas published in effective journals. Articles in the scientific magazines have been carefully edited, and unless they conformed to reasonable scientific standards they were refused. Today anyone can publish anything."

I, too, Dr. Urey, am concerned about scientific publication. I am not, however, concerned about the lack of control by the scientific oligarchy, as you are, but by the lack of

communications, the haphazard and chaotic situation that is caused as much as anything by a defective leadership in the sciences. Your kind of scientific aristocracy is precisely the reason why your subsequent claims are laughable: if there is any villainous theme in the history of science, it is the continuing attempt to deny a voice in the organs of science to iconoclasts, outsiders, and just plain *kleine Menschen*.

You will be responsible for retarding the progress of science if you succeed in reestablishing the old system of information controls. You should turn your attention to organizing scientific information rather than to suppressing it.

Similarly you should be pleased that more of our working population today are scientists, rather than coalminers or ditchdiggers. Indeed you seem to be angry with them for pretending to perform the same operations as are practiced by you happy few. "...Very second-rate scientists can get somewhere -- with industrial companies, government agencies, the space program, etc. They all have their private printing press in the back room..." Einstein with his patent-office job, Da Vinci doing his civil engineering, Freud setting up his own printing press, Darwin idling on his patrimony -- there certainly are a great number of these second-raters, without university chairs, not content to eat common fodder and let their intellectual ambitions expire peacefully!

I am beginning to see your point. You would wish only first-rate scientists such as Howard Margolis, formerly a science writer for *The Washington Star* and now correspondent for the *Bulletin of Atomic Scientists*, to have freedom of scientific expression. Your idea would be to have a kind of Empire such as Alice discovered in Wonderland where the knighthood of science is conferred by your power elite and the Sir Margolises can be sent out to harry any peasants who may have the temerity to poach upon the truth.

Your conditions for peace are not acceptable, Dr. Urey. Our condition is that science be open and public, and remain so. If you wish to alter your conditions substantially we would be pleased to hear from you again. Meanwhile, with regards to your work on tektites, I remain

Respectfully yours,

Alfred de Grazia

The special magazines given over to reporting and supporting V.'s doings have been *Pensée*, *Kronos*, and the *Review of the Society for Interdisciplinary Studies*. Each of these has carried extensive materials on the preliminaries, proceedings and aftermath of the American Association for the Advancement of Science convention panel dealing with Velikovsky's ideas at San Francisco in February 1974. According to astronomy Professor Ivan King of the University of California at Berkeley, it was Carl Sagan who suggested the confrontation. It was intended that the panel be divided into supporters and opponents of V., but over a period of months, the pro-V. nominees were weeded out. This was suspicious, and I am inclined to cast suspicion on both sides.

In the first place, both the establishment (for it can be called such also on these occasions when it puts on a face) and the heretics chose a deceptive yet revealing title: "Velikovsky's Challenge to Science." V. would never allow himself to be called a non-scientist; yet, to have his name in the limelight, he allowed himself to be juxtaposed to science. Simultaneously, the establishment (that is, the government *ad rem* in charge of the state of science), in order to isolate the heretic, allowed the personalization of the panel, in itself an abuse of the scientific method which addresses itself to ideas, not men. Might not a better title have been "The Validity and Prospects of Neo-catastrophism"? Then with eight papers, four on each side, the topics of the mechanics, the electromagnetics, the historical record, and the reception of neo-catastrophism in science could be taken up.

Did V. want to appear without support on the stage, keeping the spotlight, whether for the hero or the martyr, upon himself, and therefore did he not fight hard enough to ensure himself that support? He ended up with two neutral parties, the opposition of a biased chairman, and three convinced antagonists eager for the fray. Surely there must have been some masochistic force at work in him, coupled with an extremely clever Machiavellism: a pro-Velikovsky paper would do nothing for V.'s image as a great scientific loner and martyr.

If the one man who knew the Venus historical record best, Lynn Rose, had been present, he could have devastated, on the spot and

forever after, the presentation made by Huber. It would have been ineradicable from the book that followed, entitled *Scientists Confront Velikovsky*. If Juergens had been forced into the panel by V. then Mulholland would have been finished off. If Deg had been invited, he would probably not have gone, but if he had, he might have effectively harried Sagan and Storer, considering what these two ended up by saying. Then V. would have been off and running.

Instead, it was a gruesome exercise at V.'s cost, then and thereafter. He behaved magnificently, like Samson dragging down the temple of the Philistines upon himself. He won the crowd. The press, ignoring the crowd, and incapable of reading the papers, pronounced him dead. V. did not really go to San Francisco to have the crowd be with him. He went there to gain scientific recognition. Or did he get mixed up and rely upon the crowd, and hope for a victory against impossible odds while cultivating the fantasy of martyrdom ?

The establishment -- and Professors King and Goldsmith, the official sponsors, found themselves irresistibly playing the roles of the establishment -- was quite pleased to let the panel develop into an over-kill of V. It could not even conceal its hope when explaining the public presentation of the symposium. King, who was the Chairman of the panel, explained privately that he was so anxious over the responsibility of presenting V. at a scientific forum that he had to persist in saying that the purpose of the symposium was to refute a set of ideas that science had proven absurd. Actually he said so publicly beforehand:

What disturbs the scientists is the persistence of these views, in spite of all the efforts that scientists have spent on educating the public. It is in this context that the AAAS undertakes the Velikovsky symposium. Although the symposium necessarily includes a presentation of opposing views, we do not consider this to be the primary purpose of the symposium. None us in the scientific establishment believes that a debate about Velikovsky's views of the Star system would be remotely justified at a serious scientific meeting.

Now I would like to quote the economist Shane Mage's booklet, *Velikovsky and His Critics*, because of its elegant conciseness.

Besides, he was present at the occasion, and neither Deg nor I was there.

What took place in San Francisco was... the beginning of a real debate, even if it often seemed to those of us in attendance like a donnybrook. Of the six invited panelists, one, Norman Storer (Prof. of Sociology, Baruch College of CUNY) disavowed competence in any aspect of the subject but nevertheless managed to conclude that the mistreatment of Velikovsky, though abstractly deplorable, was also an "understandable" response of the "scientific community" to a perceived "attack by right-wing forces in American society. Velikovsky himself presented a short paper outlining the basis of, and some of the evidence for, his *Challenge to Conventional Views in Science*, and often took the floor vehemently to rebut specific criticisms. His views on the importance of electrical forces in celestial mechanics also received strong support from Professor Irving Michelson (Mechanics, Illinois Institute of Technology), who described his paper *Mechanics Bears Witness* as "an act of objective scholarship," intended to be neither pro or anti-Velikovsky.

The polemic against Velikovsky was conducted by two Professors of Astronomy (Carl Sagan, Cornell University, and J. Derral Mulholland, University of Texas) and one Professor of Mathematical Statistics (Peter Huber, Swiss Federal Institute of Technology). Almost all the media coverage of the panel consisted of favorable citations of these three contributions, especially Sagan's very long essay entitled *An Analysis of Worlds in Collision*. In the absence of Sagan, who left before all papers had been read in order to attend a taping of "the Johnny Carson Show," a vigorous discussion, involving audience as well as the remaining panelists, continued for almost two hours after conclusion of the formal presentations. Both sides claimed victory.

The logical next step was publication of the symposium proceedings, but of the panelists only Velikovsky was willing to permit publication of an integral transcript of the speeches and the floor discussion. Lengthy negotiations failed to arrive at a mutually agreeable format, and ultimately the two parties decided to publish separately.

The anti-Velikovsky case was presented by Cornell University Press under the title *Scientists Confront Velikovsky* (hereafter

referred to as S c.V). In addition to revised versions of the AAAS papers by Sagan, Mulholland, Huber, and Storer, this volume also includes a paper by Prof. David Morrison (Astronomy, University of Hawaii), prepared, in its original form, for a 1974 conference sponsored by the editors of *Pensée*. There is also an introduction by Dr. Donald Goldsmith, editor of S c.V and organizer of the AAAS panel, and a foreword by the novelist and authority on heresiology Isaac Asimov. From the proclaimed standpoint of "scientific orthodoxy" Asimov begins by raising the question "What does one do with a heretic?", with specific reference to Velikovsky; goes on, with unimpeachable orthodoxy, to write that Velikovsky's proposed physical explanation for catastrophic events recorded in the Bible is a "far less satisfactory hypothesis" than is "the hypothesis that divine intervention caused the miracles", and concludes that "Velikovskians" are totally impervious to any amount of "mere logic." (S c. V, pp. 8-15) He does not, however, recommend that they be turned over to the secular arm...

The AAAS volume is presented by its sponsors as "a full scale critique" (Goldsmith, S c. V, p. 27) which, according to the review commissioned by the AAAS Journal Science, accomplishes a definitive refutation of Velikovsky's "downright preposterous" heresy. The essays in this book "utterly lay waste his theories." Sagan's paper "is amusing, acrid, and totally devastating...his essay alone is sufficient to reduce the Velikovsky theory to anile fancy," and "Velikovsky is flatly and totally disproven... As far as Velikovskianism is concerned it is dead and buried. The final nail has been driven." (*Science*, v.199, Jan. 20, 1978, pp.288-9)

Was this appraisal accurate? Referring to the trial by press, yes. V. was further damaged in the eyes of scientists everywhere. Speaking of substance, whether of the symposium or of the papers, it was not true. The arguments of Sagan, Mulholland, and Morrison were mostly well-known and those of Huber (the surprise amateur of ancient Babylonian tablets) had been long ago considered by Stecchini and Rose. Additions and revisions allowed to the writers did little to bolster their defenses when it came time to publish the book *Scientists Confront Velikovsky*. An early analysis of the enemy dispositions appeared in *Pensée*; then, in two issues of *Kronos* (III2 and IV3), and in pieces appearing elsewhere, supporters of V., forced to waylay the establishment speakers in the

alleyways, stripped them of their arguments. The Cornell University Press, a willing captive of circumstances, which might have published a fascinating, meaty volume on the issues, published one poor lopsided volume, and sold paperback rights to W.W. Norton Company. The heretics remained in the alleyways. Scarcely any reviews (except those of the heretics) put the opposing volumes side by side and compared them judiciously, or even savagely.

I shall not go into the several dozen points of contention here, and will take Deg ' s word for it that the substance of the full arguments did more good than harm for a considerable range of quantavolutionary hypotheses, including some precisely attributable to V.

Shane Mage, in appraising the speeches against V., uncovered in them several important concessions that had been apparently achieved over the years. First, the book *Scientists Confront Velikovsky* "disavows and repudiated the entire 'Scientific polemic' of the 1950 ' s and 60 ' s both implicitly and explicitly." Next, both the sponsor, Goldsmith, and Mulholland assert that V. ' s ideas and arguments are not "un" nor "anti"-scientific, whatever the press and then the scientific community presumed to draw from the event. Furthermore, the legitimacy of cosmic catastrophic hypotheses in science was acknowledged both by Sagan and Mulholland, but the specific hypotheses of V. were attacked (and obviously the scientists are in confusion as to how they can work historically and empirically with the hypotheses that they admit.)

In line with my earlier suggestion, a different and more proper title would have brought these most important areas of agreement to the fore. If these would have been the subjects of the panel, and if Velikovsky had been only one out of eight panel members and authors, four of whom would have adopted positive positions and four adversary positions, then the world of science would have been much impressed and enlightened, and the heretics might have surrendered their weapons with honor. V. himself would have acquired many scientific allies and be better received from then on in discussions among scientists; hundreds of hours of anxious and resentful negotiations and dispute would have been avoided; and many fresh minds might have been inspired to enter the newly

opened field of quantavolution. The AAAS affair was a great opportunity lost to quantavolution by V. and the establishment agents.

Deg disliked the word "heretic." I mentioned so earlier. Perhaps I should have renamed this book. To him the word was un-American. It was one more useless nuisance for indulging V.'s self-image. True, the dictionaries include it with its modern meaning, "one who dissents from an accepted belief or doctrine of any kind," but in a modern democracy, he said, the occasions for heresy are innumerable, while, without severe sanctions, the hysterical historical pitch of the word is absent.

Whereas V. called himself a heretic both in respect to religion and to science, he chose to stress science as the offending authority. In his day, in Western Europe and America, the idea of heresy hardly held meaning for the larger society, although it could be effective in the ambiance of, say, Catholicism or Presbyterianism; even here one had to lay claim to authority heretically within the group itself.

V. was determined to be a heretic from within science but to do so one had to be a scientist in the first place, and one of the childish games played between the scientists and V. had to do with whether he was indeed a scientist and therefore properly within science's jurisdiction to be adjudged heretical. Logically, we are back with Alice in Wonderland and not the least of the skits from never-never land was the massive attack upon V. launched in the name of science and culminating in the book, *Scientists Confront Velikovsky*.

Here, from the beginning, the scientists promoting the event at the AAAS meeting in San Francisco, were befuddled. Yes, they felt, they had to defrock V., but to do so they had to frock him and admit him to their canonical court.

But to admit him they had to claim jurisdiction over him; that is, they had to legitimize him by allowing him to debate his ideas with them. One can perceive this strain and stress clearly from beginning to end of the touted confrontation over a period of years. The promoters, King *et al.*, would say, we are not meeting to discuss V.

but only to make it clear that he is not speaking as a scientist. And then, of course, they proceed by the only modern way science knows, to refute him as a scientist in public argument.

When the time came to publish *Scientists Confront Velikovsky* the establishment, operating by queer contradiction, obtained the good services of author, Isaac Asimov, the most famous popularizer of science and science fiction to introduce the work, admitting *ipso facto* that its contents alone would not fulfill the contract put out on V.

Then what does Asimov do but fall into the pit of scholasticism by spending his precious few pages as an instant expert on heresy. He accepts the fractured word and further mangles it. He concocts and improperly applies a distinction between two kinds of heretics, those who commit heresies from inside the system and those who do so from the outside. The first type can be sometimes correct, the second never. V. was the never-correct type. Says Asimov, "Public support or no, the exoheretic virtually never proves to be right. (How can he be right when he, quite literally, doesn't know what he is talking about?)"

Lest he be pilloried for such bold statements, Asimov has insured himself by the most vulgar kind of verbal trickery: he makes insiders out of outsiders if they have "reached the peak of professional excellence" whatever that is. So naturally -- once again he says it -- "the exoheretic... is virtually never right, and the history of science contains no great advance, to my knowledge, initiated by an exoheretic." There is no arguing with such foolishness. The foolishness, I must add, is compounded by self-contradiction, for is not Asimov's gun hired to introduce this book because he has a large public that buys books? So here is Asimov, the outsider, depending upon the public which, he says, is always wrong, to follow him in his denunciation of heresy.

But matters become worse for Isaac Asimov. He says that the scientific establishment (calling it the "scientific orthodoxy") is "completely helpless if the heretic is not a professional scientist -- if he does not depend on grants or appointments, and if he places his views before the world through some medium other than the

learned journal." That is, the establishment can withhold grants, appointments, and publication from its own heretical members, but cannot from "exoheretics" or outsiders. That leaves the public as the only outlet for the exoheretic's views, but Asimov says that the public is never right: "the appeal to the public is, of course, valueless from the scientific standpoint." He does not seem to realize that he is condemning himself and science, for he seems to approve this situation while granting that in rare instances an inside heretic is incorrectly punished. I cannot easily believe that the two publishers (Cornell University and W.W. Norton) and the several authors, especially not the clever Carl Sagan -- but how can one watch out for everyone's business? -- did not read carefully the few passages that prefaced their great act.

In the years of which we speak, Deg had a part to play in the establishment and it was not a bad life. He turned up in Washington from time to time. He lunched with his friend "Kirk" Kirkpatrick, Executive Director of the Political Science Association, where he was for a time a Council Member, or at the Senate or the Cosmos Club with friends; Bill Baroody was funding some of his writings from the American Enterprise Institute for Public Policy Research. Earl Voss and Tom Johnson there were pleasant companions; it was a smallish show, then, close to the Republican Presidents and Conservative after his direct relations with it ceased. Deg knew a number of Congressmen. He had access to the U.S. Office of Education when Frank Keppel of the Harvard Graduate School of Education had gone to run it, for he had worked with Keppel at the Harvard Graduate School of Education and had been offered appointment there. He consulted with the Department of Defense when "winning the hearts and minds" of Vietnamese was top priority, and went to Vietnam on a panel requested by General Westmoreland, then Commander-in-chief. He had acquaintances who were in the top echelons of half a dozen great companies, and half a dozen of the large foundations, others who were millionaires, UN ambassadors and bureaucrats, New York politicians, and so on. He helped leaders like Nelson Rockefeller on occasion (without compensation). He went as a delegate to UNESCO. He helped the Publisher of *Life* magazine to help the American Jewish Committee

to establish better relations with the Vatican, and was shoved by a wily Spanish Priest for a moment into the ample arms of dear old wobbly-eared reformer, Pope John XXIII.

The New York University President, James Hester, also from Princeton, was as friendly as he could be to a faculty troublemaker. The departmental faculty itself was to Deg 's ways of thinking too petty, unintellectual and anarchic to launch upon large schemes, and moreover his giant University was always in a state of imminent financial collapse. After his first year there, he had to bring in practically all of the funding for his projects from foundations and gifts, which is no so difficult when one is in the swim of things. His middle-level university income from his tenured appointment was supplemented by consulting fees, honoraria, and grants. He spent all the money that he could spare on his *American Behavioral Scientist*, which was felt to have a good influence on social science research, and gave him editorial influence, whether critical, or to help friends, or to assist students and up-and-coming scholars to get ahead.

Publishers were easy to come by. Advances were generous for textbooks, subsidies for the others. Complimentary books flooded his library. He could stop at practically any university in the world and be invited to lecture, dine, discuss. He traveled abroad often, always with jobs to do, always funded at least in part by some agency (never The Agency) or foundation.

To hear him tell the story, he could have gone on and on this way with *la dolce vita*, spreading his wings of influence over more and more people, things and activities. He could have dawdled more with attractive women, driven a new car, worn new suits, written books with ex-Presidents, etc. Why this was actually his way, his route, his fate, could have been foretold in childhood. I doubt that he fully realized it. But perhaps enough of the reasons become evident in the pages of this book to preserve us from going back to the "Roaring Twenties" of Chicago, Ill., U.S.A.

