

CHAPTER TEN

ABC'S OF ASTROPHYSICS

In his journal of January 12, 1968, Deg writes of a conversation with Professor Lloyd Motz of Columbia University, the same who had called the attention of scientists to Velikovsky's successful predictions of Jupiter's radio noises and Venus' high heat:

Motz turned out to be a cheerful sort, full of admiration for Velikovsky, but of course entirely convinced that the laws of gravitation and thermodynamics are much more positive proof against Velikovsky than are some historical events of which Velikovsky may have proof positive. (...)

Motz is going, obviously, by deduction from laws that he regards as immutable. He feels simply that, whatever the historical evidence may be, it would be impossible for enough energy to be generated on Jupiter to launch Venus by eruption into the heavens. He wonders whether there might not be some third body that had appeared in space and constituted a counter force that have drawn off or helped draw off Venus from Jupiter or whether Venus had come from somewhere else in space. I pointed out that Velikovsky is firm at this time that Venus must have come out of Jupiter by eruption (But not volcanic eruption -- rather from disequilibrium owing to Saturn) and that we have no knowledge of a strange third body that may have been in space at that time within the planetary system, else we might have heard the name given this body in the records of the times. Still it is worth keeping an eye out for such an intruder. Motz says the same problem besets those who think of quasars as a high-intensity explosion, an eruption from larger bodies. Where can the energy come from, he says, and how could it gather together?

With Director of Antiquities Spiridon Marinatos in 1968, Deg met astronomer Constantinos Chassapis who had studied the Orphic

Hymns and derived certain conclusions about Greek astronomy in the second millennium B.C. The Hymns, he asserted, had originated between -1841 and -1382, but probably in the 17th century. They showed the Greeks to understand heliocentricity and the sphericity and rotation of the Earth, and spoke of the attraction of the Sun as the source of orbital movement, and named the planets, the seasons, the atmosphere, and the ether beyond. Their calendar was of twelve lunar months; they identified Saturn with time; and they referred to a universal law that regulated the universe and stabilized the Earth.

Stecchini, Santillana, and Von Dechend, among historians of science known to Deg, were quite persuaded of the advanced state of the most ancient known science, so Deg was rather more impressed by the indications of modernity in Orphism, which Chassapis was exhibiting at the same time. If the hymns had originated so early, though, they went to prove a uniformitarian history of the heavens. Incompetent to challenge Chassapis' readings, Deg could but question the definitiveness of the poetic lines, which seemed indeed vague, and the technique of retrojecting the present celestial motions unjustifiably.

The Orphic Hymns, Chassapis also maintained, evidenced an early knowledge to lenses. This, too, rankled with Deg. He had worried over a mention of a lens-like object found in Ninevah's earliest levels, and had discussed the general question with Stecchini. If the Bronze Age peoples had been able to magnify the stars, meteors, planets, sun and moon, they might also have derived proportions and distances among the planets, this making Jupiter the King and Saturn the retired king. Too they might thus have perceived the rings of Saturn and bands of Jupiter. They might then for religious reasons, and because humans are anxious animals, have created a body of legends ascribing to the heavenly bodies the various adventures, including approaches to the Earth, that the revolutionaries said were historical occurrences.

Stecchini believed that the ancients had lenses, or at least would have built concave disks of copper alloys polished to a high reflectivity. He wavered often in his basic position about cosmic encounters. Always quite happy to play the game of catastrophic

models, he might still be readily influenced by Santillana or another colleague to believe that other solutions might be found in the messages sent down through the ages by the earliest voices.

Deg, on the other hand, even when he postulated ancient telescopes, could not explain away the concordance among ancient voices; did they have telescopes everywhere? Moreover the explosive speech of the modern skies and terrestrial crust were seeming to make a point. Not until 1980 did a space vehicle confirm the great and incessant electrical discharges of Jupiter, but then he had for fifteen years been persuaded that the legendary electrical behavior was real, and on a much large scale than anything that might be observed today. The same concordance on many other matters was consistent, too, with ancient legend. If the ancients had telescopes, they would have previewed the catastrophes but could only have modestly exaggerated them in their mythology.

A possibility existed, he thought, that the theocratic elites, here and there, using telescopes, would purvey to the masses distorted history, where legends survive and where are perpetuated some happenings and forecasts; but there would be no compelling reason for widely divergent cultures to achieve consensus on these. Why, let us ask, would the priests of the Jupiter (Yahweh, Zeus) age, using telescopes upon calm heavens, invent catastrophic heavens of the time of the birth of Jupiter, and of the earlier times of Saturn?

For that matter, the great telescopes of the past century have not induced uniformitarian astronomers to alter their dogma of a calm celestial history. However, they have made an increasing number of observers proto-catastrophists. So telescopes, even if the ancients possessed them, could not impress catastrophes upon men who had not experienced such. If Venus simply seemed big and beautiful enlarged 50 times, why would men go berserk, catatonic, orgiastic at her regular, safe, distant approach? Fossil telescopes could not affect quantavolutionary theory. They might even support the notion of cultural hologenesis that Deg espoused.

The great Book of Venus was of course Velikovsky's *Worlds in Collision*. In Deg's long acquaintanceship with the book there

developed practically no significant errors of astronomy or geology, errors or omission of sources, or misreporting of legends. There is some exaggeration and "purple prose", as in the title that suggests explosive impacts between the planets Venus, Mars, Earth, and Moon, which he does not claim in the book itself. The style is less timid, hesitant, than might be deemed appropriate. There are hints of arrogance as he warns of the dire fate awaiting the theses of Darwin and Newton (less unseemly today than in 1950, however). There are no appeals to religion, only rare confusions of "ought" and "must" with the factual "is". A certain repetitiveness occurs that may be impossible to avoid, but which nevertheless tends to overstress and amplify some catastrophic occurrences. He avoids scientific and pseudoscientific jargon and the coinage of terms.

I cannot here defend all of this, of which the first statement is already shocking: that "there are practically no errors of astronomy?" How can a book that enraged many astronomers commit no errors of astronomy? Apart from the main reasons, which are sociological and psychological, there occur two substantive reasons: Velikovsky established his natural history by assertions of fact; certain events either happened or did not happen and we weigh the evidence tending to the one and the other to arrive at a judgment about planetary behavior. Second, after this is done, Velikovsky asks how can the laws of astronomy permit such happenings. He understands the laws. But when the behavior of the heavens does not conform to the demands of the laws, he offers briefly some ideas as to what may improve the laws, such as the introduction of a larger measure of electrical transactions into solar system behavior. He reasons the same in respect to geology.

In legendary matters, he follows Euphemeris the Sicilian (fl. 300 B.C.) who established the scientific canon that a myth is to be explained by natural causes. And when Dorothy Vitaliano years later attacked Velikovsky while espousing euphemerism herself, she failed to realize that she was merely reducing Velikovsky, not supplanting his method, which was the same as her own.

By the standards that Cook, Bruce, Juergens, Milton and Deg came to set for sky-body conduct, Velikovsky was actually conservative and conciliatory to the establishment. He was heretical but not a

full-scale quantavolutionary. Deg came to feel almost perfunctory when he argued for the middle-road quantavolutionaries like Velikovsky.

If a mini-microphone had been implanted in one of Deg's large ears, we would be entertained by a litany of quantavolution over the years, emerging from an analysis of his stream of discourse whenever the subject occurred, whether it would be in Greece, Manhattan, or Washington, Princeton, London, Thailand, or India. What happens is this: most educated people are unaware of the case for quantavolution; the subject is perennially interesting; it is impossible to state or argue a full case; certain sloganized propositions are proven over time to have an enlightening and convincing effect; these slogans are packaged and delivered in personal and group conversations, with a couple left out where unnecessary or deemed inappropriate.

I have not had the advantage of an elaborate study, but I notice the frequency of these statements, prefaced by something like: "more has happened to change the world by catastrophe than by gradual evolution."

"Religions are obsessed with primeval disasters."

"Mankind has always been fearful of the skies, such that terrible events must have happened there."

"Venus is hellishly hot and locked to the Earth."

"Mercury now is believed to have been recently relocated."

"Cosmic disasters destroy time measurements."

"Big changes in the biosphere are connected with general catastrophes."

"Ancient legends from around the world confirm each other."

"The surfaces of Earth and its neighbors have been torn up recently."

"The world is electrified from universe to atom with potentials that can overwhelm gravitational forces when exercised."

"You can't determine what happened in natural history by natural processes nowadays."

"Science is as non-rational as any other kind of behavior."

And other such simplicities occur more or less frequently. Whether tossed out in defense or in exposition, the expressions collide with a variety of phrases with which the well-educated person is equipped,

such as:

"Gravitation accounts for the solar system."

"All methods of chronology give very old ages."

"The solar system has been functioning as it is for billions of years."

"You can 't trust legends: they say everything and nothing."

"Evolution is a fact: it took millions of years to change the horse 's foot to a hoof."

"The oldest features on Earth are hundreds of millions of years old."

"No imaginable force can move the Earth without exploding it."

"Venus ' thick clouds work to make it like a greenhouse."

"First came myths, then religion, then magic, then rational science."

"Any local disaster can be exaggerated to huge proportions."

After the clash of these sets of slogans is amplified somewhat, the discussion is usually turned off or diverted. Book reviews and scientific table-talk infrequently go even as far. Once in a while a foray in strength is launched by one or the other side. Even so, rational discussion or exposition does not ensue, but rather an elaboration of one of these slogans with the citation of authorities, or with dogmas more elegantly stated.

Rarely does the exposition break out of the brush into the clearing. It would not be an exaggeration to state that in the two decades about which this book talks, no more than a dozen public presentations have occurred in which a systematic attempt has been made by a practiced and specialized scientist in the face of opposition to destroy and bury one or another facet of quantavolution, such as the capacity of moving the Earth without destroying it.

If this condition appears incredible, it is because so few people understand the sociology of scientific communication, or human discourse of any kind. Scientists can answer questions that they pose for themselves, and spend most of their time doing so, and encourage their "stooges" to ask these questions; but they cannot well answer questions that are asked by others, true others, who come out of a different mentality and have different purposes in

mind.

Take an example from Deg 's experience in these years from a quite distant field, political science, where in parts of three different books he proposed a single equal tax on every living soul: that the annual budget be divided by the population to figure the tax of each one. The shocks, reverberations, incomprehension, suspicions, reservations, indignation and flustered unmediated ejaculations assailing the idea make it practically impossible to present or discuss, even to the point of starting up research in the subject. Yet when he captured an honors seminar at New York University and forced the students to expel all their preconceptions and prejudices, and to dig up fresh facts, the single equal tax was not only understood by the small group, but was also preferred by them, as one after another of the terms were defined, the data researched, a sample of people interrogated, and the idea drafted into the common and understandable form of a legislative bill.

On the proposition: "Venus is a young planet," first reactions tend to be equally obstreperous and incredulous. The attack builds up rapidly:

"The solar system is very old and stable, Venus included."

"The heat of Venus is an effect of its great cloud banks."

"A planet cannot be moved by any force without exploding."

"No force capable of moving a planet exists actively or potentially."

"Existing records reveal at least 4000 years of Venus observations."