There seems little reason to doubt Deg, however, when he cites his friend Ithiel de Sola Pool 's analysis of networks. By a calculus of probability, given an unstructured society, the chances of any person knowing a person who knows another person who knows

any other particular singled-out person in the society are very high. Theoretically, given the relatively sharply structured society everywhere, he could be introduced to anyone, even in the worldwide society. Deg, in his old notes on Pool's manuscript, figures that he practically needs know only his own widely differentiated acquaintances to know anybody in the top elite, and needs but jump one more acquaintanceship to meet just about anybody else. He even made a parlor game out of his directory, and proceeding to say who whom he knew would know this person. This occurs because a person who knows 2000 people is in a position to know the, say, 500 acquaintances each, of these, and this million, with its 500 acquaintances each, exceeds that population by far, but since the population is stratified, the number falls short of total success until the chain is extended.

There are applications of network theory to the workings of science. Conventional science, we know, is not a juggernaut, a palpable monster, a solid phalanx, a disciplined corps of bureaucrats, a theocracy, or even an organized political party. It is - - it must be, in order to avoid its own contradiction -- a subtle, diffuse, often impenetrable, often disguised, often unconsciously composed network of relationships.

Marxist scholars would readily comprehend this fact and would tie the whole network to the economic production mechanisms of the capitalist system. The Chicago School of political science would see in it promptly the manifestations of Mosca's "political formula and ruling class" and Deg's "ideological imperative."

Discriminated against indifferently in American Society, evangelical Christians such as many Baptists, represented in a growing movement of "Creation Science," but usually acting individually for their nooks and crannies in the system, would also be characteristically alert to the operation of the scientific reception system. So would the large number of individual American and British heretics who compose a disinherited, not formally qualified, keen and occupationally and characterologically diverse "watch and ward" network, ready to suspect the worst of the establishment. Resembling these latter would be many a disenchanting student, not yet amalgamated into the conventional system.

All of these together, plus the simply curious, might readily muster the kind of crowd that assembled to witness the Velikovsky panel convoked by the program committee of the American Association for the Advancement of Science at San Francisco. The audience, well over one thousand persons, was by far the largest of the Convention.

Let me now explain how it happens that the scientific network, or establishment, might in this case, as it has often done in the history of science, be acting against its own presumed interests and hence to repress new correct theories. How does the ruling formula of science triumph over challenging ideas, making them heretical, and chastising their proponents?

Every field of knowledge is nowadays organized. It has therefore leaders. Some of these leaders are parochial. Others have connections with relevant social networks and organizations of the other fields and other segments of society. These leaders acquire fame (which already represents the same circular system of the generation past, advancing for instance a Menzel, who inherits from a Harlow Shapley, or a de Grazia, who inherits from a Charles Merriam.)

The mass media, though it hardly reports science, seeks out or gives access to fame. Reporters, woefully unprepared, interview the leaders. Educational media, including widespread fund-seeking alumni magazines, turn to their exemplaries of the famous. The occasional television, radio, and magazine concerns about the knowledge industry result in reports that are favorable to the same group. Foundations appoint from the same leaders to their boards of trustees and consulting committees. So do scientific and political government agencies, although other interests can intrude more here. The leaders, and now we are speaking of some five thousand persons, give awards disproportionately to each other, as do generals and admirals. Government foundations, such as the National Science Foundation, are even more susceptible to network influence than private foundations.

In the area of book publishing, the ideas of the leaders largely

determine what manuscripts shall be published as textbooks, and on what kinds of books the university presses should spend their small resources. Trade book publishers for the general public have almost no viable interest in serious scientific or humanistic work. Usually what they publish in these areas is meant to blossom quickly and die, to challenge no strong interest, and certainly not to offer alternatives to major scientific paradigms unless they would join the ranks of somewhat disreputable and financially insecure publishers. Thus, if Velikovsky had published with Lyle Stuart's firm instead of the Macmillan company originally, the opposition would never have gathered. They had to have as their target a press that would seek to avoid censure for "conduct unbecoming a gentleman."

The scientific and professional magazines that report new knowledge are governed by boards and editors, who are acceptable to the leaders and are watched rather carefully by them. Fading away from the specialized periodicals are magazines of popular science, few of which are financially secure and all of which are dependent upon the good will of the leaders. The *Scientific American*, for example, would never wittingly go beyond the activities of the core elements of a science. When a troublesome or controversial theory surfaces on its pages, evidencing a conflict between two leader-led theories, it seeks to appease both sides by a second article or letters of comment. Its need to seem "original" is fed by lavish illustrations, a feature it shares with the *National Geographic Magazine*, the *Smithsonian*, *Discovery* and other periodicals. By editorial tricks, all such magazines lend their materials a glamour and adventurism that they usually do not in reality possess.

The network of leaders extends down through the public secondary and elementary schools from the colleges by way of lesser sheikhs, supervising boards, and *hoi polloi* of the fields. Not even the threat of teaching "creation science" in some state will excite overly the nabobs. The legal and journalistic techniques of handling anti-Darwinism have long been known, and a legion of educators moves efficiently into battle on this front with little direct participation of the national leadership. Private secular schools -- the Lawrenceville Academies and Grotons -- would never wish their pupils to utter the wrong titles or theories in anticipation of entering the halls of

learning hallowed by the leadership. The Catholic schools are deintellectualized; nor has the Catholic Church yet retracted its judgment against Galileo.

A word, finally, about the corporate world, where so much applied and some pure research is done, from which, too, funds must flow increasingly into the coffers of the universities. Their corporate images, hence their profits, depend upon the skills people come to believe (*via* advertising and public relations) that they command and engross. Like university presidents, leaders of science dip into corporate treasuries on occasion as consultants, board members, and officers. Just as retired generals are common in the aerospace and engineering industries, highly placed scientists, even without the need to retire, are frequently positioned in corporate research structures.

Immersed in this and in all that has gone before, a leader of the establishment network has almost no incentive to take up a new controversial theory, much less to originate one himself. He is himself subject to disciplinary actions, often quite subtle, should he stray from the fold.

The network can be most simply presented as a list of institutions through which the leaders of science operate or upon which they exert influence. The influence is continuous, is intensified on crucial issues and, in my opinion, is generally beneficial and should be enhanced throughout the system. Meanwhile, however, the influence needs consciousness-raising and built-in mechanisms of reform.

LEADERS OF SCIENCE

extend their influence into:

1. *Audio-Visual Media*
(fame; reportage)
 - a. TV and radio Networks
 - b. Public Broadcasting
 - c. Documentary films
2. *Popular Press*
 - a. Scientoid Magazines

- b. Science Fiction
 - c. Publicity (columnists)
 - d. Newspaper and newsmagazines, prizes, etc.
3. *Book Publishing*
- a. Trade
 - b. Textbooks
 - c. University Press
4. *Scientific Journals*
5. *Universities*
- a. Secular Schools
 - b. Religious Schools
6. *Scientific Associations*
7. *Foundations (private)*
8. *Governments*
- a. Executive offices, commissions
 - b. Legislatures
 - c. Government Foundations, Prizes, etc.
9. *Corporations*
- a. Research and consultation
 - b. Board of Directors

The leaders of science in the English-speaking world can be numbered from 50 to 10,000, depending upon where you wish to draw the line of influence. They are fairly concentrated geographically in the Northeast Megalopolis, Chicago, Washington, and the San Francisco Bay Area, with a small English contingent, fairly closely in touch.

An extraordinary fact is that immense scattered network ultimately engaging the whole world is composed of what in business or government would be regarded as absurdly small units. They are like the oldtime Piggly-Wiggly small grocery store, owner-operated network, not fully centralized, bureaucratic establishment. Furthermore, it is largely subconscious or scarcely perceived.

Nevertheless, in the end -- and merely to picture the network -- the librarian in Juneau, Alaska, the student at the University of Tampa (Florida), the editors of the *Times Literary Supplement*, CBS, PBS, NOF, the Ford Foundation, Harvard University Press, the Board of Education of the City of Chicago, the engineers of Western Electric, the science section of the *New York Times*, the editors of *Science Magazine* and its popular offshoot *Science 84*, the National Academy of Sciences, the curators of the Museum of Natural History in New York, and many thousands of other "nerve endings" of the science system of communications and influence respond to cues and jiggles of power from the elite group.

Surely, it is one of the most benign elites of the world. It probably rules easier and can rule less than almost all other elites. Its punishments are relatively light. It stupefies people but all forms of rule stupefy their clients or subjects; here, indeed, the science elite is more enlightening, in its double function of stupefying and enlightening, in its S/E ratio, than most elite or influence networks. But it exists, and it is effective. To evade or avoid or attack the Scientific Establishment, to invade its inner sanctum and transform its Holy of Holes, its ideological center, its paradigms, *Weltanschauung*, ruling formulas, or whatever one might wish to call its heart, is the work of decades and, at least before, of centuries, and, in the words of Lasswell, almost always involves the process of "partial incorporation," by which is meant that before the revolution is won, the elite changes its behavior to concede the victory and keep out the revolutionary personnel.

Thus the monarchical regimes of Europe incorporated in most cases the key ideas of the French Revolution before the republican revolutionaries conquered them, and the capitalist regimes went "welfare state" before the socialists could take power; so that, if the quantavolutionary movement were to seriously threaten the ruling elite of Newtonian stabilitarian and Darwinian gradualist uniformitarians, these would be reacting, as in fact they are acting now, to incorporate the quantavolutionary formulas and outlook.

Meanwhile the quantavolutionary movement would be formed out of mistakes of the existing regime, out of apostates and disaffected scientists and engineers, occult publishers, little presses, small

personal foundations, religious creations, maverick legislators, fugitive publications sliding out of Xerox machines, and a motley public crowd of dissenting readers and talkers. Sooner or later, according to Roberto Michel's "Iron Law of Oligarchy," the Scientific Establishment would be modified in attitude, beliefs, practices and personnel but would still be the oligarchy, or, let us say, "a better and more enlightened class of leaders."

CHAPTER FOURTEEN

THE FOIBLES OF HERETICS

For his first half-dozen years on Naxos, Deg stayed in a town apartment the Venetians had built in the 13th Century; then he moved out to his stone house on the isolated promontory of Stylida. In these places, much of the Quantavolution series was written. Deg 's permanent encampment at Stylida was of marbled stone and primitively equipped, not a cabin, neither a villa. Antiques jostled useful junk on the marble tables and shelves. He pounded nails into the walls and from them everything dangled. Empty plastic bags were stuffed behind shelves for further use, empty bottles were hoarded. String, cord and rope in odd lengths were saved and hung up. From this frugal perch sloping upwards, he contemplated the serene seascape before him and the battling cats of the world beyond, not excepting the heretics.

Saving rope reminded him of Frank Knight, exemplar of the *laissez-faire* Chicago School of Economics who, in his office at the University of Chicago used to store the string he too saved. According to an eyewitness, he was mounting a train for the East one day when he called out to his waving family, pointing, "There, get that piece of string!" His highly regarded economics, thought Deg, were nicely encompassable by *Homo Schizo* theory.

Knight 's colleague, the very liberal U.S. Senator Paul Douglas was dining in Manhattan, another time with Robert Merriam, Assistant to President Eisenhower, and with Deg, and Douglas told of a Republican Senator who had ridiculed the incessant internecine fighting among the Democrats; "like a bunch of alley-cats" they were. Whereupon Paul had risen to add, "That may be true, but what in the end is the result --many more cats!" And while they were laughing, the waiter handed the distinguished-looking elderly gentleman the bill and they had to laugh more as the Scot, Quaker, economist, and statesman, and foe of loose spending, winced,

grumbled, and paid.

The cosmic heretics, bereft of resources, collected pieces of string to build bold systems. Coming out of nowhere, and without structure or discipline, they fought like alley-cats. Rebuffed by the world of the press and science, they often became morose.

Deg's Journal, January 25, 1970

I spoke to Immanuel on the telephone. He is feeling poorly and he intimates both a throat ailment and sinister external moves as the source. We are all suffering vague symptoms in the world. For months, I have felt this and the pain and scarcely know to what to attribute them? There are thirty physical and psychological causes all intermingled and the physical uneasiness is appropriately vague. So many millions in the world are, I think, similarly affected. It is as if the germs of diseases were directed by a mastermind, who says to them, "Now man has learned to be specific and special in his therapies, so you must now be as vague as possible, so that he will not know what he is suffering from."

Deg might as well have gone on to talk of the generalized "germ" of schizotypus, which suffuses human nature and finds a great many ways of emerging in disease, now specific, now general. It may be no coincidence that in this decade two reciprocal kinds of slogan clashed with each other in the mind of society, the one aimed at pandemic expressing of paranoia, the other at fighting off paranoia, so that everyone was "unavailable" and "by appointment only," and "fill out the form" while people were telling one another "reach out and touch someone." Highly special acts of terrorism increased around the world as highly general public opinion surveys showed the public to be regarding every group of leaders and every special group as untrustworthy, including their own national and world leaders.

"The most despicable of all ways of suppression is denying to me the originality and correctness of my predictions." So said Velikovsky at a philosophical panel at Notre Dame on November 2, 1974. He was directing himself at the moment to Professor Michael Friedlander. Friedlander had announced, "One of the things I'm not going to do is to attempt to defend the foolish, and

intemperate, and venomous statements that have been made by scientists over the last 25 years." He proceeded then to incite Velikovsky's outburst (which one might also call "foolish, intemperate, and venomous") by addressing himself to V.'s astronomical scenario of the Venus encounter with Earth.

To be useful a prediction must be derivable logically and unambiguously from the model. If the prediction bears only a tenuous relation to the model, then the validation of that prediction may in fact say nothing about the model.

In rebuttal, V. pointed to the details of his own early claims: that Venus was incandescent in historical times; that the planet had to be very hot to carry the gaseous hydrocarbon clouds that he believed to be there; and that he had declared the first announced temperatures of 600 degrees to have been too low, and in fact they were.

What constitutes a prediction gives grounds for incessant quarreling and namecalling. Deg was convinced that scores of his own prognostications in sociology, economics, and politics could be culled from his own books and shown to have been realized. For instance, he had predicted at one time that the achievement of equal population districts ("one man -- one vote"), so stoutly advocated by the cities of America, would result in heavier political weight for the cities' chief frustration, their own suburbs. He was not surprised nor did he put in a claim when the prediction was fulfilled. He never got around to predicating when the world would end, but, should it end, he could in the thereafter cite some highly probable estimates.

I did not know when Velikovsky got onto the claims and predictions "kick." I am guessing that the famous letter by Bargmann and Motz got him going. It was the first nice thing ever said about him in a scientific journal. The letter was V.'s idea and he provided much of the contents. It asserted that V. had suggested radio noises were emanating from Jupiter and were discoverable; they were discovered serendipitously by Burke and Franklin over a year later. Further, in 1950, V. said that the surface of Venus must be very hot, and, sure enough, by 1961 the heat had been discovered by reliable instruments.

Practically nothing was said of the method employed to arrive at these advance claims. But so guilty are scientists in the matter of "claims" and "priorities" that V. profited greatly from his cryptic and general utterances. And, no doubt, had he been guiding NASA research, these items would have been systematically uncovered.

The practice of advancing priorities is childish and the idea of proving a general cosmogony by a race of claims is ludicrous. There can be no crucial test or event. Even if Venus were to slip its moorings and drift toward Earth tomorrow, the historical scenario would not be proven. If the cosmogony is accepted for working purposes, the prediction (or test) will have meaning; if the cosmogony is not accepted, the prediction cannot be stated. This is shown by the resilient way in which the great heat of Venus has been claimed as a greenhouse effect by Sagan and others.

A member of the audience at the Notre-Dame panel made the most fitting remarks:

Each side has constructed its own version of what would count as a crucial test, and has constructed its own judgment as to how that test has been passed or failed. This is a singularly sterile manner for resolving disputes....As far as rational dispute is concerned, we have to begin by saying we *might* be wrong....to say what would count against us in our own book.

It would certainly be appropriate, within every scientific work and in a discussion of it, to confess its weakness, to argue its null-hypotheses. We are bound to do a poor job of attacking ourselves. And, of course, disputation may overburden issues to the harm of clear presentation of the theses. Nevertheless, Deg, in writing *Chaos and Creation*, was anxious enough about excessive positive argumentation to give over a chapter to the Devil 's Advocate.

In one sense, the cosmic heretics in the Velikovsky case were a conservative group, asking for law and order in science, demanding even that the letter of the law be followed, all the more because their substantive ideas -- erratic planets, forceful electricity in space, short geological time, etc.-- were deemed untrue. In fact, like the typical heretical group in politics or religion, they had logically to deny that the word "heretic" could apply to themselves; for theirs

was the truth. To those who like myself believe that science enjoys only hypothetical and useful "truths," a scientific heresy is logically impossible. Heresy is an excrescence of authorities.

Heretics typically are intolerant of other heretics, if only to hold together their highly vulnerable and unruly group within a miasma of ideas. We find a push-pull phenomenon occurring: the heretics are pushed out of conventional science and attract or pull in the religious, the occult, of ESP, "Ancient Astronauts," UFO's and astrology, the eccentric, and the revolutionary types. All of this provides a hustle and bustle on the fringes of science. All scientists are normally neurotic about their fringes. Only the wisest (read "self-aware and self-knowing") and self-loving of them could understand and sympathize with what they saw going on.

Onetime, in the fall of 1976, far from the scene of action, Deg heard distant sounds of strife and the name called out of his old friend, Professor Paul Kurtz, a pragmatist philosopher and Editor of the *Humanist* magazine. Besides many pleasant hours working together, Deg remembered how Kurtz had let him introduce a scatological remark into an article of this well-mannered publication. He wrote Kurtz a tender of good offices, suggesting attention ought to be given to neo-catastrophism, and sending a privately printed essay on *Homo Sapiens Schizotypicalis*.

Kurtz replied (in confidence, for he was a careful keeper of the peace) explaining that the fracas had generated out of a single sentence against Velikovsky in an article by Sprague de Camp, a detested figure among Velikovsky's cult. Kurtz said that even if he had wished to do so, he could not censor de Camp. He was startled by the vehement and even menacing letters that he received arising first from publishing the De Camp article and then from a possibly garbled quotation of him in the *Washington Post*. At the same time, Kurtz acknowledges, "The followers of Velikovsky claim that he was unfairly treated by Shapley, etc. -- with which I fully agree, I remember full well your justifiable concern." He was, he said, open-minded, aware of general disbelief in V.'s theories, but not conversant with them, or with Deg's for that matter, and he wanted to know Deg's theory of evolution: "Your thesis is most creatively provocative. My major question is what does it do to the theory of

evolution?"

Deg told V. of Kurtz's letter, V. spoke to Greenberg, and Greenberg fired off a letter to Deg, wondering how he had come to be in touch with Kurtz, and retelling the story as he saw it: "Kurtz may be your friend, but we are certainly not enemies." Deg could only wonder once more at how Greenberg could turn any situation into a personal threat and from this into an aggression.

The Humanist did publish an article by V., defending himself strongly against the then current voices of his opponents. Possibly the pressure of anger unjustified impelled *The Humanist* to give V. his say; after all, isn't the lesson of democratic politics that a group needs anger, not justice, to make its point?

V. was lucky enough to have a few opponents who made a hobby of him. They kept an eye on the news about him and cast enough aspersions his way to maintain his more diligent supporters in fine fettle. In keeping with the history of ostracized movements, nearly all of the heretics worked part-time at the job. Most were poor, although they did not reveal their poverty like oldentimes Parisian bohemians. They were, too, mostly unreliable, partly because of their busy-ness and hand-to-mouth existence, and because they were not under the lash of the dollar, but also because they were often afflicted with intense inner struggles. I would quote Nietzsche regarding them, "It takes a chaos within oneself to give birth to a shooting star." "That's it, they're crazy," one might say, which is a fraudulent pretense of those who are crazy-normals.

Astronomy professor George O. Abell of U.C.L.A. writing in the *Skeptical Inquirer* says that the followers of V. "are actually following somebody who may be a bit crazy. For isn't there something psychotic about a person who claims that he alone in a field with which he is unfamiliar, can fathom the pure truth, while hundreds of thousands of specialists with lifetimes of experience behind them are muddling about in the darkness? And doesn't the popular acceptance of such a scientific-religious hero suggest a problem, or at least some kind of an unfilled need, on the part of the follower?"

Deg ' s Journal, Princeton, December 27, 1978

Warner Sizemore here yesterday, 10.45-1.30, discussing many affairs.

He reported that not only Greenberg and others were angry at the SIS magazine group in England but that Velikovsky was upset because of their caviling at points and their undermining his theories instead of developing them.

Further V. ordered Sizemore and Greenberg to drop Peter James as Senior Editor from the editorial board of *Kronos* in three months, or else he would give them no further material of his own to print. James is associate editor for the historical content of *SISR* and also on the *Kronos* board.

Then, says Sizemore, V. reconsidered and told them that he didn ' t mean what he said. Sizemore did not guess whether this was a conclusion of principle or of expedience. (There are several reasons for expedience: the scandal, the harm to Sizemore and Greenberg, as well as *Kronos*, etc.) In the later case V. would remain guilty of the very behavior of scientist upon which his own case of persecution is based in part. If his retraction of his order was in principle, then the action may be partially excused because it was withdrawn.

It is not the first time that V. has come perilously close to practicing the behavior of his enemies. He is by character domineering, and suppression of the opposition would come easily to him under other circumstances.

V. had been called a charlatan but there was nothing to it. Deg asked himself, how could anyone use the word? And that they used it as others use curses and obscenities. At most, on occasion and like most men, he believed suspiciously hard in ideas that were not so firm, but none, thought Deg, in this sense had never written a thoroughly honest book and none ever could, by the very limits of language, for language is fundamentally a compendium of psychic tricks, played upon oneself and others, fraudulent in a sense.

But now, I think, reflecting upon the heretics, that fraud is a remote cousin of pretension. To lay claim to something is a human necessity. Yet whoever has any claims must be a fraud. To say "I am alive!" is a pretense and a fraud, a boastful claim to what after

all is a delusion about nature, a question begged. We are all such frauds.

There is something else, too, another kind of subtle fraud, a fraud in the too delicate sense of being wronged, and this V. had. One who feels that he had been defrauded is a fraud, as, for instance, in criminology, many victims of fraud are engaged in attempted fraud to begin with, making money out of nothing, etc....And then, persuading others that one has been defrauded, is also a fraud. At such persuasive tactics, V. was a master.

He could persuade by overpowering belief and documentation that he had been defrauded on a grand scale. He could persuade the most pathetically defrauded people that he had been defrauded more than they, and the defrauded turned their purses of energy and sympathy over to him. For he had converted his defrauding into the collective conscience, and was collecting retribution and returns on his defrauding because his supporters neglected their own suits in order to pursue his suit but received no more than abstract justice.

It was as if all the gas company's customers thought they were cheated and put all their energies into the case of one them, making the case a landmark, but the favorable decision on behalf of the test case resulted only in the vindication and compensation of that person, while the rest could not afford to sue, and the gas company hardly changed its practices.

Now the time had come for Deg to print *Chaos and Creation*. It was 1980. An outsider, innocent of the sociology of heretical groups, would expect the publication of *Chaos and Creation* to be welcomed. The field would open up further. Fresh material would offer itself for discussion. The implications of the work of V. would be extended. New possibilities would be manifest. There might even be some personal congratulations in order, for no one had yet produced any considerable work in the format of a book that could be readily assimilated to most of what the readers of *Kronos* were versed in and attentive to.

Not at all. When the book was in page proofs, it induced the dormant strain in relations between the directors of *Kronos* and Deg to rupture into hostilities. The occasion for the hostilities came, as if often does in human relations, whether personal or international, out of a situation promising well. Executive Editor of *Kronos* Sizemore and Deg were meeting weekly out of friendship. They ate, drank, walked and talked together for hours on end. Sizemore was enthusiastic about Deg 's manuscript of *Moses*, and had also been reading *Chaos and Creation* as the proofs arrived from India.

At the time, Deg and Aim had largely abandoned Manhattan and were living in a tiny apartment in Princeton, writing their books, and spending as little money as possible in order to pay for the production of *Chaos and Creation* in Bombay. When Warner came to visit, they would huddle their sizable frames together amicably amidst piles of books and papers for a while, until Ami would retreat to the second room to write upon the kitchen table between the sink and the small bed.

The Indian production was nightmarish. A thick file of correspondence attests to the pains engendered by cultural and physical distance. A perfect book was out of question. The work was being set in hot type, linotype, which, unlike the word processors of today, lets new errors creep in as rapidly as old mistakes are expunged. For weeks a strike of Indian paper mills stopped supplies to the printer. The quality of the paper, never good, worried Deg, too. The poor Indians were trying to conserve their old machines and paper and ink and Deg could not tell from the proofs whether fonts were broken or the paper was refusing the bad ink, and, worse, whether the final printing impression would be uniform on the pages. The book was loaded with proper names of extreme diversity, with illustrations, and with hundreds of citations, three most common sources of typographical, printing, and formatting mistakes. Deg had known the same printers from a decade before; they had printed *Kalos: What is to be done with our World* and *Kalotics*; he had been to their shop; he liked the several owners and workers. But it was a different world, of different standards, and to convert it acceptably to American tastes, while keeping costs down and work within hailing distance of the

schedule, was continually frustrating.

Warner, believing Deg would be pleased (and no doubt he would have been pleased) to see some portion of the work printed, sent (without Deg's knowledge) a photocopy of the page proofs to Greenberg, then in Florida, and spoke to Greenberg about the progress of the work in the course of their frequent telephone conversations. Greenberg was enraged by errors still in the proofs, or so the issue was presented to Deg by Warner. Deg, already upset by the defects and by the report, asked Greenberg on the phone to be specific about the work being "full of errors." When the letter came, the little that was added to the mistakes transmitted by telephone was rushed off to India for correction. There were mistakes so slight as a compositor's misspelling of Greenberg's name in a footnote crediting him with contributions to quantavolution (his name being mistakenly misspelled by the compositor as "Queenberg," for instance, in itself sufficient cause for paranoiac fury), and a wrong middle initial for Earl R. Milton, who received 'Earl S.', a complimentary psychological mistake tying him to a dear old professor of Deg, Earl S. Johnson, the same to whom *The Divine Succession* is dedicated.

Writes Greenberg:

After going through half of the text of *Chaos and Creation*, the Citations, and Bibliography, I have decided to enclose a sampling of pages that is symptomatic of the entire work. The kind of repair help that you need goes far beyond any *gratis* assistance that I could provide. I have already spent the better part of three days reading your book and no relief appears in sight. Typos abound, names are misspelled, publications are improperly cited and dated, many dates are questionable and just plain wrong, not to mention glaring omissions from the published literature. The catastrophic sequence proposed by Velikovsky has been rearranged (*Mercuria precedes Jovia*) and work by people such as Warlow has been uncritically accepted, etc., etc.

He goes on to list various, mostly brief, articles, and certain contributors to *Kronos* that were not in Deg's bibliography (the longest and most complete that had ever appeared on catastrophism and Quantavolution), concluding "What you have done is

downright insulting and I find it hard to believe that it wasn't deliberate."

Deg replies on April 2 from Princeton:

You agreed to telephone me collect, later on, and to recite your list of such findings into my tape-recorder. You knew that the need for any corrections was immediate. I kept the machine by my telephone for six days more and now here is your letter. Several additional typographical errors are indicated, two of which I wish I might change, along with the aforesaid. Otherwise your letter pullulates with grotesque exaggeration, unsupported allegations, hostility, and vanity. Dealing with paranoia makes one paranoid: could it be that you first promised and then decided not to offer corrections of the proofs because you want to be free to slander the book?

Deg was surprised at the rapidity with which the situation deteriorated. Sizemore, father, organizer, producer, financier, executive editor and trouble-shooter for *Kronos* let Deg understand that a selection from the book would not be printed and that the book would not be reviewed. Deg scoffed at this: how could it not be reviewed? Whose magazine was it? It would be a mockery of the pretenses of *Kronos* magazine, both substantive and libertarian, to suppress its mention. Warner unhappily suggested that the book need not be reviewed in *Kronos*. Deg insisted that Warner do something about the matter, to no avail. Their warm friendship abruptly froze.

Many months later, the book arrives from India. A review copy was sent to Greenberg. Other copies were sold respondents from an announcement by way of the mails. One day in April of 1982, Deg received a letter from Stephen Franklin, whom he did not know. [I find that they exchanged letters many years before.]

Dear Dr. DeGrazia:

I wish to obtain a copy of your book *Chaos and Creation*. Please let me know whether I may obtain this directly from you, & if so how much, etc. If not, where? I am enclosing a copy of a letter I received from *Kronos* since I feel you may be interested in how they are handling requests for information

about your book...

Franklin was referring to a letter from Leroy Ellenberger, who had been promoted from a free-lance gadfly on V.'s opponents to Executive Secretary of *Kronos*. The letter was written on *Kronos* letterhead with a Glassboro State College address, and did not oblige Franklin's request for Deg's address. The letter follows:

Dear Mr. Franklin:

With respect to the book *Chaos and Creation* which is the subject of your March 25th inquiry, be advised that KRONOS has chosen, after examining it, not to be associated with its promotion or distribution. For your information, the book was published privately in India. Its author is in charge of its commercialization.

As a reader of KRONOS, you are no doubt aware that we are not averse to presenting a critical approach to Velikovsky and that we will entertain responsible alternative, and even opposing, views. Given our interest in developing a Velikovsky-based catastrophist alternative to uniformitarianism, we would be more than anxious to inform our readers of new, fruitful sources of information. The book in question leaves too much to be desired to merit, in our opinion, serious attention.

If your curiosity gets the better of you, so be it. CAVEAT EMPTOR.

Deg called Franklin, received authorization to use his name when raising the issue, and with malice afterthought, sent a letter to the President of the College, reproaching him for letting the College be a party to damaging slander through people who were pretending to be connected with the School. Official action and an apology were asked. Expectedly, there came no reply, but Sizemore was aggrieved by the step, calling it ridiculous and a charade.

Meanwhile, Deg chose out of the "staff" of *Kronos* several individuals whom he knew personally. He wrote to ask them their attitude in regard to not reviewing his work. All replied sympathetically; still not one found the issue serious enough to deliver an ultimatum to *Kronos*, not Frederick Juenemann, not

Cardona, not Lynn Rose.

Rose aroused Deg's ire for postulating an enmity between Greenberg and Deg which did not exist, and evaded the issue of Ellenberger. (Deg liked ornery characters like Greenberg more than suave types like Rose.) He wished to hurl at Rose a statement in *Kronos* made by V. against Storer of the AAAS panel: "One who maintains 'neutrality' between a gross offender and the victim of the offense does not give an objective account of the realities; the account is *biased* in favor of the offender."

Even Earl Milton who was so close a friend and collaborator did not take up a strong position. Irving Wolfe at University of Montreal replied that *Chaos and Creation* should be reviewed and said that he would tell Greenberg so. Greenberg held firm, something he was good at doing; some of the heat was turned against Ellenberger, as if his letter had been a willful rash act, and a decline in his fortunes began, partly accounting for his retirement to his original home base in St. Louis. But Deg regarded Ellenberger and even Sizemore as toys of Greenberg in this instance. Toys for what? For psychiatric play-therapy, he insisted.

Many months later, as three of the "Staff" and friends including Deg sprawled about a sunny dock and swam in the August waters of Lake Kashagawigamog near Halliburton, Ontario, they talked of the affair and all seemed to agree (no vote being taken) that Lew Greenberg was acting the dog in the manger, that he acted so habitually, that Ellenberger was irresponsible, that the book should be reviewed, that Deg should cool down his reactor, and that *Kronos* would collapse if Greenberg resigned, as he frequently threatened to do. And if *Kronos* collapsed, where would its 2000 readers go, and where would its score of writers go to publish their articles? Dwardu Cordona, a writer and editor of hard opinion but essentially sweet character, asserted he would bring up the matter with Greenberg again. Deg was noncommittal. Later on, he did receive a letter of Cardona from Vancouver mentioning, *inter alia*, that he talked to Greenberg, who was still without remorse, and even still angry.

The past could not be recaptured, despite the restoration of a distant

relationship, and the major issue remained (the refusal to review *Chaos and Creation*). Sizemore sent a note of condolences when Deg's mother died and then another note apologizing for addressing the first note to "Albert" instead of "Alfred." Deg had not noticed the mistake or, more properly, had noticed it and thought nothing of it. Now he apprehended that the printers' errors, which misspelled Greenberg's name in one place, etc., and the personal slips that made Earl R. into Earl S., and so on, might be compared with changing the name of Alfred to Albert, this involving a close friend of many years. Poor Sizemore, thought Deg, caught up in an object lesson; I should have thrown the fit of rage he expected.

Sizemore was at this time enormously busy. He had four major occupations, beginning with his professorship in philosophy and theology for one. Secondly, he was, as I said before, a creative artist who had put aside his larger skills to create a singular commodity, friezes in wood, copying in detail great (or lesser) paintings. And these he carried around to sell at fairs on certain weekends, and while sitting by his works he read books and articles and newspapers by the bag-load. Then he entered upon the national Amway corporation, and began to build a network of clients and customers to purchase a wide range of consumer goods; this entailed meeting upon meeting; much of the vast energy that had gone into advancing and promoting Velikovsky was moving into a truly American promotional enterprise -- part crass materialist, part ideological fervor, a hybrid of love-thy-neighbor and get-rich-quick. Deg would not join him; he regretted the diversion of the intelligent energies that had placed Sizemore among the top dozen of no more than a few score active promoters of quantavolution in the world.

Yet he understood the figure of the missionary-capitalist, for he was reminded of the time he studied the leading caucasian families of Hawaii, who had emerged from their work at Christian conversion owning a good part of the land, commerce, and industry of the Islands. He believed, unlike others, that Sizemore and his wife, who had never before plunged into an enterprise with him, might well make a fortune. Max Weber, Richard Tawney, Edward Shils, Sebastian de Grazia, Benjamin Nelson and their brethren of

economic sociology would instantly recognize the puritan-capitalist nexus in Amway and in Warner Sizemore.

Nor, meanwhile, excepting his break with Deg, did Sizemore neglect his primary responsibilities in quantavolution. He still was the mainstay of Greenberg (and I do believe that Sizemore, were he to strike it rich, would generously fund *Kronos* and set up seminars, publish books, and promote the general development of the field); he still visited and helped Elisheva; he kept up with the field. He aided friends in need, as he did Sigmund Kardas, first when Kardas moved his house, and then when Kardas was nearly killed crashing into a wrong-turning trailer truck one midnight on the highway near Bordentown.

In October, 1982, upon returning from Greece, Deg was still needing Sizemore:

Dear Warner:

I hope that all goes well with your enterprise;

I trust that you have known of *Kronos*' decision last winter to not review *Chaos and Creation*. After your long history of interest in the book and its writing, this must have come as a surprise to you. Have you spoken to the staff about it?

Before leaving for Greece last Spring I submitted a note to Jan Sammer as Associate Editor of *Kronos* to read and forward for publication. I commented upon Velikovsky's Baalbek article. Sammer has since reported to me that when he told Greenberg about it, Greenberg said that he would not read it or publish it. This appears to be one more step in the recapitulation of the unconscionable techniques which, we say, were employed in regard to Dr. V.

Also, out of the blue sky came the enclosed letter from Ellenberger. [Not carried here.] I cannot afford the hours of rebuttal and psychiatric analysis that it calls for. What should I do with it?

Are you, or are you not, Executive Editor, father confessor,

and angel of this mad show?

Sincerely yours,
Al

P.S. As you may know, we have been denied the privilege of renting *Kronos*' mailing list to announce the publication of *Chaos and Creation*. On the other hand, I have received in the mail on more than one occasion postcards advertising Leroy Ellenberger's Velikovsky T-shirts, beer mugs, etc., using *Kronos* addresses. I fail to appreciate the philosophical principle at work here; should you not consult with Lynn Rose and advise me on it?

The letter aroused Sizemore to stiffer opposition. He railed at Deg for trying to separate KRONOS from its Glassboro State College letterhead, and advanced two propositions. This first was that "factual errors" in *Chaos and Creation* (which apparently he had not discovered in his intensive and enthusiastic reading of the manuscript and page proofs over a period of months) made its mention in the pages of KRONOS impossible: "it would be difficult with such errors as would reflect upon our integrity." Second he rejected any analogy between the treatment which the reviewing media had meted out to Velikovsky and that which was rendered Deg by KRONOS, adding that V. had "not once in forty years of correspondence with his opponents" resorted to "invective or scorn." This is close to the literal truth, just as the fact that General Eisenhower never killed an enemy soldier.