"Bode 's law of planetary spacing forbids its moving from elsewhere or being elsewhere."

"Planets cannot move from ellipses to circles, and to move they must take up elliptical orbits for a time."

Against these, the quantavolutionary argument, as it was developed by Velikovsky and his friends, asserts:

"The arrangement of the solar system is only stable by our recent historical observations."

"Venus is an exceptional planet in its dense atmosphere and with its great heat of 900 degrees F."

"The heat of Venus is an interior heat moving upwards to the surface and into the clouds."

"The hot planet Jupiter could have contained Venus, expelled it by fission (nova), and given it its great heat."

"Venus rotates retrogradely, unlike the other planets."

"Venus is locked to the Earth (not to the 10^4 times larger Sun's tidal force) in two ways: each inferior conjunction (243.16 days) finds it presenting the same hemisphere to Earth; and its axis of rotation is perpendicular (within one degree) to the Earth's orbital plane (even while 3 degrees off its own orbital plane)."

"The postulation of historically active electrical forces allows a planet-sized body to move orbitally, axially, and rotationally without destruction, as an effect of the distribution of charges throughout the solar system and of the near passage of a large body."

"Sacred and secular legends from around the world allude to the deviant behavior of Venus in vicinity of Earth."

"The Venusian atmosphere, compared with the Earth's, contains 300 to 500 times more Argon-36, a gas thought to have been dissipated from the planets shortly after they were formed."

"Venus practically lacks a magnetic field, it being 10^{-4} of Earth's."

"Venus possesses a comet-like blowing away from the Sun that is much longer than the Earth's relative to their respective magnetosphere radii."

"The Venusian surface is heavily featured, despite its great eroding heat and eroding wind turbulence, but has no ocean basins."

"Fires seem to be burning on the surface of Venus, which may be caused by burning methane or hydrocarbons."

"Chemical composition of the clouds indicates no hydrocarbons (or components) yet, but the question is not

closed."

"Slight indications are present that Venus may be cooling off."

The idea of a double sun, the system of *Solaria Binaria*, as Deg named it, came with shocking suddenness. It was a monster that came leaping at him even before he had a name for it, and before he conceived of a dynamic for it. On April 28, 1963, shortly after becoming concerned with cosmogony, his journal reads:

Discussions with Velikovsky and Livio have not cleared up the phenomenon of the similar planes of the planets in solar revolution (maximum of 7% off) or even of why they rotate. Velikovsky and Stecchini are not very concerned, since Velikovsky's theories hold anyway. But I wonder whether the nebular hypothesis that has the sun throwing off the planets in an initial series of explosions is true and ask:

Could the Sun have cast off the planets at different times, or more importantly, could the planets be created on their common plane by the pull between the Sun and a second sun or planet revolving around and near (a twin). Then from time to time a planet would be released from one or the other...

While the people of his camp were arguing with conventional scientists over the origins of the heat of Venus and the chronology of Egypt, he took the time to wander about the cosmogonical fields and ponder what his friends might have known better than he, that is that changed motions of large celestial bodies signified not aberrations but somewhere back in time a basically different order.

The old order must have functioned on some basic principle, probably a simple principle. What could it have been? He knew next to nothing about formal astronomy or palaeontology or chemistry. What he was picking up might be scornfully and legitimately called static, a buzzing of voices, weak signals from many directions, from alleys and haunted houses of science, disreputable astrologies, occult references, stern and orgiastic religious cults and sects, ancient poetry, restless cemeteries of legends, the rage for science fiction, anomalies, contradictions

overlooked and brushed aside.

Probably if he had not experienced the hubbub of politics and warfare, where all is said and done and almost nothing is true, he would have avoided all of this, shut his eyes, clapped his hands over his ears. Even earlier, the presumptuous liberal education at the University of Chicago, which combined in a nettlesome but hardfast marriage with skeptical sociological pragmatism, had irrevocably attuned him to ideological quarrels.

Perhaps, too, had he not been pummeled by contradictory and obstreperous personalities among his friends and family, his neighborhood and his schools, he would have been quick to settle upon a regular line of thought. And, to be sure, the din was pierced by his immoderate ambition, which clamored louder than all else for solutions. He did not wait upon his betters.

He asked himself what he could contribute, and in line with his character it had to be "the bigger, the better." It had to avoid competition with superior heretics, not to mention superior conventional scholars, whenever there appeared a well-worn path -- solar chemistry, celestial mechanics, the fossil record, and so on. His head contained a large quantity of whispers and scratches telling him what to avoid and what might be chosen. He disagreed with most of Teilhard de Chardin's work, for instance but in reading *The Appearance of Man*, he caught a fine phrase that would describe his own mental set: "On the cosmic scale (as all modern Physics teaches us) only the fantastic has a chance of being true." Chardin followed this course by continuing as a Catholic priest; Deg followed it more specifically.

It was strange that an old, different order of the heavens did not suggest itself much earlier. However, going through the hundreds of titles that Earl Milton and he had compiled for the research on *Solaria Binaria*, Deg could find no statement that the solar system had been anything but a great sun which had cast off its planets in its early history. The history had been stretched greatly over the past century, from some millions of years to several billion years. A rotating hot ball of gases, interrupted by its own violence, perhaps, had operated as a centrifuge. An alternative theory had predicted a

passing body which by gravitational attraction had pulled off the planets and gone its own way.

Perhaps somewhere in the literature, as there always seems to be precedents, an obscure passage or writing would suggest that the Sun had a companion that had withered away, or, who knows, even Jupiter may have somewhere been called such a companion. If so, it remained hidden to contemporary discussion.