Such ruptures of relations among heretics are common. In this instance the main material effect was to suppress attention to Deg's book for three years among a key audience for works on quantavolution, represented by *Kronos* magazine. By the end of 1983 Greenberg was intimating an interest in advertising and reviewing Deg's books. [Again he renigged.]

I have come near to demonstrating that grand principles of morals and science can equally well be extracted from the dross of existence or flare out of imperial trumpets. The phenomenon of "self-destruct" is ever threatening in new movements of all kinds. Yet another phenomenon here deserves mention before passing on to other matters. It has to do with energetics, or more simply

laziness. And I am fortunate for having spoken so much of Sizemore for he exemplifies the non-lazy, the antithesis of the phenomenon of limited energetics or laziness. The phenomenon has also to do with the motives of the persons in fringe movements, with what they want to get out of their belonging and in fact do get.

The cosmic heretics were fond of reciting the litany, Velikovsky in the lead, that if his new ideas were to be admitted to scientific discussion, the textbooks of most disciplines would have to be revised. Astronomers would have to correct their own lamentable errors, and also they would have to study electricity, geologists astronomy, anthropologists geology, historians mythology, and so on. At the same time, a number of cosmic heretics were solely Velikovsky buffs: they were incompetent and unfamiliar with other quantavolutionists. Some had never had, nor now wished to have, an education broader than that afforded by *Worlds in Collision*. They derived their political, moral, and intellectual sustenance from a couple of books and a man. They were housed in this comfortable concrete defensive pill-box from which they would sporadically fire and venture forth on forays and to scavenge.

To this type of person, the threat of *Chaos and Creation* was as real as a full-scale attack upon *Worlds in Collision*.

To read another thick book? And more to come? A hobby would have to become a chore. Horrid possibilities in religion, geochronology, and human development had to be confronted. Much reading was required. A "snap-course," with its slogans, became suddenly a curriculum.

The format and style of the new book was itself a threat; it read well, but was organized like a text-book. The several hundred readers of its first year found even a chapter in it devoted to negative criticism. The chapter, called "The Devil's Advocate," was written by Deg under his dropped middle name of Joseph and an English translation of "Grazia" into "Grace" for the cognomen. He felt that a full self-critique, carried as he went along, would have been useful but would have doubled the size of the book. So he did his best to demolish his work in a single chapter.

That he succeeded with some is evidenced by an editor of

Athenaeum Press who, in rejecting the manuscript, claimed to be persuaded by Professor Grace, and by a review in the newsletter of the Canadian Society for Interdisciplinary Studies, whose author wrote that much of what he had to say was well put by Joseph Grace. Deg did not like subterfuge and had foreseen that a reader who liked or disagreed with the chapter would soon enough catch on to the dodge. Still, Elisheva read it and was amazed by its being there and asked Deg who the writer was. That caused a laugh. And Leroy Ellenberger himself, even after hearing the explanation, was so suspicious and perplexed that he wrote to Deg to confirm that the writer was not a professor at Glassboro State College. Deg noted with interest that Leroy, who would not let the readers of *Kronos* hear of the book, was reading it, presumably having wrapped it in a plain cover after receiving the gift from Deg.

On January 17, 1982, Brian Moore is telling Deg about the difficulties the British Society is having with its publications and asking him to come and share a platform with Dr. Don Robins who is to speak on isotopic anomalies in radiochronometry. The Society would also like a talk on the past ten years since Deg published *The Velikovsky Affair*.

Incidentally, mention of the *Velikovsky Affair* above reminds me of my current fracas with Lewis Greenberg which you may like to include in your comprehensive survey of the history of Velikovsky (when you eventually come to write it). I had received permission from Dr. Hewsen to print in *SISR* his talk to the last Symposium at Princeton in which he criticized Velikovsky's use of his sources. Lewis, of course, would not print it in *Kronos* as it was too critical for his taste, but as we advertise ourselves as a forum for the Velikovsky "debate," we felt it could be a useful contribution from an informed Velikovskian. The result was hugely ironical; Greenberg has threatened us with legal action if we publish it as the words were actually spoken at his Symposium. To me it seems the ultimate sin for a Velikovskian to attempt to suppress views which he finds unpalatable, but when I put this point to Greenberg he avoids the question and suggests we terminate the correspondence! There the matter rests for the moment. Rather sad.

Deg notes to himself on the margin of Moore 's letter. "Shall I send letter to Lew on this with copies to *Kronos* board?"

He does not do so. Instead, he calls Professor Hewsen, and later replies to Moore:

I spoke to Hewsen about your fracas with Greenberg, also Sizemore. Neither H nor S is strongly interested in the matter; H confirms the offer to you but thinks G is serious about a suit; S would advise against such an action, which, to my mind, would be only taken up by a lawyer as nutty as G. H. never gave away any rights to publish. And, of course, the attitude of G is disgusting. I find G 's polices and behavior frequently irrational and arbitrary, and have not talked to him in some time. S is occupied with a new commercial venture now as well as teaching, so sees into little. Ellenberger and G do the whole bit. I think that G would do battle with all the 1500 *Kronos* subscribers and all authors and with Mrs. Velikovsky and Shulamith Velikovsky and anyone else who would come into sight, especially all females; he is the most handsome rhinoceros in these parts and generally exhausted from his struggles.

And Brian answers:

SIS still seems to be *persona (prope) non grata* with Mrs. Velikovsky. She would not allow us to put slips in the British edn. of M in A drawing attention to the Society. We are also excluded from the book itself though *Kronos* is listed. Warlow 's book of course lists both organizations (though this has not stopped *Kronos* from berating him in their latest issue. With colleagues like this, who needs the Sagasimov?.) Which reminds me -- I mentioned the Hewsen Affair in my last letter and this obviously prompted you to enquire a little into the matter. I 'm afraid this has fueled Leroy 's paranoia even more. When I last wrote to him I said I was not going to pursue the matter, but he now thinks that I "asked" you to "intervene" on our behalf and gave me a little homily on hypocrisy to boot! Still, don 't lose any sleep over this -- such misunderstandings are endemic in our relations with *Kronos*. Leroy and I continue to collaborate on other matters, so there is still a positive side to the relationship.

Greenberg and Ellenberger manage next to enrage Peter James, who has a sweet disposition but a sharp tongue. He resigns from *Kronos*' editorial board with a vengeance, and later in London tells Deg, yes, certainly, if you want to publish my letter of resignation, do so.

Dear Lewis and Leroy,

In view of the present shitty relations between KRONOS and *SISR* I can't see much good reason to provide *Kronos* with any further copy...

Permission on "Darwinian man" is withdrawn (or at least suspended).

The same applies to my *BAR* and Stiebing correspondence, and to the promised section on Carchemish from my Glasgow Conference paper. Whether this material has been set in type or not, permission is *firmly withheld*. I had also better tender my resignation from the KRONOS staff as well..

Frankly I don't see why Hewsen's paper has put the wind up you lot so much. On the other hand maybe I do. All Hewsen was saying is that we must not treat Velikovsky as a tin god, and that we would be doing far more service to the man's genius by admitting the weak parts of his work and sorting the wheat from the chaff. The KRONOS staff suppress his paper (yes, *suppress*), at the same time protesting that they are not Velikovsky cultists. Give me one GOOD REASON why Hewsen's comments should not have the publication that *he wanted* them to have, apart from the desire of the KRONOS staff to suppress a point of view that doesn't exactly square with their own.

I am, to say the least, *disgusted*. I thought the name of the game was free speech and fair discussion. The "Velikovsky movement" has been crowing for so long about the suppression of Velikovsky's ideas. It makes me sick to see people who pontificate against Velikovsky's enemies do the same to someone who is basically sympathetic to Velikovsky's ideas. Go to the back of the class and join the Shapleys and the Sagans. You should both hang your heads in shame.

There was nothing untoward or irregular about Brian's letter to Hewsen. It was not going behind Lewis' back, conniving or

in any way deserving the hysterical reaction we got. *Hewsen* wrote the bloody paper, a fact that seems to have been forgotten in this silly squabble, not Lewis Greenberg or Leroy Ellenberger. Brian quite rightly wrote to Hewsen about it, and *asked* him to clear things with LMG. There was no intention of "stealing" anything without KRONOS Permission. Hewsen was asked to request KRONOS Permission. Get that straight. Nothing criminal, nothing strange. The reaction? Sheer hysteria, and the usual childish threats of legal action. And why? You tell me why. Ask yourselves, have a good think about your real reasons for trying to suppress someone's thoughts...

I also find KRONOS' attitude to Peter Warlow rather weird. Why have you got it in for him? Answer: JEALOUSY, plain and simple. If he lived in the States and was one of your immediate clique you would be breaking your backs to help him find some answers to Slabinski, instead of running him down all the time as you do. Along comes the guy who for the first time produces a model and a mechanism for a Velikovskian event and publishes it in a well established physics journal, and you lot just try and jump on him. Rose, in his comments about Senmut's ceiling, doesn't even seem to be aware of Lowery and Reade's extensive studies, or Reade's later work on the Ramesside star-tables. What are you going to put in place of Warlow's model, which satisfies the mythological and geological evidence so well? Spin reversal? Crustal slipping? Go on then. Provide us with a model that will make Slabinski happy. You know damn well that Slabinski's calculations can't and don't take into account electromagnetic effects. These are, after all, part and parcel of the Velikovskian view of celestial mechanics. So why do you take such great delight in Slabinski's calculations when they ignore them? Answer: jealousy.

I have taken a lot of stick from KRONOS staff for the criticisms I made of *Ramses II and His Time* in my review. Letters from Greenberg, Rose, and others made an incredible fuss as if my criticisms had come out of the blue, and I was told repeatedly that I was knocking Velikovsky's view of this period without putting anything in its place. On the 19th February 1976 I wrote a 5 page letter to Velikovsky, summarising several years work, pointing out my major objections to his equation of the Hittites and the Chaldeans, and the 19th and 26th dynasties. In February 1977 Velikovsky

wrote back pretty well ignoring the points made, except to postulate an *ad hoc* invention of a second Neringlissar to get around problems in the Neo-Babylonian succession. In 1978 *Ramses II* appeared, and the major areas of problem which I had pointed out were almost completely ignored. The reader was left totally in the dark about key material that shows Velikovsky's scheme for this period to be impossible. So I felt perfectly justified in raising this problem for the benefit of *SISR* readers. It would have been intellectually dishonest not to have done so, particularly since I had raised the main points with Velikovsky two years before...

KRONOS no longer strikes me as a "magazine of interdisciplinary synthesis"; it is rapidly becoming a cross between a Velikovsky fan magazine and an anti-*SIS Review*...

I am very sorry that it has come to this. But when KRONOS is filled over and over again with one-sided *ad hominem* piffle about Gammon, MacKie and Warlow, three of the most valuable contributors to the Velikovsky debate, and when KRONOS still continues to treat Velikovsky's work *in toto* as the proverbial sacred cow, then things have gone too far. I am only interested in having honest assessments of Velikovsky's work, to find out what is right and what is wrong. I am not interested in a silly KRONOS vs. *SISR* struggle which seems to interest you far more than the academic issues involved...

Peter James

But this is only part of the letter which I suppose might be summed up in the words of St. Paul to the Phillipians (1:15): "Of course, some of them preach Christ because they are jealous and quarrelsome, but others preach him with all good will."

The explosive discourse among the heretics, we have seen, is often as vituperative as the salvos of heretics against the outside world. It is also more personal and intensely felt. There were times when Deg felt that Greenberg's tiny clique of *Kronos* was trying to make a sort of Trotsky out of him for advocating world revolution rather than "revolution in Russia" as Stalin would have it. He was consoled to know that the invectives and diatribes were the lot of

other heretics and conventional figures venturing into the line of fire. Nor was he without blame; so that he could not but remind himself of the saying, "He who lives by the sword dies by the sword." Or "he who lives by the pen is poisoned by the pen."

By contrast with the heretics, the conventional scientists were most gentle among themselves on the subject of the heretics. It was almost unprecedented when once Robert Jastrow mentioned in print a serious statistical misapprehension of Carl Sagan in an attack on Velikovsky; Sagan defended himself vociferously. I do not mean to say that the conventionals are more fair or decent; they are nicer and more polite, and must go to print under institutional barriers against vehement expression. The heretic cries havoc and unleashes the dogs of war, and is often too distraught to tell friend from foe.

If all of this seems trivial, that is because the word "trivial" for a dispute is defined by contrast with horrible and bloody conflict. Or, I think, it is all trivial, even when there is horror and bloodshed. Examine the horror and bloodshed of history. Is it not very often over the trivial -- a sentence of Marx, an oath to the King, a remark "against the people," a failure to salute the flag, the greasing of bullets with pork fat, these and a myriad of like trivia -- which manage to bathe mankind in bloodshed and keep people in terror much of the time.

One can never tell from a virulent heretical letter or a smooth conventional reasoned critique whether, were the author possessed of the power, he would not exercise violent sanctions. The men and women who run affairs -- in all spheres of life -- are very often like the infant whose rages, so ludicrous, would be regarded with the gravest concern and even panic if *abracadabra* suddenly the infant sprang up adult and armed.

But that is the point of keeping the peace at nearly any cost: if people are kept from destroying themselves and each other, sooner or later they will be happy that they failed in their wishes. They will recognize that their aims are foolish, trivial, misguided, and mistaken, or that they would have been themselves erased, or that their enemies had agreed in principle with them, or that they and

their enemies, alone or together, might find a better resolution of their mutual problem.

What has been shown here is that the establishment has violated most rules of logic and fair play in literary and scientific intercourse, but, further, I have shown that the heretics, in dealing with the outer world and among themselves, have also violated most rules of logic and fair play in their literary and scientific intercourse.

What then can be concluded as a matter of principle? Call down a plague upon both their houses? Go in search of honest men like Diogenes forever carrying a lantern to illuminate any rare finds? Favor the weak against the strong, the heretic against the conventional establishment? Continue to expose such illogical and unjust conduct wherever and whenever it appears? Psychoanalyze, especially in the sense of self-analysis, everybody including ourselves? Reform the scientific reception system by institutional inventions to bring about a rule a law, emplaced as part and parcel of the rules of scientific method?

The questions answer themselves. Each implies a herculean task. Yet each implies a remedy of value. The answer to each and all of these questions is a resounding "Yes!" All must be done, no matter that each in itself is, if not impossible, exceedingly difficult. In *Homo Schizo I* and *II*, Deg put forward a persuasive, if apparently pessimistic, analysis of human nature. *Homo Schizo* is incurable, imperfectible, by nature. He can only be modified, constrained, trained, and controlled within limits. But within these limits stand at the one extreme the most horrible conduct and at the other extreme the most charming, endearing, and harmless conduct. The main trouble in the latter case is human unreliability.

Meanwhile, work was beginning on *The Cosmic Heretics* and I wrote Carl Sagan in 1981 asking for a meeting in the line of reporting first-hand something of Sagan's ideas about Velikovsky and about himself. A reply came, dated 9 November, 1981:

9 November, 1981

Belated but very sincere thanks for your letter to Professor Sagan asking if he might meet with you at some point while he is in New York City to discuss Immanuel Velikovsky as part of the background for the book you plan to write about Velikovsky. Unfortunately, Dr. Sagan is now totally immersed in science, having just returned to Cornell after an absence of more than two years. To his regret, he will not be able to accept your invitation. If you have not yet read it, you might wish to have a look at the chapter on Velikovsky in Dr. Sagan's book, *Broca's Brain*, published in paperback by Ballantine in 1980.

With kind regards,

Cordially,
Shirley J. Arden
Executive Assistant to Carl Sagan

I had indeed known of the aforesaid chapter, which had already appeared in at least three different publications and which had been mauled and dissected to the point of uselessness, Brian Moore's *SISR* review being perhaps the most nicely done of the valid commentaries upon the book. Perhaps a rebirth would come with the baptism of being "totally immersed in science" that would impel him to drive his own *Cosmos* TV series off the airwaves. Or to withdraw his book, *The Dragons of Eden*, from circulation, of which N.J. Macintosh wrote in *Nature* (27 April 1978): "It is inaccurate, full of fanciful and unilluminating analogies, infuriatingly unsystematic, and skims hither and yon over the surface of the subject, unerringly concentrating on the superficial and misleading... profoundly unscientific."

Sagan was the latter-day Harlow Shapley for many a heretic, though Deg could never quite tell why. Sagan had denounced Velikovsky's suppression, criticized his work publicly, and at worst was slipshod and sophomoric. On Deg's last visit among the English heretics in 1983, and amid some chortling, Deg was told of one Michael "Mike" Saunders, a true-believing Englishman, who was representing interests in the never-never lands of the Gulf States sheikhdoms, and was ringing people up with "great"

schemes, one of which was to win over Sagan by setting up for him a professorial Chair for Interdisciplinary Studies at Cornell University, counting upon him to sing a new song of solar space. After Deg stopped laughing, he opined that such things had happened before (see, e.g. the Morton Prince case, that is described in the next chapter), but that star professors are much too clever and ornery nowadays. Like the time when a large donation to the Psychology Department for the purpose of pursuing telepathic research was accepted by Stanford University but diverted to other uses, perhaps to construct bigger and better mazes for running rats. Apropos, unlike rats, professor avoid any mazes built for them and devise their own crooked ways. And some are quite principled, need I say?

PART FIVE

CHAPTER FIFTEEN

THE KNOWLEDGE INDUSTRY

Deg detested the new Bobst Library building at New York University from the moment he entered it on 16 December 1972 at 16:00 hours for a reception to celebrate its opening. The old central library had been in the basements of the Main Building. It was rumored that one could draw a book from there, and he did so from time to time. But now they had obstructed the view of Washington Square from his apartment to put up a casbah-red structure that from the outside seemed transported from the Near East while inside there was a giant space towering to twelve tall stories up, a roofed atrium around which wound narrow bands of shelving areas, obviously inadequate save for a few years of collecting, and already requisitioned on its top floor for the administrative officers of the University. The sensation was vertiginous; the building floated with its books tucked around its waist; how could a scholar study with his ideas precarious on the edge of exposed space?

A dance band was playing and he promptly envisioned how the design would permit its use by a Las Vegas concessionaire to bail out the near bankrupt school: a pavilion for dancing on the marble main floor, baths and massage parlors below, a bar on the second floor, social rooms on the third, a bordello for men on the fourth, one for women on the fifth, one for homosexuals on the sixth, then levels of gambling and a sky restaurant. One of the most expensive pieces of land in Manhattan had been used to roof empty space. The spectacle was dazzling. He rarely used the library.

When he was there he would ask himself whether it was hyper-critical of him to have such feelings, part of his basic envy of a world that rushed along without his consent, getting things done nevertheless; or was he simply observant of facts and aesthetics that most people, those in power as well as their subjects, could not

see or think of. This happened often, that he would no sooner denounce something, privately or aloud, than he would reprimand himself for thinking that he could see truth and value and contradictions thereof that groups of intelligent people working in financial, architectural, legislative, and other task forces could not see.

He did not wish to believe only in himself; he would rather enjoy the warmth of consensus, the applause of the crowd, but it would rarely work out so. Everything he did, everything he got, it seemed to him, even under the conditions when he was boss, gave him not a whole loaf, nor even half a loaf, but a thin slice. (I am not speaking of material goods, but of the quality of the product.) The situation regarding money alone was bad enough; the incompetency of the rich society to obtain value with its money was much worse to suffer.

Throughout his career, Deg found that it was harder to get money, the better the cause. A wage for oneself was not difficult, a salary slightly more so, commercial money for an imaginative project easier the quicker the turnover and the realization of profit. The trouble with your ideas, Rodman Rockefeller said to him once while they were conspiring about the world, is that they do not involve things that people regularly consume in large quantities, like canned food and cement houses. Not that Rodman was spectacularly successful with his company. IBEC, which went progressively from more romantic to less romantic, from third world to first world projects. In those times, Deg wondered at how year after year Rod could go on administering -- ever so comfortably to be sure -- a business without breaking out more often into some of the more imaginative enterprises and social adventures that he obviously enjoyed visualizing. Deg blamed affable father Nelson for the suppression.

To continue on money: then longer-term money became harder, then money for a vulgar or fashionable charity, then money for important research or an extraordinary book. Money came hardest for a cause that one believed to be purely for the public good -- unless it was a commonly recognized public good like the Bobst Library or some other building for a respectable university to house respectable and vulgar objects, or unless it was a concealed fraction

of a public good (the thin slice of the loaf again), like a significant sociological question slipped into an advertising survey for dog food, or unless it was illegally obtained, wherefore some political radicals have robbed banks and others their families, and still others lived under miserable and dangerous conditions.

Deg made a dozen attempts in search of a teaching and study platform for catastrophe and quantavolution. Recall this was a period when all kinds of new courses were being pressed upon universities and colleges; standards were in general decline. Professors were wringing their hands and burying their files for safekeeping. Yet they consistently rejected the advances (never mind seeking the help) of quantavolutionists who had more respect for the traditional research materials of the culture -- in classics, linguistics, foreign languages, history of science, philosophy, etc. and whose attractiveness to students would have erected massive barriers against the anti-intellectual and book-condemning feelings rampant in student bodies everywhere.

A score of teaching heretics had managed to insert V.'s materials into their courses under various pretexts and in several cases could even carry his name in the title or subtitle of a course. The Dartmouth Experimental College at Hanover, N.H., invited V. one time for two days of meetings with a seminar; at least six faculty members of as many different disciplines met with the seminar before and after to discuss his books *Worlds in Collision* and *Earth in Upheaval*.

V. was generally unhappy about the educational system, although he was displeased, too, with the student rebellions when they occurred. A dramatic polemic against the system of higher education finally appeared posthumously in three pages of *Mankind in Amnesia* (182-5). At least this statement is available to save him from reproach for never having attacked on general grounds (as opposed to personalized ground) the foundations of authority or their institutions.

Before converting his own social invention course to a course on quantavolution, a one-time unauthorized change to which no official objection was made, Deg tried a frontal appeal. Here, in 1973, he

addresses an assistant dean for curriculum, after discussing the matter with Bayly Winder, Dean and friend. He is making as few waves as possible, by placing the course in the summer session (where "imaginative offerings" are encouraged). The proposal went to the Committee of Deans:

October 29, 1973

Memo to: Dr. Sylvia Konigsberg

From: Professor Alfred de Grazia

Subject: A proposal for a summer
*Institute on Primeval Catastrophe
and the Development of Human Nature*

A large and increasing public is interested in the theory that ancient astrophysical and geophysical disasters caused profound changes in the human environment and human nature. Much of the interest centers around the work of Immanuel Velikovsky and his school of thought. Wherever Velikovsky appears to speak, his supporters and critics assemble by the hundreds and even thousands. His sole talk at NYU drew hundreds of students and professors several years ago.

I have worked for a decade on problems raised by Dr. Velikovsky since the publication of my book, *"The Velikovsky Affair."* in 1963, and am presently going to press with another book on the disasters of the Homeric Age. A heavy flow of written materials and archaeological reports has begun and promises to be practically endless. There is a need for an academic center for presenting and discussing the problems they present to all fields. Excellent scholars are available to participate. I suggest that such an Institute might be held from July 1-20, 1974, at New York University. It would occupy three hours of class time on fifteen days, would allow students not-for-credit, undergraduate students for four credits, and graduate students for the same (4- credits). The required readings would amount to 1200 pages and graduate students would prepare a research paper. It is expected that from 80 to 200 students can register for the Institute.

Personnel for the course would include:

1. Prof. Alfred de Grazia, Supervising Professor, Full-time;
2. Adjunct Prof. Annette Tobia, Ph.D., Einstein University in microbiology and presently lecturer at NYU, full-time.
3. Prof. William Mullen, Ph. D., Princeton University classicist (one-third-time);
4. Prof. Livio Stecchini, Ph. D., JD, Patterson State College, historian of science (one-third-time);
5. Mr. Ralph Juergens, Engineer and astro-physicist, Associate Editor of *Pensée* magazine, (one-third-time);
6. Visiting Lecturers and Discussants (one day each): Professors I. Velikovsky; (general theory); Lynn Rose, SUNY, (philosophy); Frank Dachille, Pennsylvania State Univ., (geology); Edward Schorr, Fellow, American School of Classical studies (archaeology); and possibly an additional person or substitute;
7. Prof. Nina Mavridis, CUNY, Political Scientist, administrative coordinator, full-time.

There would be fifteen primary one-hour lectures and 30 one-hour discussion meetings which would break the lecture audience into small sections of 25 persons. Related lectures and discussions would meet on the same day.

The titles of the lectures follow:

*Primeval Catastrophes and
the Development of Human Nature*

- | | | |
|----|--------------------------------|-------------------------------|
| I. | Time, Nature, and Human Beings | |
| 1. | The Theory of Catastrophes | De Grazia |
| 2. | Origins of Human Nature | De Grazia |
| 3. | The Geological Record | D ' Achille
or Burgstahler |

- | | | |
|------|---|------------|
| 4. | Historiography of the Solar System | Stecchini |
| 5. | Correlations Of Geology and
Astrophysics | Juergens |
| 6. | The Synchronization of Prehistory | Mullen |
| II. | Case Studies in Disaster and Development | |
| 7. | Case I: Atlantis Stechini | |
| 8. | Case II: The Age of Pyramids | Stechini |
| 9. | Case III: Exodus | Velikovsky |
| 10. | Case IV: The Homeric Age | De Grazia |
| III. | Origins of Behavior and Institutions | |
| 11. | Theology and Government | De Grazia |
| 12. | Literature and the Arts | De Grazia |
| 13. | Sexuality and Aggression | Tobia |
| 14. | Technology | Stechini |
| IV. | Final Problems | |
| 15. | Is Human Nature Governable? | De Grazia |

Discussion leaders: Professors De Grazia, Tobia, Stecchini, Mullen, Juergens, D' Achille, Burgstahler, Mavridis. With 100 students, nine daily section meetings are required. If the number of students exceeds 100, we should add to the faculty.

Readings: In addition to several paperback books that will be required the staff will prepare a collection of readings difficult of access, and Xerox them. The basic readings will be *Worlds in Collision* by I. Velikovsky, the study of Homeric catastrophe and literature by A. de Grazia, and the collection of readings that will represent, among others, the rest of the collection of readings that will represent, among others, the rest of the faculty. A valuable and unique supplementary bibliography will also be provided, and, finally, a set of maps, drawings, and a

special lexicon.

Continuation of Project: We would like to begin work on the project as soon as it appears probable that we would have 80 students, and to continue research in connection with, and to prepare for, successive Institutes. Therefore, it is suggested that 50% of the gross receipts from student fees (less additional faculty costs) for students in excess of 100 in number be placed in a special project fund in the University for continuing study and development of materials in the subject-area.

27 November 1973

TO: Professor Alfred de Grazia
FROM: R.B. Winder

The Committee of Deans discussed on Thursday, 15 November the proposal for a summer institute on primeval catastrophes as outlined in your memorandum of 29 October addressed to Dean Konigsberg. The consensus was that although the proposal might very well produce a large enthusiastic audience of paying customers, it probably would not do so from degree candidates. The Committee felt SCE might be interested in sponsoring the program, and I suggest that you take it up with Dean Russell Smith forthwith.

I do appreciate the drive you are putting forth for funding of various sorts and am only sorry that we felt this one would not work in the context proposed.

Nothing could be worked out in the unprestigious "School for Continuing Education." My academic readers can practice a dry run on this proposal, or another like it as carried in *The Burning of Troy*: their own committees might well respond similarly. Practically all universities in America capture their students with "credit courses" and find "course anomalies" as distasteful as anomalies in science.

The New School for Social Research was not so impeded, although it, too, became divided into "non-credit" and "credit" areas. V. gave a successful series of lectures there in 1964. Clark Whelton also taught there a non-credit course on "the Velikovsky Question" in the Fall of 1979 and significantly some students kept in touch with him afterwards, interested in keeping informed and hoping to

form an association.

Milton to de Grazia February 15, 1980:

Our department is being reviewed, and me with it. Trainor is one of the referees, the other is hostile. Yesterday he said, Milton is not doing physics because *Kronos* is not include in Physics Abstracts nor Science Citation Index. That remark deserves immortality. Hang in there, Al, we 're winning.

Milton was a popular professor at Lethbridge University and was teaching and reading quantavolution in his general physics and astronomy classes. He was an intellectual force on the vast Canadian Prairie, in touch with the press and radio systems. He knew the vast skies there like a Polynesian navigator. His lifelong asthma kept him in a lifelong course in advanced nutrition, organic chemistry, and atmospheric science. Then he read into myth and legend, and there was no stopping him. In every picture he discovered fresh signs. Aside from his personal qualities, he could connect with the more than ordinary number of students there who had heard everything good about God and the Bible at home, but nothing at all, if not bad, about these subjects in "education." Even only to hear the Bible being used as a learning tool was exciting to them. One should recall, too, how low the estate of physics had fallen.

We find our Dean of science reporters, Walter Sullivan of the *New York Times*, admonishing us.

Physics is the most basic of the sciences, apart perhaps from mathematics. All phenomena, when probed to full depth, are controlled by its laws...Yet physics is in trouble Student enrollments in that science have plummeted...There is a public distrust of physicists that borders on revulsion and the physicists themselves are pursuing lines of research more and more remote from the problems of everyday life...

Sullivan's key lines were the juxtaposition of two anomalies -- public paranoia and physicists' schizoid remoteness of character, traits that do not marry well. The American Physical Society was discussing the low state of physics, and Sullivan wrote that generally the leaders thought that more money should be spent by

the government. The British physicist and astronomer, Fred Hoyle, wanted even greater accelerators. He also wanted scientists to participate in politics. "You see why the world of politics is such an indescribable mess. Think of the opening of the baseball season. Think of the ceremonial first pitch. Think of what the baseball season would be like if that sort of pitching went on right through the summer. Then you have it -- the present state of affairs." Presumably under Hoyle's new-age baseball, physicists would pitch and baseball would become nothing but home-runs as the batters perfect themselves to bang away at the invariable straight-ball coming right down the center. Or perhaps Hoyle was saying that physicists should join the pluralist republic, as the ethnic strain of physics, helping where they could. Deg was not sure this was "according to Hoyle," but he liked the idea.

Milton tied together the Eastern and Western Canadians, and the Canadian belt triangulated to the Princeton-Trenton-Philadelphia area where Sizemore, Deg, and Greenberg kept shop. In the *Kronos* network, besides Greenberg, Sizemore and Ellenberg, might be found Rose, Vaughan, Wolfe, Cardona, and Jueneman. Some say that there should be added Milton, Sherrard, Westcott, Hewsen, Ransom, Talbott and Sammer. It was a unifocal net, with Greenberg as the focus. Deg connected with London, Holland, Paris, Basel. Greenberg, losing Peter James in London, found Bernard Newgrosh as correspondent. Marvin Luckerman, a doctoral student at the University of California at Los Angeles, founded a biennial magazine, *Catastrophism and Ancient History*; relations with Greenberg were cool, and the British were not much impressed with his first issues, but praised the good try. Still he rounded up a thousand readers and began to improve his journal. The creationist groups stemming out of Los Angeles, Ann Arbor, and Seattle were quantavolutionary perforce, having been given only a few thousand years by the Bible to produce everything. Here and there were quantavolutionaries of orthodox connections -- Gould at Harvard in paleontology, Ager of geology in England, and so on for several countries. The password that could readily cut these out from others was their answer to the question, "Has a planet moved?"

A very small group it all was, absurdly so when compared with the

network of thousands of periodicals, scores of associations, and the mass media that served orthodox science. It makes one wonder whether the heretics were worth considering: certainly by the usual American standards of great-sized multiplex technology they were not.

Deg heard when young from his democratic teachers how smartly the vested interests turned to minister to public needs, and was continually surprised when old to see how reluctant they had become to give themselves away. As his friend Lasswell put it, when writing with Abe Kaplan *Power and Society*, no ruling class gives up its goods without being forced to do so. This goes *pari passu* for philanthropoids and publishers, two industries affected with a public interest. The philosopher, artist, composer, author, administrative innovator, and physical inventor, if he is to be creative, typically is driven to become a sneakthief, or revolutionary, or go mad, or all three. So says Deg, who worried only about becoming a revolutionary, because then he would have to spend his time among sneakthiefs and maddies as well.

"Of course the heretics would not get support, they did not apply for it. One must play the game by the rules. Apply and apply and apply again." Deg knew more about this than his heretical acquaintances by the time they had encountered one another. He had enjoyed the fleshpots and studied what motivated the foundations, publishers and universities. He could warn the heretics that they need hardly try -- and V. was of this opinion, too -- or, worse, in order to succeed, they must prepare themselves to spend much of their energies in trying, and he was insistent upon a point that few could appreciate, that only a peculiar type of masochistic personality could apply incessantly to the point of success without losing the vigor, freshness, profundity of his ideas and the vital energy needed to pursue them for their own sakes.

On a few occasions, the heretics would solicit funds from individuals in small amounts to disseminate a publication about Velikovsky, but efforts at larger funding failed. The Foundation for Studies of Modern Science initiated a series of approaches, of which

I have already spoken; still, I shall add one more instance.

Murray Rossant, Director of the Twentieth Century Fund, was reported by someone to be attracted to V.'s work. Because Deg and his brother, Sebastian, were already known and had been working with the Fund in very different fields, FOSMOS sent two fresh and handsome faces to meet with Rossant and his colleague Schwartz, Bruce Mainwaring and Coleman Morton, both enlightened businessmen. A friendly encounter ensued, the upshot of which was that, although the Fund had never gone into this area, the two officers were interested personally in seeking other sources of funding, and when all was said and done, nothing happened. Nothing, that is, except that the Fund itself gave money to Giorgio di Santillana and Hertha von Dechend for research that they were doing on ancient and primitive myth and legend which, it was believed beforehand, would show that mankind was clever and scientific long before it was credited with being so, but also that there was no need to invoke catastrophism to explain the nature of mankind's early preoccupations.

This was recounted to Deg and the others by Stechini, who was well acquainted with Santillana and von Dechend. The product of the research, *Hamlet's Mill*, was welcomed by the heretics, nevertheless, for its intimations of ancient quantavolutions, but, if the reader wishes to understand the rampant confusion of the book, he may simply apply the hypothesis: here are two great scatomatized experts trying to avoid mention of catastrophism.

Though they be liberal or conservative, foundations are unlikely to be creative. They think they are able to judge creativity, of course, and especially if large, "creativity" and the "independent sector" of society are often included in their slogans. Their size and their bureaucracy correlate well.

"But in any event," writes Deg, who had urged the Ford Foundation to apply this, his scheme, "they are unlikely to make lists of all the people who lay creative claim to their bounty, and dispense it equally among a random sample of them. No they put the applicants and petitioners through the hurdles that they learned in their first course in Business and Public Administration should be set up to employ typists and

junior managers. So it happens that if all the people who ever applied for a Guggenheim Fellowship had given the same quantity of intense energy to a story, a painting, a song, or a study as they gave to applying, American culture would be up a notch or two over all its length and breadth. The waste of creative energies going into the national foundations of the sciences, arts and humanities is truly enormous; they use up at least a tenth of the country's creativity, with their stick games between the insiders and the outsiders. I would close them down and give their hundreds of millions to the colleges of the country whatever their defects -- in proportion to their budgets."

The cosmic heretics might discern that they were outlaws without going to the trouble of applying for their identity cards. But they could not help themselves: after all, they were educated in a way, bathed regularly, were fluent in the language, and found their interests carried in the index of foundation provenances. So they were tempted from time to time to try for a grant or subsidy. To my knowledge, they invariably failed. (I am not speaking of the occasional hand-outs tendered by friends and other heretics but of the system of lending a hand as institutionalized by the private or government foundations.)

Deg had enjoyed many experiences with foundations, small and large. The large were too "responsible" and proper to be bold. The small were generally pets and hobby horses of their founders.

Exceptions occurred that were interested in large social issues. A small foundation, the Reim-Earthart group, was a pleasure to deal with. It had a tough board, and was administered by James Kennedy and Richard Ware, both of whom bet on the man, not the institution, and did not try to make useless work for themselves and others. (The Cornuelle brothers, Herb and Dick, were this way, too, when they were in the foundation business. So was Bill Baroody.) Deg did a variety of economic and political studies with their help over the years. They were not occupied with ancient history or natural history. Since they lent you aid, they must be "good," I say to Deg sarcastically. Very well, he says, shall I give you some bad ones that have helped me? Never mind, I said, I'm in enough trouble with you already.