How did it happen that a few minds adventured in new directions? Let us extract some of the ideas that seem to have influenced the turning of thought.

Legends were gaining respect. After two centuries of general neglect, the idea of Giambattista Vico that behind legends stood a substantial truth began once more to pick up support. It is not without significance that Giorgio Tagliacozzo, an economist and employee of the Voice of America conceived a lush Tree of Knowledge whose fruit was of all the sciences and schools of philosophy and brought it to Deg publication in the 1950's. Then Tagliacozzo went on a one-man crusade to resurrect the figure of Vico and Deg became the recipient of a continuous flow of material, which, however irrelevant to *Solaria Binaria*, carried a message of the validity of ancient materials. There were others to come, the historians of science, Stecchini, de Santillana, von Dechend, and of course V.

But, going back, too, some twenty-five years, there were the anthropologists and sociologists whom Deg knew at Chicago, who respected the customs and ideas of so-called primitive peoples. By his simple and radical logic, it seemed always that if these people were so smart about the present, what they said about the past could not be more stupid than what the great religions said. And, if the two -- the "civilized" and "primitive" -- agreed that a great god blew a great wind over the Earth, burned it and flooded it, here might be the beginning of a historical truth.

Perhaps this was not all so easy. The anthropologists hardly went farther. Nor did the historians of religion: Mircea Eliade went a great distance to establish the obsession of peoples everywhere with their traumatic beginnings, and the beginnings generally

correlated; Eliade just failed to take the step, enveloped as he was in the uniformitarian song of science, to say that these earliest peoples spoke some universal truths.

Nor was it a simple matter to detour around Sigmund Freud, Carl Jung, and other psycho-historians. Freud had his own basis for reality, a primeval cultural event establishing the oedipal complex, guilt, obsession, recapitulation and, for the cosmogonies and catastrophes, nothing but uniformitarian principles. Jung had archetypes, primeval to be sure, cosmic also, but purely psychic in origin.

Velikovsky's was a different story. He generated a formidable sometimes caricatured obsession out of ancient catastrophes, and, further, had attached to the beliefs-cum-faith of mankind an original series of skies that carried two explosive bodies the Sun, Jupiter and Saturn, then later Venus that looked like a Sun in its approaches to Earth.

To my mind, there is but little doubt that if Velikovsky had been able to focus upon the general cosmological problem of the solar system, in the last decades of his life, he would have provided an ingenious explanation of the behavior of Saturn and Jupiter within a dynamic system. He understood that Jupiter's behavior was akin to a "dark star" it being "cold" (i.e. non-luminous) but with turbulent gases, and suggested that it sends out radio noises; his unpublished talk on the subject preceded by less than a year the actual announcement of the detection of the radio signals by Burke and Franklin (1955). In the same paper containing the bold surmise, he had been arguing on the solar system and, just before mentioning Jupiter's radio noises, he had used the analogy of a close binary or double star to illustrate the presence of electromagnetic effects between stars. He had also brought forward late studies demonstrating a correlation between the positions of the planets and electrical effect detected upon Earth.

He had argued in *Pensée* and in conversations that Saturn must have gone nova to eject immense waters some of which flooded the Earth during the Noachian Deluge. Then X-ray emissions were discovered to emanate from Saturn, a possible sign of recent nova.

On 4 November 1976, Milton was asking Deg's advice about mentioning this in a Foreword to *Recollections of a Fallen Sky*. "Ransom suggests that I not draw attention to this claim until Sagan *et al.* make some claims about Saturn's heat, magnetosphere, and X-ray emission. The point is relevant to Velikovsky's talks, but Ransom may be right, 'don't give them any points to avoid, let them commit themselves first. '"

In no case, however, did Velikovsky venture the concept of the solar system having a full binary history. In several passages here and there he broaches the idea that Jupiter and Saturn may have encountered the solar system and wreaked havoc from a distance, and he appears to have favored the idea that collisions between Jupiter and Saturn may have caused the Deluge and later on made Venus erupt from Jupiter. It was difficult to try to discuss such matters with him, and when, in his last years, Deg mentioned to him working upon a theory of *Solaria Binaria* he let the subject pass like a report on the local weather.

Meanwhile, most cosmic heretics who followed Velikovsky were devising schemes by which the major encounters among the planets occurred incidental to their clustering as satellites around the two giant planets, a kind of independent Olympian system interacting at a great distance from the Sun. They believed that the present solar system was occasioned by the forcible ejection of the planets into their present positions in consequence of disruptive encounters of Saturn and Jupiter, after which these large planets spaced out. What may exist in the way of specific scenarios for these occurrences rests still in private files unpublished. When Deg and then Deg and Milton came out with the model of *Solaria Binaria* in detail, they met with an initial refusal within V.'s circle to consider it; it was lamented that these two had "made up their minds;" the existence of Ouranos as a sky god was denied and other key assertions were denigrated.

The respect and patience of Ralph Juergens towards Velikovsky assumed proverbial proportions. Juergens devoted most of his professional life to establishing a fully electrical theory of the solar

system, including especially the explanation of solar radiance as the reflection of an accumulation and dissipation of electric charge from the galaxies. When Deg asked Velikovsky, more than once, whether he could accept Juergens' theory, he would reply with a definite negative. He adhered to internal thermo-nuclear fusion as the secret of the Sun's radiation. Because Deg respected Juergens, and then came upon Melvin Cook and then Bruce and Milton, he was never of this opinion. And now, looking backwards, one must wonder whether Velikovsky should have spent with Juergens the many hours that he spent instead, and writes a book about, with Einstein.

In introducing a posthumous paper of Juergens, a "pioneer in the study of electric stars," in 1982, Milton comments that Juergens perceived the astronomical bodies as inherently charged objects immersed in a universe which could be described as an electrified fabric.