Yet the very deprivations and constraints that help Deg in his quantavolutionary trap made him more determined and passionate. Again Deg is writing in his notebook, perhaps to warn himself, like a politician warns himself to refuse favors or an infantryman warns himself to keep his feet clean:

There is this in common among a gold miner, a terrorist, and a purveyor of new ideas; they often come to exist in a new moral dimension, called immorality and outrage. Lunacy, lying, cheating, contempt and inconsiderateness for others; misappropriation: the pandora 's box of the creator spills these out.

Deg never committed such follies -- almost never -- and blamed his frustration correctly or incorrectly upon his own character: he inspired himself but could rarely inspire enough of the all-important others. Society is run by networks and gangs, and you have to join a gang, stick with it, use it and let it use you, and if ultimately you fail or perish with the gang, well, that 's the end of the trail, it 's a life-term establishment. Most gangs and network fails. Therefore skill and luck in getting into and out of the appropriate gangs is often essential to success.

"We 're working on an *ABS* issue about what needs to be done with the science of economics," said Deg to his colleague, Professor Arnold Zurcher, who was also Director of the Alfred P. Sloan Foundation. The Foundation operated in this area and Deg wondered whether they would provided support for the project in the neighborhood of \$10,000. His colleague represented an approach to political science that Deg regarded as outmoded and intent upon replacing. He was a jolly fellow and they were friends, and he knew that Deg was carrying the weak finances of the *American Behavioral Scientist* on his back. Do up the proposal, he said, I think that you have a good chance and I ' ll support it.

Not long afterwards, Deg received an official letter from the Foundation rejecting the proposal. He was surprised -- the request was logical: it was for small money and enjoyed support. His colleague was apologetic. Al, he reported, the proposal passed from one vice-president to another, with Margolis ' article from the *Bulletin of Atomic Scientists* about the Velikovsky affair attached,

and a big "No" scribbled on the face of your proposal. (Later on Bill Baroody of the American Enterprise Institute came up with some money to support the issue, and economists were assembled and the issue published.)

April 22, 1964

Mr. Ralph E. Juergens
416 South Main Street
Hightstown, New jersey

Dear Mr. Juergens:

I continue to be amazed that sensible persons continue to give attention to the Velikovsky affair. I wonder if you have read the statement by Howard Margolis in the April 1964 edition of the *Bulletin of the Atomic Scientist*.

Very sincerely yours
Warren Weaver
Vice President
Alfred p. Sloan Foundation.

Warren Weaver was a career philanthropist, wrote a good general survey on probability and, like many another, was a nice man. New York University named its Computer Center after him. (For a photo of it, in context, see Deg 's *Politics for Better or for Worse*.)

May 4, 1964

Professor Moses Hadas
Columbia University
New York 27, New York

Dear Professor Hadas:

As long-time subscriber to Reporter magazine -- actually since it started -- I was very much interested in your excellent review in a recent issue of "Hebrew Myths: The Book of Genesis." by Robert Graves and Raphael Patai. I did draw a long, deep breath, however, when I read in the first paragraph that "in our own time Immanuel Velikovsky, who was maligned for making myth the basis for a cosmic hypothesis, appears to be approaching vindication."

As a scientist, until 1960 a professor of chemistry at Columbia and an admiring colleague of yours in Columbia College, I have always regretted the action of a few misguided souls who reacted 13 years ago to "*Worlds in Collision*" by attacking Velikovsky 's publisher -- I think it was Macmillan. The book, in my opinion, should have been classified as science fiction but, nevertheless, it was unrealistic, and humorless as well, to expect a publisher interested in profits, as they all have to be, to overlook an opportunity to make a few extra bucks. The reaction to "*Worlds in Collision*" and a subsequent book, the title of which I do not recall, was fairly violent but, as I remember, reviews by Harrison Brown of Caltech and a woman astronomer with a hyphenated name from Harvard pretty well disposed, so far as I was concerned, of Mr. Velikovsky and his theories of cosmology. But now along comes Mr. Howard Margolis to tell us in a recent issue of the Bulletin of the Atomic Scientists that "Velikovsky rides again."

Perhaps you have already seen Margolis article, but if you have not, I think you may find the attached copy of interest and perhaps amusing.

With kind regards.

Sincerely yours,
L.H. Farinholt
Vice President
Sloan Foundation

To all medical psychologists: what is the vagus nerve syndrome that make a man "draw a long, deep breath"? *Re* Harrison Brown and the "woman astronomer" with a hyphenated name from Harvard, see *The Velikovsky Affair*, Alfred de Grazia, Editor.

6 May 1964

Mr. L.H. Farinholt
Alfred P. Sloan Foundation
630 Fifth Avenue
Rockefeller Center
New York, NY 10020

Dear Mr. Farinholt,

Thank you for your kind letter and its enclosure. I can have no opinion about the validity of Velikovsky 's work; his ideas may be wholly misguided, but I know that he is not dishonest. What bothered me was the violence of the attack upon him: if his theories were absurd, would they not have been exposed as such in time without a campaign of vilification? One after another of the reviews misquoted him and then attacked the misquotation. So in the Margolis piece you send me I read "Pi-ha Hiroth which Velikovsky has altered into Pi-ha Khiroth, further enhancing his evidence." But the two are equally acceptable transliterations of the Hebrew, and the latter is the more scientific. For the Egyptian name, Margolis, following old books, writes, Pekharti, but the Egyptian has no vowels, so that the correct form is P-kh-r-t, and of this Ph-khirot is very plausible expansion. The ha in the Hebrew is merely the definite article. It is his critic, not Velikovsky, who is uniformed and rash -- and so elsewhere also. The issue is one of ordinary fair play.

Yours sincerely,
Moses Hadas

May 31, 1966

Dr. Warren Weaver
Alfred P. Sloan Foundation
630 Fifth Avenue
New York, NY 10020

Dear Mr. Weaver:

I have harbored for many months your critical note concerning the studies of the *American Behavioral Scientist* on the reactions of scientists to Immanuel Velikovsky, thinking all the while of an appropriate constructive response.

We have recently published an enlarged version of the same studies in book form and I have asked the publishers to send you a copy with my compliments.

There are, of course, two issues in the Velikovsky affair -- one, the conduct of scientist and the press; two, validity and utility of his theories. The issues are separable but an involvement in one naturally inclines one into a stance on the other. I think that you can help many people, including myself, find their way through these issues, granted that you may have

neither the time nor the inclination to take on major responsibilities for the problems raised.

What I should like to suggest is that we get together for a day's conversation on the two issues in the company of several other men, with the sole end of educating each other. I have in mind persons such as Professor Donald Fleming of the Department of History and Science at Harvard University, Thomas Kuhn, Professor of History and Science at Princeton University, and Professor Harold D. Lasswell at the School of Law at Yale University. I believe that five would be the right number.

I have mentioned a reunion to none of the men named, and have an idea only of Lasswell's thinking about the subject at hand.

We might spend the morning on the question of validity (not "solving" it, but working to understand it) and the afternoon on the question of treatment of unorthodox ideas in science.

I am quite at your disposition on the matter. Hoping to receive your opinion, I remain

Sincerely yours,
Alfred de Grazia
Editor

There was no reply.

4 March 1974

Dr. Eleanor Sheldon, President
Social Science Research Council
230 Park Avenue
New York City

Dear Dr. Sheldon:

I have become increasingly interested over the past few years in the origins of human nature, prompted largely by a growing familiarity with some new ideas that Dr. I. Velikovsky has introduced in the treatment of pre-historic and ancient catastrophes befalling humanity. The field is not new, of course, and several disciplines in the social sciences and

humanities currently share it. But a lively set of controversies with a considerable potential for new discoveries and new syntheses has begun to erupt here and there. Hence there may be occasion for the kind of interdisciplinary research -- discussion efforts that are appropriate to the SSRC and ACLS or both.

Perhaps the eye of the cyclone moves around the question: Did homo sapiens become human and cultured in gradual steps, as received theory would have it. Or was he compelled to think and behave humanly by the effects of natural forces so immense that factors such as sex, commerce, and "normal" invention must take a secondary role in explanation?

In preparing a monograph on the effects of disasters in homeric times, I have encountered and had to deal with problems that are central, not related incidentally, to the fields of linguistics, historical chronology, astronomy, physical and cultural anthropology, comparative literature, archaeology (worldwide), geology, fossil paleontology, soil chemistry, electromagnetics, astrophysics, sociology of sex, ecology, climatology, oceanography, theology, chemical and fossil dating, psychology of infancy and of stress, epistemology, the history of science, and political science for the origins of theocracy, bureaucratic system and collective violence.

The problem of approaching the field is not as impossible as might appear from the listing. It can be stated as an excellent model for cross-disciplinary investigation and theory. The numerous sciences involved have been shocked and compressed, taken aback, you might say, and the time may be right for a reappraisal of where they all stand in reference to the question. I have felt continually the need for the kind of sounding board, stabilizer, consulting resources and motivator that I once experienced via the establishment of the first Political Behavior Research Committee of the SSRC and its subsequent operations.

Should you be of the opinion that the subject might interest the SSRC and be within its jurisdiction, I should appreciate the chance to discuss it with you in some detail

Sincerely yours,
Alfred de Grazia

April 5, 1974

Dear Professor de Grazia:

Thank you for your interesting letter of March 4, in which you suggest a possible role for the Council in exploring human socio-cultural evolution, particularly in the light of an hypothesis that posits discontinuous advances, following a massive challenge and response model, rather than incremental steps.

It is true that this kind of problem is inherently cross-disciplinary, is of potentially great interest, and needs strong guidance if it is to make progress. Also, I am aware that Velikovsky's ideas are receiving wide attention again -- or, perhaps, at last. Nevertheless, the topic you outline, which demands a unified approach is too enormous for the SSRC to handle, and even if the ACLS were to be involved (obviously, I cannot speak for the ACLS) it would still be unlikely that we could marshal the appropriate efforts. At the very least, the physical sciences, as you point out, would have to be closely involved.

As you know, the Council is now addressing itself to more than a full intellectual and administrative agenda, and I cannot foresee a way in which we could be helpful with this topic. It certainly deserves attention, however, and I wish you success in your capable efforts to bring that about.

Sincerely yours,
Eleanor Bernert Sheldon

In reflecting upon all that happened to V. and to Deg and the others, it would be unfortunate to keep one's eyes on the immediate characters alone. For they are all symbols, too, players in a drama, representing types of our civilization. If V. is subject of a hundred book reviews, these reviews are signs of the times that happened to gather electrostatically like fluff around his work.

J.B.S. Haldane, a noted biologist who also wrote on *Science and Ethics*, found V.'s *Worlds in Collision* a degradation of both science and religion, a peculiarly enraging combination, apparently, for a marxist and fellow-traveler, whom Deg, with a long nose for hidden political mazes, suspected might be waving the flag (red,

that is) for his American colleague, Harlow Shapley; and when Deg, duty-bound to probe wherever necessary, intimated these sensings of political psychology, he was scolded by certain naive and intensely tender liberal consciences, as if political processes of leftist politics, external politics, could never enter scientific processes. So he was amused when, in perusing an edition of Frederick Engels' *Dialectics of Nature*, a work which many Soviet scientists find it *de rigueur* to praise highly somewhere in their books and which contributes to biological science roughly in the same measure as Adolf Hitler's *Mein Kampf*, he had to note that the adulatory introduction to Engels' book was by none other than J.B.S. Haldane, who apparently could see contemporary marvels in the century-old work of a communist that he could not perceive in V.'s book. Furthermore, had not Marx and marxists been universally insistent upon the interconnection of all things with the ownership of the means of production and therefore all things were politicized and relevant subjects for investigation.

Indeed, Deg, in his typically optimistic manner (he would pick up a redhot stove), had conceived of the true interests of marxist theory as residing in catastrophism, not uniformitarianism. Why he asked himself, sometime around 1978, did Marx and Engels so strongly endorse Darwin, fashioning the pattern for marxists to follow ever since (the heresy of Lysenko in the 1950's being a significant incident thereto)? Perhaps, he thought, the model of catastrophism did not give them a broad natural inclined plane for the progression of history; it defeats man's greatest works in an instant. It pays hob with the development of the pure but reversed Hegelian dialectic of thesis-antithesis-synthesis in the historical process. It depresses man's will and capacity to build an ultimate utopia. And Marx and Engels, despite their rejection of the Hegelian "will" and ideal, conceived of and nurtured the most fantastically strong human will, one that could overturn social orders and political regimes (of course, with the aid of history). So they needed natural change to back up social change -- Engels waxing polemical on this need -- but the change must not overturn catastrophically the works of revolutionary men.

Still, Deg thought also that the problem of arousing the masses was immediate and paramount with them, whereas, the problem of

nature and history (just mentioned) was less important. Now the masses must see themselves as the symbol or substance for a great tidal wave, storm, explosion, and destroyer. Therefore, the imagery of catastrophe would be more effective than the interminable gradual incremental change of Darwin and bourgeois society. And indeed there are indications the Marx smelled an ideological rat in the theory of evolution. Furthermore, in reading Soviet studies pertinent to quantavolution, Deg could sense a slackness in their basic tie to Lyellism and Darwinism. In the back of Deg's mind there was an ulterior motive, to loosen the anchor of uniformitarianism (or "actualism" as the Europeans call it) in the marxist setting, thus to free up a flow of new quantavolutionary energy.

So Deg wanted to address himself to this problem, and he asked his daughter, Victoria, who was a professor by now, eminent on intellectual movements of the past century, and who said, yes, it did seem like a good idea, and she being much better attuned to the marxist mentality and avant-garde currents in the field than he, Deg promptly submitted a proposal to the political science and sociology section of the Natural Science Foundation. When the refusal came, he asked for and received the critiques of the review panel. He was a little dismayed to discover that he was illiterate and ignorant beyond his worst fears, even more so than most scholars must be on the measuring scale that the Foundation had provided conveniently to its panel.

But when he thought that he might judge the responses to his proposal better if he knew who were writing them, the request was refused, on grounds of "policy," and, of course, the policy was, as is usual, good for those who were in charge of the policy and working behind the defenses afforded by the policy. Momentarily Deg thought to investigate the law on the subject, and to have introduced a bill for laying open such matters, as an amendment to the federal law on freedom of information, or even to launch a lawsuit, seeking a mandamus to produce the records. He didn't do so, of course, because, as my readers by now amply appreciate, *ars longa, vita breve*, Two years later, a postscript to the episode occurs in his journal:

January 20, 1980

A famous letter from Marx to Darwin is said to ask Darwin's permission to dedicate a volume of *Das Kapital* to him. Year before last, the National Science Foundation turned down my proposal to study the question why Marx and Engels, who perhaps should have been ideological quantavolutionists, not evolutionists -- that is, catastrophists, not uniformitarians -- would have so warmly accepted Darwin's group. (The anti-religious connection is, of course, obvious, but the Europeans were not so friendly to Darwin and were non-religious too). Then [1976] came the exposure that the famous letter had not been written by Marx at all and the mistake was traced back to its source in early communist revolutionary Russia. Marx could say once more "Je ne suis pas marxiste" (if he ever said it). I wonder whether he would also have said "Evolutionem non fingo." Probably he was content with two of the thrusts of Darwinism: materialism and historical progressivism.

But enough of foundations, lest I have no energy left for treating of publishers. The lesson that publishers learned from the *Velikovsky Affair* was the same as a first-term convict learns in jail, how not to get caught a second time. The unfortunate victim of the lesson was any author who was preparing a book in the field. Macmillan Company dumped Velikovsky's book and Doubleday Publishers made a good deal of it over the years. All the nice people and the pundits and the heretics believed that Macmillan, Doubleday, and other publishers would have "learned their lesson" and a new age in publishing would dawn. Controversial books would not be discriminated against, and so on. To Deg (I hope that I am not giving him too much credit for saying so), this was utopian thinking, and he ought to know, being a utopian, a "realistic utopian," he insisted, by which he meant precisely a person playing a high risk game knowingly, because the game involved some worthy ideal. He said this to those who called his works on world order, "Kalos" and "Kalotics," utopian.

Publishers, on the contrary, did not venture into catastrophism, nor make any money out of the "pseudo-science" or "fringe science"

of catastrophes. Ransom's *Age of Velikovsky* was privately published, and when later published commercially, sold only modestly. Patten's works were published privately and did well. Deg's *Velikovsky Affair* was handled by two small, high-risk publishers and sold under 5,000 copies, and later in England sold another 10,000 copies. David Talbott's *Saturn* did not repay Doubleday its large author's advance. Melvin Cook's book, *Prehistory and Earth Models*, published in England, sold very quietly and modestly; it was technically written, but an "acceptance" would have sold many copies in college courses, technological industry, and the *Scientific American's* public. Hapgood's book on *The Path of the Pole* sold modestly. Milton's *Recollections of a Fallen Sky* failed to reach the American market from Canada.

Henry Bauer's book on the *Velikovsky Affair* took six years to be published and a University Press did the job (Illinois); since Bauer found little of substantive value in V.'s work, one need not wonder how a pro-V. work would have fared in the same circles. Dorothy Vitaliano's anti-catastrophic book on disasters in geology (Indiana University Press) enjoyed only a small sale. So it is not being pro-or anti-catastrophism that sells, but books on the subject are either unsellable or the publishers will not bring them out or promote them properly.

The most successful publisher attending to quantavolution was William Corliss' *Sourcebook Project*, a household concern, that culled the history of science and current reviews for worthy material, finding thousands, reprinting hundreds, all the while maintaining a nicely neutral position.

What was true for book-publishers held also for magazine publishers. The only magazine with a general readership that gave sympathetic attention to quantavolution was *Frontiers of Science*, edited by Elizabeth Philips. It failed after several years because it was part of a conglomerate operation that used the bottom line to weed out unprofitable properties. The very small journals, playing to between 300 and 1500 subscribers were fully unprofitable. Yet without them, there would have been no means of advancing a viewpoint attractive to millions.

By the rationale of laissez-faire economists this should not have occurred; in fact it is normal in the world of education and science. The contradiction between a society's need for creativity and the resources allocated to creativity is stark. It is further exaggerated in the inner organization of education and science where the more creative the work the less the outlets for it. New journals in the sciences often form out of failures of the reception system. *Theoretical Physics* was founded because some scholars could not get enough of their material into *Physical Review*. Deg founded P.R.O.D (*Political Research: Organization and Design*), to advance new ideas in political science and sociology; it later became the *American Behavioral Scientist*, which was markedly altered in format, approach, and contents when he gave up its editorship in 1965. One of Deg's students, Howard Smuckler, became editor of magazines of Ancient Astronauts and ESP; from the beginning they were given newsstand circulations of 200,000 copies, with the proviso that wild nonsense be given free rein. The most fortunately situated scholar in the country for communicating occasionally his ideas of quantavolution, sometimes subtly, at times explicitly, was paleontology Professor Stephen Jay Gould of Harvard University who wrote a regular feature for the magazine *Natural History*, published by the New York Museum of Natural History with a popular circulation reaching a million readers.

Various publicists such as Sprague de Camp and Theodore Gordon gave chapters over to mocking or explaining Velikovsky, but their books were not greatly affected by these chapters. One of the best of the publicists was Fred Warshawsky who wrote *Doomsday: The Science of Catastrophe*. Picking up Rene Thom's mathematical topological theory of catastrophism, presumably applicable in any field, he applied it nonmathematically, heuristically, in discussing the many works trending toward the quantavolutionary outlook. He undertook with V. a couple of long sessions that curled his hair and set him straight on what to say of V.'s achievements in an article for the *Reader's Digest*. Having escaped perdition, he went on to write a full book on catastrophes, ancient and modern, which was published by the *Reader's Digest Press*. This company made a distribution agreement with Harper and Row, which performed so poorly with his book that Warshawsky complained bitterly to

everyone and achieved some promotional effort. The company then closed down, and Harper and Row stopped selling the book, returning its very large remaining stock. Then McGraw Hill bought rights to the book for its back list, to no effect. Over 8,000 copies were sold, but 17,000 copies were "remaindered" at a pittance. The *New York Times* ignored the book. Some favorable reviewing occurred. It went out of print after only several years. And please to note the way in which an author's "property" is kicked around.

The situation, as I surveyed it, is that not one major publisher has in print a book on quantavolution, excepting Doubleday, Morrow, and Dell, all with Velikovsky, and excepting, too, the New American Library with a reprint of Francis Hitching's *The Neck of the Giraffe*, in which the head of the giraffe is quantavolution, the neck is the long disdainful connecting link, and the body is conventional biology. (For those who might think otherwise, I should say that Erich von Daniken is an "ancient astronauts" buff, not a catastrophist, except in mood. I say this because I am often asked what I think of von Daniken and I respond that he is not a quantavolutionary; he blithely propounds mysteries without worthwhile solutions, but he is, alas, a cosmic heretic.

On October 31, 1982 (Halloween) the 15 Paperback Bestsellers (trade) which were listed in the *New York Times* around the U.S.A. carried six (6) titles dealing with the cat, Garfield. The number one bestseller was "Garfield Takes the Cake," then, number 4 was "Here Comes Garfield," number 10 "Garfield Weighs In," number 13 "Garfield at Large," number 14 "Garfield Bigger than Life," and number 15 "Garfield Gains Weight." If Garfield were missing, Rubik's Cube would occupy several of its places, vying with books on diet. The *NYT* defines this class of paper backs as "softcover books usually sold in bookstores and priced at average higher than mass market."

One cannot read Deg's notes and hear him talk without deriving an apocalyptic view of the publishing industry. "It is a doubly sick industry. It is economically sick and it is functionally sick. By 'functionally' I mean physically, ideologically, and morally. It is dominated by cheap nonpublishing money, coming from extravagant swashbucklers and conglomerates of merged and

paralyzed units. Ownership is alienated from editors, editors from producers, editors from authors. It is characterized by some of the worst labor practices, witness to the shadiest deals, and engages in the thoroughgoing degradation of writers."

This is the way he often spoke. He wouldn't say much and sometimes in a group or committee be quiet, abstracted, even appearing bored. Then suddenly he would be seized, and as if to make up for lost time and to persuade others that he was only speaking because what he was saying was being torn from his lips, he would hammer out the words, scalding rather than sweetening the atmosphere, so that when he finished, there was neither applause nor babble of dissent, but a pause, until someone evasively spoke around him, and when that happened he didn't insist upon his point but subsided for a good while.

Deg could recite a long list of great writers who had put out their own books, he even claimed that most great writers did so. First of all, up until the late Eighteenth Century -- Franklin, Voltaire, the Encyclopedists -- every writer put out his own books, unless, after burying him, friends or relatives printed his work. In a marginal note to one of his late anatomical sketches, Leonardo de Vinci implored his "neighbors" to see to it that his works would be printed.

The publishing racket (Deg's word, not mine) developed sweetly out of bookstores and printing shops where it belonged and should have stayed, but by the latter part of the nineteenth century Balzac was excoriating the thieves and profiteers of the business in an excellent novel, *Illusions Perdues*. Dickens, Dostoevski and Flaubert sweated to carry their novels first as serials in magazines. But where are the magazines, bad as they were, today -- they carry a single chapter, but usually the pain of editing a chapter for a magazine is damaging to both the author and his book.

Is it names you wish? (And he would begin.) Walt Whitman, Friedrich Nietzsche, Stendhal, Beatrix Potter -- yes, *Peter Rabbit* -- James Joyce (an angel helped), Marcel Proust, Rainer Maria Rilke, Virginia Wolfe, Andre Gide (*The Immoralist* issued in 300 copies), Sigmund Freud and, if you will, Velikovsky himself published his early pamphlets. Colette was

published by her husband Willy who even stole her name as author. America's best autobiography, *The Education of Henry Adams*, was put out by the author.

The myth of Thomas Wolfe is used continuously by publishers to show the unknown young writer discovered by the great fatherly editor of a conventional publishing company and led carefully to reveal and convey his beautiful achievements to the world of readers. Even this case is mythical, as the editor involved, Maxwell Perkins, tried to explain in a recent edition of Wolfe's *Look Homeward, Angel*. But the truth will never catch up with the lie until publishing circles come upon a similar myth to serve them.

If Charles Darwin's *Origins of Species* sold out through a book store in 1859 it was because writing and printing were still for gentlemanly use and the book was not deposited behind a mass of their friends. Dammit -- nowadays you can't even sell a book to a friend! Besides there was a prurient and agnostic public altered to the sensationalism of the book. Surely you must know, too, that Darwin's thesis was already well-worn and agreed upon; he was selling evolution even though he didn't use the word and the book's *raison d'être* was the silly mechanism of natural selection, which was nothing more than a watered-down Lamarckianism, a slogan for bird-watchers and garden clubs. It was an easy sale.

Deg had one arrow in his quiver to fire at the now pathetically wounded publishers. They are frauds, announced he.

They pretend to publish the books of the country. Ninety per cent of the serious writing, and I include even novels and poetry here, is put out by government presses of several types, by subsidized university presses, subsidized independent and university institutes, scientific associations, and self-help amateurs like myself. Further, much of the serious writhing put out by so-called independent publishing houses is subsidized, by insider deals, involving mutual back-scratching, agreements to arrange publication of one's editors, promotional devices such that no established book reviewer need fear his shit will go down the drain when there are people who will eat it, [I am sorry, but that is what he said], by quiet subsidies, by guarantees of sales, by tricky deals with film-makers, press agents, television companies, and corporations, and you name it.

At this point I intended to escape Deg ' s diatribes by telling how he came to enter upon his writing campaign and then to publish his own works. Lest you think that such violent opinions as his come out of intense suffering and exploitation, let me once again remind you of Deg ' s character, acquired in earliest childhood: he could be and was often indignant about a person or an institution or a system, without being hurt by them and even while being helped. In a way, he was rather like his children ' s generation and the hippies, except that he had the forcefulness and discipline that produce alternatives; he seemed always to have ready a proposal for another way of doing things. In this way, he was more sprung from the nineteenth century utopians: Fourier, Brook Farm, St. Simon, Marx, Henry George, As you will see here, he didn ' t expect much, he didn ' t suffer greatly, he didn ' t mind sacrificing, and he did not dance a jig when he finished the job. I assure you once more of that great difference between Deg and V. Deg did not see himself as a victim; V. saw himself as a victim.

Deg moved into the field of quantavolution slowly and then ever faster. This I would attribute to his heavy involvement ' s between 1962 and 1966 with the *American Behavioral Scientist* and the design and production of retrieval of bibliographic annotations in the behavioral sciences. During the same time, he was writing heavily in political science, especially on the reform of relations between Congress and the Presidency. After he turned from these in the period 1967 to 1972, he wrote *Kalos : What is to be Done with Our World?* Hired by Simulmatics Corporation, and given the assimilated rank of a general with "Top Secret" access by the Department of Defense, he spent a few weeks in and out to win over the Vietnamese people and to bolster the morale of their own troops.) The job led him quickly into urging measures that were too radical and diversionary for the forces, civilian and military, that were moving in an irresistible death-dance toward the ignominious withdrawal of the United States presence in Indochina.

He was writing poetry and before flying to Vietnam in 1967 he collected his poems and put them to press as the *Passage of the Year*; some of them he framed in what he called an "eccentric," "super-sprung" rhythm. He gave a copy of the book to Harold

Lasswell who said, yes, he had written poetry when young, at which Deg commented that poetry was more accessible to the senile than the juvenile. He gave a copy to Velikovsky who, it appeared, had published a small book of poems under the pseudonym of Immanuel Ram, in Russian, in 1934. V. read Deg's poems and used a quotation from them on one occasion to persuade Deg of a point. Suddenly it seemed that mankind was a secret crowd of poets.

He then joined with a University instructor who had not studied directly with him, and had met in the annual Department reception, Nina Mavridis, a tough, emotional, polyglot petite blonde smartly turned out, whom he later married. They went in search of a Greek island house, and he bought a parcel of land on Nazos, which was then a quiet backward island, and there built the stone cottage facing across the straits to Paros.

He turned to several of his former students, graduates, and "drop-outs" from the system, and together they organized an experimental college, *L'Universite du Nouveau-Monde*, and settled in for a hectic year upon the Alps of Valais, Switzerland. All the while, he visited Princeton, coming and going, keeping in touch with the Velikovsky circle there and with whoever of his immediate family happened to be home from schools and wanderings around the world.

With the University of Switzerland closed down, the United States withdrawing from Indochina, his work on a new world order totally ignored, his family disassembled, efforts at reforms within New York University ending only in cosmetic changes, and resettled efficiently with Nina in an apartment of Washington Square Village, just across from one of his classrooms, and a block from his office, Deg drove through the resulting energy gap into the field of quantavolution. He completed two books of political science during this period, neither requiring heavy research but both of which, *Politics for Better or Worse* and the "lectures to the Chinese", *Eight Bads, Eight Goods*, he considered as "state of the art" philosophically, and innovative in format and perspective. Both were "successes," he thought: neither earned much money \$18,000 in the first case, \$3,500 in the second.

His University teaching had never in his career cut very deeply into his time for study and writing, partly because he did not "pal around" with students and varnish their wasting time. Too, he avoided committee assignments that seemed useless, and had little need for generalized social encounter. During nine months of the year, he gave an average of twenty hours per week to straight pedagogical work; the rest went into his projects -- editorial, political, pedagogical, consultative -- and writing. Wherever he had taught, including New York University, he was expected to be a "producer," to do research and writing in return usually for a lighter teaching and committee load. He was usually expected "to bring money into the University," which sometimes he did, and to find funds for his research and activities, which sometimes he did. He used his time fully and completely for these latter purposes, working year-round, seven days a week, for three to twelve hours. (obviously, everything did not "come easy to him," as so many acquaintances believed.) His journal slackened off, through the sixties and seventies, entries occurring only every several days on the average and even then deprived of events recited in their fullness.

He rarely spent more than ten minutes on the day's newspapers; he watched television several hours a week; he listened little to music and rarely played his trumpet any more, but often was humming and whistling to himself. Except when reading a novel or a poem, he did not read in the conventional way. Reading was an instrument of research and writing. He would pounce upon a book or article and seek directly the point that he was addressing, which had made him pick up the work in the first place. If it wasn't helpful, he would put the work aside. He could rarely be trapped, for instance, by some lurid description of a disaster. At the rate of 100 pages an hour he could tell whether there was anything useful to him in a succession of books or articles. An issue of *Science*, though it might contain 100 pages, would ordinarily occupy 10 minutes, just enough time to see whether there was something of interest in it. He would however, spend hours on a relevant two-page article in a strange field -- a paleontological article using explicit chronometry, for instance, learning the method used, looking for the expected illogical turn or twist, the weak point in a piece which after all had

been fashioned with extreme care, was the darling of the authors' eyes, and had been rigorously criticized by conventional readers.

At first both current materials and ancient materials on quantavolution were not so easy to find. Stecchini was alone as supplier of references outside of V.'s works. As the network of scholars like Mullen, Juergens, Milton, Crew, Sizemore, Moore, Lowery, James and several dozen others came into the field the supply of references grew exponentially. *Pensée*, *Kronos*, *The S.I.S. Review and Workshop* and Corliss' *Sourcebooks and Newsletter* brought hundreds of citations to light. I cannot do less than say that the names of the hundred authors of the articles and notes in these magazines is the measure of 90% of the field. If screened for relevance and translated into quantavolutionary terms, several hundred more names would be added -- not that they would gladly accept being added -- from the conventional output of scientific books and journals.

In a combination of disgust, impractical judgment, and worthy motive, he decided in 1977 to resign all obligations to teach and supervise dissertations and to be at hand for the various faculty meetings; he found the University ready to pay him a third of his salary to engage solely in research until he would arrive at the age of 63, after which he would be considered as fully retired. The agreement was soon followed by a considerable general inflation of the economy, and a reduction in foundation activities, so that he was constrained to stringent personal economy, not so evident on the surface, but oppressive in reality. He had no illusions about the interests of foundations and government research agencies in quantavolution and in fact received no help. He earned a little money here and there, whatever could be done rapidly without taking his money here and there, whatever could be done rapidly without taking his mind off of his quantavolutionary studies. He sold a piece of land on Naxos. He sold, too, a small house he had bought for his retirement, near Brown University where he had once taught and close friends still lived. These funds and more went into research costs -- typing, Xeroxing, travel -- and to the occasional support of his mother and other family members. Nina, although she finally earned her doctorate, and was a most effective teacher, could not get into and hold onto a position in one of the

college systems of the New York area. Whatever money she had, she spent fully and equitably. This is no place to speak of her at length; she was everywhere in those years, but when Deg comes to tell of Naxos, it will be up to him to tell of Nina. By the middle seventies, she and Deg had split, and came finally to see one another as friends only, there on the island where she bought and remodeled two medieval Venetian homes and lived with her husband Peter whenever possible.

Deg's first book in the Quantavolution Series, *The Disastrous Love Affair of Moon and Mars* was written in the early seventies. He had thought for several years that he should write a textbook on what he was then calling revolutionary primevalogy, but before he had settled among several outlines of the work and written a few passages, he reached back for a journal entry written while staying at Pythagoreion on the Island of Samos and decided to try out the new field with a case study.

Pythagoreion, Island of Samos, July 12, 1968

I have come across and read for the first time closely and consciously the song of Demodocus at the house of Alcinous. How wonderfully it describes what Velikovsky said was the actual set of cosmic events of the Seventh Century before this era, of how bright-crowned Aphrodite loved the god of battle Mars-Ares, and how they repeatedly fucked "in the house of fire," whose master, Hephaistos, finally entrapped them in a net and put them upon a more pious course. The passage must be analyzed Word for Word: the parallelism is beyond coincidence; either Velikovsky wrote the myths of the Greeks, or something like the physical events he describes historically took place.

The story referred to is a brief lyric of a hundred lines, sung in Book VIII of the *Odyssey*, the epic poem of Homer. It tells of a much longer opera ballet sung and danced for Ulysses.

Deg showed his manuscript to Juergens who was surprised at its coincidence with his own electrical theory of the events, which was to appear ultimately as two articles in the magazine *Pensée*. V. would not read it. Deg wished to dedicate it to him. V. said let Bill Mullen read it and if he likes it, go ahead. Mullen did, very much.

Cyrus Gordon liked it, but could not respond to the astrophysical scenario. Further he suspected Aphrodite to be Venus, not Moon. The English acquaintances of Deg got onto the manuscript when he submitted it to the publisher, Sidgwick and Jackson, who had published *The Velikovsky Affair* in England, and he showed it to them. They liked it, but in all conscience could not accept the identification of Aphrodite with the Moon, for they identified her instead with Athene, Ishtar, and the morning and evening star, Venus.

This disagreement meant that the English group was ready to dispute an important point of Velikovsky for, in his application of the *Iliad* to the Martian disturbances of the seventh century, he had found Aphrodite joining with Ares in the Trojan War to fight against Athene. Whereupon, and for other reasons, Aphrodite was assigned to the Moon. Desertions were numerous on this score. When James published a critique of Deg's identification of the goddess, it stood without rebuttal, and Cardona, Rix and others were convinced of James's case.

American publishers were not turned on by the *Love Affair*. W.W. Norton, through Brockway, said it was well written but not to their tastes. So it went with one publisher after another, Simon and Schuster, Dodd and Mead, Doubleday, Random House, Harcourt Brace, Stein and Day, Princeton University Press, Harper and Row, Atheneum, Sidgwick and Jackson, Free Press, and even the New York University Press (unless a subsidy were paid). Deg thought he should "toot his horn" perhaps, as his mother used to tell her boys, so he prepared a blurb about it.

He made the *Love Affair* sound as if it might attract the masses, but publishers were quick to point out that the book was serious, learned, of dubious validity, and sophisticated: in a word, forget the masses; indeed, betake yourself to a university press. But Deg knew already the university presses were eager for wide publics, undercapitalized, dominated by editorial committees of the more conventional members of their faculties, and slow and painstaking to a fault. He visited Jerry Sherwood of the Princeton University Press. She returned the manuscript in time with the expected advice. Deg stopped peddling the book. He was too busy with the

general work, *Chaos and Creation*, to carry on the sometimes interminable pingpong of serious publishing.