"The Sun," writes Juergens, "is the anode end of a cathodeless discharge extending from the perimeter of the solar system." The solar photosphere is comparable to the "tufted anode glow" in an electric discharge tube. The Sun gathers electrons from galactic bodies and plasma, and sends out an ion current, the solar wind, to the galaxy.

Juergens dismissed the thermonuclear explanation of the Sun's heat in favor of a galaxy-solar electric exchange. The thermonuclear theory, recently developed, sought to explain the Sun's properties of luminosity, temperature and stability by its essential chemical composition, mass and size, assuming that the Sun and its behavior are effects of the conditions in galactic space, not in its interior. So, much of his time went into seeking ways of detecting and measuring the suspected inflow, capable of reflecting a continuous output of electrical power amounting to 4×10^{26} watts, or 6.5×10^7 watts per square meter; this, it happens, registers 0.137 watts per square centimeter at the Earth's position in space. The searched-for input must amount to 4×10^{26} watts as well.

Now whereas scientists have for a long time accepted the invisible source of power known as gravitation, they have largely ignored

and disdained the possibility of an invisible source known as electrical discharge in a gas. "Electric discharge is a known and observable phenomenon, yet we might live immersed in a cosmic discharge and know nothing its existence."

V.A. Bailey of Australia published in *Nature* (1961) his calculations, based on the data of Pioneer space probes, that the Sun must possess a net negative charge with the potential of the order of 10^{19} volts. Bailey visited Princeton to meet V. and there Juergens and Deg became acquainted with him as well.

V. was always excited by indications of unforeseen electrical forces playing about the universe. Still he never accepted Juergens' theory, possibly, as he told Deg, because the thermonuclear theory seemed solid to him, and it is indeed regarded as fact by physicists, astronomers, science publicists, and of course the educated public. Since V. never read or discussed Deg's theory of *Solaria Binaria*, which accepted Juergens' theory and satisfied so many requirements of V.'s own reading of natural and astronomical history, it can be surmised that Juergens' theory was not working for him, V., and should be tolerated because of the usefulness of Juergens' ideas and work, whether as an ever-respectful historian of the V. Affair or as indefatigable discoverer of electrical forces and effects on Earth, Moon, Mars, Venus, and in planetary encounters. Long after Juergens pulled up stakes from the Princeton area to find a new life in Flagstaff, Arizona, partly to be "his own man," V. tried to coax him into returning to collaborate on one or another of his books.

Juergens persisted in developing his theory, while repeatedly coming to V.'s aid in the astrophysical exchanges in which V. engaged. Never was the issue of the origins and prior shape of the solar system introduced to systematic discussion. V. generally reacted negatively, even harshly, when material which he objected to or deemed irrelevant sought its way into the magazine *Pensée*. Ultimately the magazine was discontinued in part because of a disagreement between V. and the Talbott brothers on the question of broadening the magazine's scope. However, he behaved gently towards Juergens' material, and Juergens' ideas did receive their initial publication in *Pensée* where Deg could study them, along

with the rebuttal of them by Princeton Physicist Martin Kruskal, to learn something about the Sun. The date was 1972. Juergens had already moved from Hightstown, New Jersey, to Flagstaff, Arizona.

Deg was by now knocking the planets around like billiard balls, looking for the right pockets. He came to realize in the legendary succession of Greek gods, which might be afforded backup from divine successions in other parts of the world, a possible sequence of real cosmic events. His basic god became Ouranos (Uranus), generally ignored by V. and the other heretics. And, reading in the century-old esoteric papers of Isaac Vail, and elsewhere, he found an original divine Heaven, which eventually produced a Sun-like figure which was still called by the name of Heaven. Thence the succession, of events took shape: Ouranos-Heaven, Ouranos- Sun, *Kronos* (Saturn) Sun, Zeus (Jupiter) Sun, and the antics of the Olympian family of planets -- Earth, Ares (Mars), Hermes (Mercury), Apollo, Poseidon (Neptune), Uranus-Minor and Venus. Each and every one of these had been a principal in catastrophes upon Earth, and victim of catastrophes itself.

Deg thought that these might be interacting meaningfully and in a series or succession, ending at the beginning of the present historical period, when Greek philosophy was born, which could be regarded as the Solarian Age. From that time onwards, the Sun (and Moon) seem to have been the dominating bodies of the sky and no intruder -- planetary, cometary, or meteoroidal -- appears to have played a major role in the sight of mankind, excepting always in the beliefs of astrologers that carry down to us their fossil memories.

Deg speculated as follows: there were three legendary Fathers -- Ouranos, Kronos, and Zeus. Hence only these three major bodies had to be accounted for as the basis of the earlier solar system. But, since Zeus was the son of Kronos, and Kronos was the Son of Ouranos, only one body had to be accounted for, that is, Ouranos. Now, since Ouranos was originally a thick cloud enveloping the Earth when mankind's legends began and was the first subject of creation legends, this canopy-sky must have been an atmosphere thicker than any in historical experience, thicker even than those provoked by known catastrophes such as the temporary darknesses of Exodus and other legendary or pre-historic episodes and the

recent volcanic explosion of Krakatoa. But finally Ouranos emerged and exposed himself, enveloped in clouds. To some, he was the Cosmic Egg.

The birth of Kronos and his revolt against his father was readily pictured as successive explosions of a super-Uranus and the establishment of the new body, Kronos. The birth of Zeus out of Saturn was analogous. The planetary children of Zeus, of different mothers, remained under his nudging regime until the settled skies eroded his rule and, indeed, all planetary rulership, except in myth and astrology.

Deg imagined that electricity might do what seemed impossible for gravitation, although he clung to both powers until Earl Milton persuaded him that all the problems could be solved without gravitation, letting Deg cling only to the inertia which he had cherished all along as the vital element in "gravitational" behavior. In 1976, he was in touch with Milton, who was coaxing a key paper from V. for his book, *Recollections of a Fallen Sky*. He was also in correspondence with Juergens, and he told both of them what he was up to in *Chaos and Creation*. Both were sympathetic.

On April 22, 1976 he wrote to Milton a memorandum of "Alternate scenarios for the shift of planets, including Earth, from a proposed binary system to the unitary solar system." He conceived of the planetary system as strung out between Sun and Super-Uranus and rotating around the common electrical axis while the axis, carrying the whole set, wheeled in revolution around the Sun. He is becoming enthusiastic:

I am beginning to feel my oats, Earl. I can visualize as neat and elegant a model as anyone might wish, replete with formulas. What great blooper have I made, cher colleague? Are you still holding to your generous offer to collaborate? Is scenario II our preferred kick-off? We are having a thunderstorm with lightning.
Perhaps Jupiter knows!

Further exchanges took place: then came a week's discussions in New York in 1977, ten days together in Washington, D.C. in early 1978, the same in Princeton in early Fall of 1978, the months on the

lonely promontory at Stylida, Naxos, by the Aegean Sea in the Spring of 1980, where most of *Solaria Binaria* was written in its final form. On May 26 1980, Deg notes in his journal 'Finished 1st draft of chaps II and III of *Solaria Binaria* with Earl Milton 1230 hours.' He tells how they would discuss heatedly from early morning until early afternoon, sometimes arguing stridently, their voices echoing over the rocks of Stylida, putting their only competitors, the crows and seagulls, to flight. Afternoons and evenings they would write in their separate rooms. In the early summer of 1981 they met again in Princeton and New York, and again in late 1981, spending a strenuous ten days at Edward de Grazia's beach house at Rehoboth, Delaware to complete a manuscript of the full work. Leroy Ellenberger, not far away, called repeatedly but was not invited to come, for a visitor would have disrupted the relentless pace through the manuscript. (This incident may have triggered Leroy's animosity, who before had been deferential and complaisant.) Pages of notes and reprints lay in piles about the large room, on the floor, the chairs, the tables. Upstairs Ami worked quietly at her novel. Outside the low sun beat weakly upon the great beach and roaring waves. They drove to Annapolis to visit St. John's College where Bill Mullen and Joe de Grazia were now teaching. Deg and Ami dropped Milton off at the Washington Airport amidst a howling blizzard for his long flight back to Alberta.

The notes and manuscripts had traversed the continent and the Atlantic Ocean several times, punctuated by messages and phone calls, and by "Did you receive....?" letters, with chapters and cassettes chasing the men like heat-homing missiles. By the Spring of 1982 the book was completed and stood in line for publication.

So ambitious a work should have been created under ideal conditions, with at least a solid year of side-by-side collaboration and next to a giant library. If they had waited for this setting, the book would never have been written. Milton had been troubled by asthma most of his life. He was placed under great pressure in the writing of *Solaria Binaria*. The discussions were heated, the environment often strange, yet he was less troubled by poor health when they were exerting themselves upon their creation to the point of exhaustion.

Milton worked steadily over the years to make a respected place for V. and quantavolution in Canadian thought. He was a popular teacher and, at some risks to his career, he systematically introduced the new ideas into his courses. Canadian higher education employs outside evaluators whose word goes far on matters of curriculum and promotion. He was able successfully to fight off professional criticism of his innovations in teaching and writing, and ultimately achieved an influential role as spokesman for quantavolution.

He was a principal agent in persuading his faculty to offer an honorary doctorate to V., the only one ever given him, and within a decade he was once more agitating at the University for the same honor on behalf of Deg. He held meetings, journeyed to contact potential supporters, wrote reviews, spoke on the radio, and was an organizer of the Canadian Society for Interdisciplinary Studies. He was the principal Canadian representative in England and the United States. Only Irving Wolfe, at the University of Montreal, and Dwardu Cardona, living in Vancouver, approached him in effectiveness and productivity. Two papers of Milton, written at the turn of the decade, one erasing gravitation as a necessary concept in celestial mechanics, the second dealing with Earth-Venus close transactions, are among the classic expositions of astronomical quantavolution.

Ralph Juergens was struck down by a heart attack in 1979, a few weeks after Stecchini expired, and a few weeks before V. died. He was gearing up to participate in the writing of *Solaria Binaria*. I doubt that the final manuscript would have been much changed if Juergens had taken an active hand. Milton thinks not. He had gone over the general theory with him, and Juergens had received in 1976 and 1977 Deg's skeleton of the book and chapters from *Chaos and Creation*. In Juergens' home, Deg's accumulated manuscripts were used as a raised seating facility for Milton's little son Davin, when they were visiting.

Afterwards Milton examined Juergens' rigorously organized archive of materials and manuscripts; *Solaria Binaria* would have been improved, but no contradiction would have ensued, given

Juergens' outlook. Deg and Milton dedicated the work to Juergens, for his electromagnetic theory was deeply implicated in it. To the dedication the ancient fragment 64 of Heraclitus was appended: "Lighting steers the universe." Deg wrote a poem to his memory and sent it to his widow. It was printed in *The Burning of Troy*, along with an *oratorio* to Stecchini and a memorial to V.