Time after time over the next decade, he would pause in his work to recalculate the options of his predicament. Naive friends counseled him: "Any press would be happy to consider your books." A publisher encountered would say, cordially, "Let us see it by all means." Get it down to 160 pages -- less. No footnotes. One only, not really new, idea. The emerging rule seemed to be: "Never underrate the unfitness of readers, media, and publishers."

Yet it was like a drug, this pushing one into the marketplace, or like television, One succumbed from time to time, had a bad trip, and came away cursing himself for not having avoided the encounter. The condition of the publishing industry in America was unbelievably bad; would that it were terminal. All that could be said of it was that it was freer than publishing in Nazi Germany or Soviet Russia, or for that matter in most other countries. It was as bad or worse than the political system of the United State in meeting its obligations, much worse than the educational system with all its weakness.

But unhappy thoughts of this kind did not obsess Deg; they occurred often for a moment (as when he examined the book review section of the *New York Times*, or looked at a publisher's list). Long before, in the days when his work seemed ordinary, when his means of rewarding and insulting were conspicuously in readiness, publishing his books and articles was no problem.

The society, however, was enveloped in the myth that the publishing process was a logical affair, constrained tightly by the message between the covers. A writer's fortunes were thought to vary with the quality of his message. So many useless and dangerous myths rule society! Like the myth among scientists of myriad readers perusing their article in a reputable scientific journal -- 10,000? 5000? 500? yes, 50 and feel lucky.

Now, all of this jeremiad is preliminary to announcing that at certain point in time, probably it was in 1978, just after he began his final race against dwindling finances, Deg decided that he would,

unless intercepted by an angel, proceed to complete his work and then by one means or another publish it himself. Somehow the money would be found, and he thought to publish it in Bombay, where he had connections with friends and a publisher, the Popular Book Depot, which had produced *Kalos* and *Kalotics*.

One premise he maintained firmly: he would not be finally frustrated and incapacitated by the publishing system. Another premise was his delusionary Paternoster: that what he attempted might be great importance to mankind. It was the best work he could set himself to -- and who else could do it -- none whom he knew of -- and his other great object in life, a new political order of the world, offered at this time no opportunity nor chance of success.

The decision was not easy, hardly definite in fact, because like many decisions he made, it was long foreseen and warmed upon a little burner in a recess of the mind. It was not an optimal solution, by any means. The myth, social binding, and conventions of publishing are so pervasive that none of his acquaintances thought this procedure wise, prudent, or even possible. All too poignant was his awareness that the controversial matter that he was writing would combine with its unorthodox publication into a hard prejudice against the books. Under such circumstances, more than a touch of megalomania is needed.

He pushed ahead imprudently, erratically, and stubbornly, or so it seemed to others, and they were correct, but they could not see how such failings of character might add up to an achievement. He wrote everywhere and under all conditions on all sizes and kinds of paper with pencils and pens of any type, and now and then on typewriters, electrical, or a portable, mechanical one. He read in several libraries, bought very few books, was sent Xerox copies of many pieces by Sizemore, Milton, and others, corresponded, and ultimately had made notes on some hundreds of books and articles. These were often caught on the wing, and he was often exasperated upon completing a book to have lost a citation, forgotten the spelling of a name, left relevant pieces now in Greece, now again in New York.

There is nothing special to recommend in his research and writing

procedures except what one cannot anyhow imitate: a wide-cast unerring eye for the salient, the strong background of methodological -- especially epistemological -- thought and theory, a modest skill at writing, a great skill for synthesizing material, an inborn will to let nothing stand in one's way, a lifetime practice in doing much with little. Once in the while he got help; Donna Welensky, whom sometimes he paid for her typing and sometimes not, whom he came to love for her energy, efficiency, and ineffable kindness to the world, never mind her brawny blonde beauty.

The latter half of the dozen strenuous years were dominated, physically speaking, by the presence of a quiet deep-voiced dark-haired, brown-eyed, French novelist whom he encountered first at Naxos, where she was joyfully spending a few francs that her publisher had let her have as a consolation for not publishing her latest book, *The Paladin*. With great difficulty for her assets were almost literally on her back, she obtained a visa to come to America, and thenceforth Deg took care of her, and she took care of him. In 1982, they married. They lived in New York City, at Princeton, in Washington, on Naxos, and in Paris, appearing more affluent than they were or pretended to be.

They visited her ancestral village, Habsheim, between Basel and Mulhouse, they traveled to England, Italy, Hungary, and Canada. She loved the journeys and loved Deg and adapted quietly, imposingly, to the net of human ties and implausible projects of Deg with a broad, engaging and ever-ready smile. When Elisheva, sculptress forever, met her for the first time, she was awestruck at bones that made her strong hands ache for a chisel and hammer. "How did you find such beauty?" she asked Deg. She could be happier than anybody whom Deg had ever met, under the poorest conditions of life -- but then, as he often said to her, and she fully agreed, we are much better off than humanity is or has ever been or will be.

In more than a decade from 1972 to 1983 Deg gave over perhaps no more than eight months to work outside of quantavolution. Almost all of these few months was spent consulting directly and indirectly with the *National Endowment for the Arts* with Carl Stover, a friend of thirty years standing. Given a general directive

and promoted by Carl before Nancy Hanks and Livingston Biddle, directors of the Endowment, Deg wrote a number of sketches of what might be done to stimulate a broad range of cultural areas, but principally he committed a trenchant irony called "1001 Question on Culture Policy" in which using the format of a book of interrogations, he was able to say all that he wanted to say. The work was an implication that nothing intelligent and basic was being said about public policy on the arts and humanities. Stover even managed to obtain from the Ford Foundation a subsidy with which to send copies of the work to most prominent leaders of the organization and direction of cultural affairs of the United States. Copies were also distributed in Western Europe. The effects, so far as might be perceived, and disregarding the encomia that are easily aroused by techniques of publicity, were nil.

Otherwise the quantavolution investigation progressed and enlarged grossly. By 1975 the basic *Chaos and Creation* was calving. The theory of *Homo Schizo* emerged and went one way,, ultimately two ways, in two volumes, one on the origins, one on human nature today. A great fragment fell out of *Chaos and Creation* and became a treatise on exoterrestrial aspects of geology, *The Lately Tortured Earth*. On a sojourn in Naxos there occurred an idea for an article explaining why the Pharaoh should have pursued the Jews in Exodus; quickly, stimulated by conversations with Anne-Marie, it transformed into a book of exhilarating discoveries and, in the end, *God's Fire: Moses and the Management of Exodus*.

He had already devised a theory of how the solar system might have enacted the set of quantavolutionary dramas which he had been uncovering and classifying. He wrote of it to Ralph Juergens. He found agreement there, and then he achieved the support of Earl Milton, Earl opted to come in on the enterprise of a book; Ralph became engaged, too, but hardly had Earl gone down to Flagstaff, Arizona, to go over their preliminary notes with him, than Juergens died suddenly, of a heart attack. Over several years, in Princeton, Washington, Manhattan, London, and Naxos, and by telephone and correspondence, Milton and Deg worked to complete the book. Its Index, in a unique format, which they named the Omnindex because it merged glossary, bibliography and key words, was finished at the Cosmos Club in Washington, D.C., on February 16,

1984.

The *Moon and Mars* book was standing by for revision. *The Burning of Troy*, its title taken from its first essay on the calcinology of Troy IIg, was organized to contain studies, reprints, essays, and notes. *The Divine Succession* was taken up; its central theory, that all gods are of the same family, was put forward; an anthropological and psychological discussion of the major aspects of religion followed. Then, as Deg stood back, gazing anxiously and unproud into the manuscript, there came to him the idea of adding two new proofs of the existence of gods, and also the scheme of a catechism for whosoever might wish to contemplate a possible new religion alongside the old.

There was left only *The Cosmic Heretics*, which I undertook to write. Its origins lay in Deg 's intention, growing over some years, to write an autobiography in half-a-dozen volumes. He still nourishes the thought, cowering over the prospect of its passage through the gauntlet of fast-gathering, spiked-leather-fisted knights of time. But perhaps I can also do this job for him.

In 1980 he sent off *Chaos and Creation* to India for production. Delays were many. Stephanie Neuman lent him \$3000 to defray some of its costs. He paid her back two years later. Funds came in from the sale of the book through the mails to lists of friends and of purchasers of William Corliss ' *Sourcebooks*. Corliss himself sold copies. But larger sums were needed. They came from an advance of Ben Gingold, a friendly architect who intended to purchase land in Naxos from Deg, from cashing in 10% of the annuities that were to take care of his retirement, from yet another property sale, and from a personal bank loan. Household economies were the rule. The logic was simple: a small saving enabled thirty letters to be sent out, thirty letters might elicit a couple of orders. Deg and Aim moved into a dingy little brick house on an old street of Trenton, in a neighborhood that sociologists call by the menacing term "marginal."

Publishing in India was becoming costly. The Indian rupee which

should have lost its international value, maintained itself steadily against the dollar, letting India pay its debts at a loss of export, but then it exported little anyhow. Nevertheless, Deg let himself in for a third round with Indian printers, sending off in early 1982 the bulky manuscript of *The Lately Tortured Earth*.

He rationalized his private publishing company in a memo to readers, but then decided not to print it in his book. Here is a better place for it, so I am carrying it:

A Note on this Edition

The Edition is intended to bring the materials of *Lately Tortured Earth* to the attention of the small number of scholars and students who are directly involved in research into quantavolution and catastrophe. It has not undergone the ideal processing of several expert readers, critics, and editors. It has been published for the very purpose of arousing comment and criticism.

Four major reason occur for this procedure:

There are inordinate delays and difficulties in publishing through the natural channels of the trade book and textbook publishers and university presses. This book and others in the quantavolution series have already been in manuscript form for some time. It may be better, therefore, to publish the work promptly in this manner than to let more years slip by until finally some convinced entrepreneur will be bold enough to undertake its publication.

Since the work enters upon numerous fields of sciences and humanities, expert readers would be required, a veritable conference of critics, and, logically in each case, a possibly unfavorable critic and a possibly favorable one. Many copies, much time, and thousand of dollars in fees would be needed. Based upon the author's experience with the editorial services of some prestigious publishers, the cost is too high to pay. Publishing the book on the author's responsibly alone will enable hundreds, instead of a score, of experts and students to weight the validity and utility of the work.

Third, authors of unusual theories and controversial types of evidence are strangers to specialists of most relevant fields.

Foundation support, university backing, and publishers' advances are practically impossible to obtain, all of which might otherwise be used to avoid editorial, factual and linguistic peccadillos and to comb more efficiently the library stacks for materials on "non-fields."

Fourth, new high technology has come to publishing, but there is a shameful disparity between the high-level technology abundantly available for the most useless kind of publications and deeper problems of human culture and natural history, most of which necessarily occupy the attention of only a few persons. While university presses, never an ideal solution, deteriorate and while commercial publishers vie for scrupulous material, and while publication technology vies for faster addressing and delivery of junk mail and selling computers for games and word processors to enchant the bored secretary, those to whom consigned the progressive evolution of culture are hard put to survive, assemble, and operate the tools of their trade.

We hope, in sum, that our readers will be fully critical, yet tolerant of our not so sleek editorial packaging.

Delays loomed up in India with *Lately Tortured Earth* so he turned to domestic production. Once again he had to review all of the possibilities for cheap book production in America. His initial constraints were several. He needed a secure conventional binding, preferably cloth or sewn. He could not publish in a large format, say 8 1/2 x 11 inches, because he wanted to put the book before the reader in a familiar form. He needed a bookish type font, an even right margin, running heads and other "luxuries" that American readers had come to expect and demand. He wished to insert many illustrations; this would be costly if they required redrawing or screening.

He observed the rush of new technical systems, computer memory word processing equipment, "perfect" glue binding machines, automatic cameras, small presses of various kinds and alternative Xeroxing machines. None of the products and suppliers with whom he treated had a clear perception of what his needs were and he found himself lecturing them about the greediness and unresponsiveness of industry that is set up to treat deferentially the unconscionable matter of junk mail and the industrial wordage of

the culture -- and he would sound off sometimes on the gamut of the intellectual pariahs, the serious writers, artists, and scientists.

From time to time he would play with the design of an ideal system of personal and small-group publishing at a cost the humble creators of culture would afford. He put aside consideration of systems of microform production and distribution, because the fast culture was still too slow to accept them. He foresaw in the meanwhile a word processor with software for book-setting; a memory capable of handling a book as a whole; software for intelligent spelling and indexing and storing and addressing networks of acquaintances and potential customers; big readable screen; means of composing tightly and finely; a tape that could be stored and would feed a composer that could be slow but must print out a handsome book font and a generally useful caption font. Then the output, automatically paginated, would be pasted up on cards, the cards then printed in multiple copies on a reliable copying machine that could handle from one to a hundred copies of four pages (11" x 17") at a time, after which a collating machine could fold and merge the pages into a book that would then be placed into a thermal, glue-binding machine, capable of handling up to a 500-page text with its covers, be they cloth or card. Next the book would be trimmed, then, if cloth-bound, jacketed with a paper that had been produced by the same system. The small edition, by which Deg meant from fifty to five hundred copies, would be shelved until sold and shipped. Meanwhile the announcements would be coming out through the same system and would be addressed by the automatic print-out of the stored customer and complimentary lists. Small gadgets and work routines would be devised for the interfaces of the system components. The whole publishing company would fit in a garage or basement comfortably. It should not cost more than \$20,000, including initial supplies, and a year's maintenance contract. It should be affordable with a \$2000 down payment with the balance plus interest in extended payments over a 36-months period. Facilities for the bookmaking announcements, or its equivalent in magazine and pamphlet production would be provided; actually a much larger output would be possible.

The system he envisioned is quite feasible technically. Beginning in

1981, Deg could set forth the named components and locate their suppliers to provide a complete system in the range of \$30,000, but the system would have uneconomic, inefficient, superfluous, and flawed elements. The field was moving rapidly. At some moment, it could be brought together and a revolution in publishing accomplished. Or rather, what would happen is that the great majority of thousands of creative groups of the nation would cut themselves off effectively from the commercial and university press publishers, building firmly and at a cost they might afford the printed communication network which they needed if they were to survive. When a company called the *Who's Who of Contemporary Authors* circularized him, asking the usual information and adding a request for "words from the wise," he wrote (May 18, 1981):

SIDELIGHTS: "Two futurisms for the debased and desperate intelligentsia: A) With the decadence and collapse of the publishing business, creative writers should discover how to publish themselves and reach their own special audience; commercial publishing is 95% an exploitative delusional myth. B) With the decline and collapse of the existing world system, the free intelligentsia should cut back on writing just anything for money or prestige and begin to assume responsibility for picturing and propagandizing a revolutionary new world order."

He never got around to seeing whether they printed it.

Nothing approaching a new full mini-publishing system was achieved by Deg with the Quantavolution Series. The name "Metron" meaning "Measure" was revived from a personal reporting, consulting, and publishing company he had employed mostly in the 1950's and 1960's to put out the *American Behavioral Scientist*, the *Universal Reference System*, and books and reports. Now it was to be the name of the first quantavolutionary publisher. The means of publication were only half-new, a melange of all ordinary systems. Word-processing with photo-composition by large machines, Compugraphic composition, and old hot-type linotype systems and by already old-style small offset presses. Bindings ranged from Smyth-sewn cloth-covered board binding to new compact "perfect" thermal binding. Deg designed all the covers and the format, under heavy constraints of

format, color, and costs.

The printing and publishing industry was in a technological and marketing revolution and it was annihilating the old breeds of manuscript-evaluator, copy-editor, proof-readers, and designer. All of these operation now were more expensive and provided less reliable and competent services. Deg arranged much of the composition, printing, and binding with Rick Bender of the Princeton University computer center and with the University's Printing Services. They became adept at running small editions in the interstices of time that occur with a large computer and photocompositor.

In all, the labor of his wife and himself as designers, editors, typist, clerks and managers of production and distribution, would have cost \$65,000 to purchase as services on the open market. Direct research and overhead costs (actually paid out or otherwise absorbed) came to about \$60,000 over the whole time; direct production costs amounted to \$41,500; early mailings and advertising cost \$6,000. Without any allowances for the author's time or advances against royalties (he being the author), the total real cost amounted to \$172,500. The total number of books produced was only about 6,000, and many of these were not intended for sale. The editions were numbered. The average real (but not cash) cost per book, then, not including any compensation for the author, amounted to \$28.80 per copy.

When I spoke to him before turning this page over to the printer (taking care not to be seen laughing) the returns had totalled \$7,500. He expected receipts to reach \$30,000 in a year's time and finish off the balance of immediate direct costs, \$17,500, during the second year. This would also exhaust the first edition copies. The main chance of compensating for the \$125,000 of other non-monetary but poignantly real costs would be to sell rights for new editions to other publishers. As for the royalties of the author, in our simulated account here, these would have to wait until further new editions were issued, and were ticketed for archival expenses. Apparently the *avant-garde* or heretical author is frustrated whether by the publishing business or in his own efforts to reach out and communicate.

Deg was continually irritated by the ignorance of the intelligentsia concerning the engine rooms of the ships carrying them. They are brainwashed by the language of Hollywood, in the markets of best-sellers, and in the display quantities of ads of rich corporations. The intellectuals, with few exceptions, inflict upon their creative brethren the oppressive standards of the rotten rich --fame, money, connections. Dick Cornuelle and Deg enjoyed examining some of the exquisite typography, color-drenched illustrations, and perfect printing that went into annual reports of companies which had bought dearly Cornuelle's more than ample writing talents. No expense, no technology, no skill was spared to convey to some thousands of barely interested shareholders and stockbrokers how well or badly the managers had run their affairs during the year. The annual report, no matter how expensively published, was but a trifle in their operating costs of the year. Yet it would have covered the costs of publishing beautifully fifty creative works.

Where are all these creative works? Is that the objection? Most of them are abortions of a culture of intellectual and science prostitution. They do not appear because they cannot be carried to full term. They do not appear because they expire too in their creator's archives. And this is why Deg, as he came to the end of the Quantavolution Series and I near the end of telling its history, began to harangue his family and intimates to set up an Institute for Creative Archives. A billion dollars a year, he claimed, is the cultural loss to the American nation of the death of the archives of its creative workers. This was a real loss, not registered in the unselective National Economy's Accounting System. He wanted to do something about it.

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