On December 8, 1980, Deg writes to Milton:

My *Chaos and Creation* is due for March 1 publication, already outdated in certain respects by what you and I are doing in *Solaria Binaria*. It makes me uncomfortable to know this, but then it helps to recall that Galileo had already committed worse "crimes" in science and philosophy by the time he was brought to trial for heliocentrism. It will bring pleasure to admit errors in *Chaos and Creation* if the truth is measured by what appears in *Solaria Binaria*.

I don't think that we need to fear competent appraisal and criticism. Apathy is a more real problem. Physicists and astronomers are ordinarily paid to go about their work without making waves. They are not philosophers, or even interested in philosophy. Nor are they competent in more than their specialized areas; it doesn't pay them to be so. That is why remarks like, "It isn't physics," or "If that's astronomy, then I'm King Tut," often carry weight. Phrases like these are the shock troops of reaction in science. If they fail, then somebody -- hopefully someone else -- is awaited, to bring up the heavy artillery. But then maybe the heavy artillery is not there; maybe it is rusted from disease; or maybe there is mutiny among the cannoneers. We shall see.

In 1979 he was beginning a friendship with geology Professor Frank Dacheille at Pennsylvania State University to whom he sent *Chaos and Creation*, and who engaged himself in the new astrophysics. Dacheille wrote to Deg:

...In the earlier letter I indicated that I have browsed through your mss; since then I have read it completely through, but not with hypercritical attention. I expect to read it again, but I doubt this will be done before we leave for Africa. Frankly, I am quite shaken and taken by the intensive physical processes described, generally fitting well the human recordings of the time. However, I still feel that I would have to understand the

processes analytically before I could accept them without reservation. Shaken, too, I was by the views that the Moon was not always up there; also Venus. So, I went back to Velikovsky, am now reading *Worlds in Collision* -- really the first time. My first contact with V. was in a magazine article about 1950, when I browsed through *Worlds in Collision*, but was turned away by what I felt was his cavalier treatment of I. Donnelly, and the too easy flip-flopping of planets. Kelly and I were already working on *Target: Earth* -- that is, I was going over his original manuscript, started by him about 1947 or so. I was deeply involved trying to quantify the mechanics of the collision process, including axis change, orbit changes, figure of rotation, inertial response of water, slippage of shells, atmosphere...My contributions were just intended as suggestions to Kelly, but he asked me to come aboard as co-author. I think you can identify my work by the diagrams, calculations, chemistry, white bills, dry points, epilogue. In all this time, while I was, or *we* were aware of V., his work did not contribute to ours in any way. I did feel however that his work strongly supported Kelly's historical presentation, that is, the ancient records were, in fact, describing horrendous events touched off by what Kelly called Cosmic Collisions. As I said before, I quantified the collisions, based on impact processes, and found that sub-planetary, or small asteroid bodies would be necessary agents. I did not consider electric fields between bodies at a distance. To me the very clear evidence of impacts on the moon provided the simplest, continuous, mechanically sufficient process or mechanism -- collisions involving objects up to 600 miles in diameter. Combining the size-frequency distribution of collisions with the erratic records in the geologic and evolutionary columns, I found support for the impact processes; it was not necessary to involve *planetary* approaches.

However, after reading your book, and going into V., I think that occasional close passages of large (but not quite planetary) bodies will have left their marks on the Earth. So, it appears to me now, massive collisions by the hundreds of thousands have forged the earth in its ca 4 1/2 BY history; by the tens or hundreds close passes by generally larger bodies will also have left their marks. As you know, Kelly has been suggesting close passes as a process operative on the geology of Mars, perhaps even Venus. It seems that Bob Stephanos has a fly-by process. Beaumont too. And, of course, Donnelly. It was Donnelly's work (Ragnarok, Atlantis) that got me thinking in this area, plus my activity as an amateur astronomer....thinking about

electrical charging of the "spheres." I do not know enough EM theory at this time to quantify the mutual interactions of two oppositely or identically charged planetary bodies. Then there is the problem of conservation of momentum and the scale of energies involved. The energy in the earth's magnetic field is many, many orders of magnitude less than that of its rotation and orbiting. How a flip-flop can be affected by magnetic or electric coupling I cannot understand at this time.

Well, you can see that I am thinking along with you. The Cosmic Collision, in all its variants, must be of utmost importance in the history of the earth and life. Last winter term I introduced the subject to my students in the Geology of the Solar System. The coming winter term I intend to intensify my presentation...

On August 3, Deg replied from Naxos:

"Dear Frank,

Thanks for the excerpts and clippings. Io is full of surprises. Purely sulphur volcanoes, someone writes now. But note the pulsing electric arc between Jupiter and Io. It compares with my postulated arc between the Sun and its binary partner, Super-uranus.

Your work on collision-electricity interests me. Also sphere-charging, and passby-electricity. Regarding the last, you should certainly know Ralph Juergens. Eric Crew has done some thinking, and an article on the funneling effect in meteoroid and lightning strikes. I hope to get a chance to read your full articles when they are available. I can give you the Juergens and Crew stuff when I return. Juergens, you know, would say, in reply to your query as to how a million craters could strike the moon in a few thousand years, that a great many of these are the marks of lightning bolts, not of meteoroid falls. Further I imagine that after the major passbys, and a couple of collisions ("Apollo") and fissions (novas) as conceived in *Chaos and Creation*, the space would be jammed with a great many millions of pieces of debris. Ovenden sees the asteroid belt as remnants of an exploded planet many times the size of Earth, not too many millions of years ago. I call it Apollo, set it in human times, and can readily imagine the debris of Apollo and its Destroyer. We have a big gap to close between our solar system time scales; if you grant the conceivability of what I say in my chapter on the

subject, I'd like very much to discuss with you the seemingly impossible obstacles to it. I guess you won't see Olduvai George; there's a fine place (the African Rift) to test the theories of chronology given the hominid and hominid finds on various levels..."

It is depressing to many to think that the planets may have once undergone displacement; it is much more depressing to think that they may have changed motions recently. Of course we must admit that displacements must have occurred to bring the planets into existence, and to place them where they are now. But very few astronomers and philosophers have let the planets shift thereafter, and practically none allowed this within the time span allotted to mankind.

Malcolm Lowery, in a letter to the *London Times Literary Supplement* August 27, 1976, named several latter-day movers.

In 1960 W.H. MacCrea -- then president of the Royal Astronomical Society -- calculated that no planet could have formed inside the orbit of Jupiter. In 1965 T. Gold concluded that the planet Mercury could not have been in its present orbit for more than 400,000 years, as it is still rotating with respect to the sun. J.G. Hill's 1969 model indicated that Jupiter and Saturn were originally the outermost planets to form, and that Uranus, Neptune and Pluto were displaced into their present orbits by planetary encounters.

Robert Bass in 1974 exposed the prevailing common misunderstanding of the mathematics describing planetary stability, even when based upon present recorded behaviors, such that planetary orbits could not be proven stable for more than a few centuries or millennia. W.M. Smart, wrote Bass, "demonstrated unquestionably that the interval of assured reliability of the La Place-Lagrange perturbation equations is at most some interval 'small' relative to 300 years; Prof. W.M. Smart's exact words are 'one or two centuries.'"

Bass went on to apply to astronomers the kind of pragmatic critique that impresses experts in propaganda analysis: "...Whenever these

authoritative statements about time intervals of validity have been made, they are without exception accompanied by words like 'supposed,' 'appeared,' 'hope,' 'seems' 'might,' and 'think,' revealing clearly that the writer was relying on his personal intuition rather than quantitative evidence."

Bass repeated his findings at a Glasgow (Scotland) conference held by the S.I.S. in April 1978, where there appeared to speak also Astronomy Professor A.E. Roy. Roy agreed with Bass, saying that "even under Newton's law of gravitation, we have not changed by more than 1 or 2 percent over a period of more than, say, 50,000 years." This figure allows humanly witnessed perturbations, but is not enough for the wilder of the cosmic heretics, who want to bring changing planetary orbits within memory of myth-making man and even historical mankind.

Thus it occurred that when Melvin Cook, Ralph Juergens, Earl Milton, Eric Crew, Deg and others -- and V. in principle -- wanted to move the planets more, and recently, they turned to electromagnetics, and Bass once more, now in 1978, applauded their heretical stance, affirming that "if planets approached closely, there *would* be electrostatic and electromagnetic interactions not predicted on the basis of orthodox theory."

This was not enough. The solar system had to operate as a electromagnetic system, and, though Bass produced an awareness of the sources of such theory, in Juergens and Cook, it was Milton who, with Deg cheering from the sidelines, took the fatal leap onto the plane of non-gravitational fully electromagnetic operation of the solar system.

In a paper circulated in 1979, called " $10^{-36} = 0$ " to connote the vastly superior forces at the disposal of electricity by contrast with gravitation, Milton wrote that the phenomenon of gravitation implies "an interaction of slightly unequal strong electrical repulsions between distantly separable objects (or centers) that yield a weak net attraction." Thus masses vary when determined gravitationally insofar as they represent an electrical transaction between two bodies of unequal negative charges. In close encounter masses undergo polarization and transact strongly as

dipolar bodies. Rapid and forceful exchange of charge then occurs which can modify motions significantly and suddenly. Hence the absolute level of electric charge on a body is indeterminate, as is, for example, absolute motion under relativity theory.

Deg's image of the whole solar system as consisting of bodies lined up between Super-Uranus and Sun within a tube of gases and rotating with the gases around a discharging electrical current, with the whole system falling apart recently into its present configuration, proved to be just the mechanism to display a non-gravitational system, and Deg, who had never quite understood gravitational mechanics in the first place was happy to observe his model work nicely within the systems of permissions and restraints belonging to electromagnetic theory. He was doubly pleased because he had been so fond of Juergens and found Milton so congenial: one should not dismiss compatibility in scientific achievement; any scientific (or social group) manager will be glad to elaborate the proposition: compatibility is as important as computability. An eloquent instance of this proposition suffuses James Watson's autobiographical account of the construction of the DNA molecule in his book, *The Double Helix* (1968).

V. was the Great Hostess, in the earlier time, of this whole business; he took no active part at all in it, and the heretics dutifully thanked him at every opportunity in their writings. It will be remembered that Juergens left the Princeton area in flight from the domineering proximity of V. Milton was too far away to be captured intellectually, though he was continually active in defending V.'s views. What Deg received from V. in the theory of *Solaria Binaria* was nil; all he got from V. was the useful dogma that electricity had been neglected by scientists and was an essential factor in cosmic encounters. Whether V. discussed much of importance with Einstein will not be known until the manuscript devoted to this subject is made available. My hunch is that Einstein retarded V.'s growth in electromagnetics just as V. retarded the growth of some heretics in this regard.

V. made no attempt to relate his work to that of Charles E.R. Bruce of the Electrical Research Association of England, whose seminal work of 1944 on electrical discharges in astrophysics had been the

basis for correspondence initiated by Juergens in 1965, and whose work was introduced by Juergens in *Pensée* in 1973. Bruce was a cosmic heretic whose ideas made little or no impression upon British astronomy. They were carried into the British quantavolutionary circle by Eric Crew when it was organized. To this day his one hundred and more articles and notes have not been published in assembled form. Milton caught on to Bruce in the early seventies, Deg after his meeting with Crew in London in 1976.

Bruce observed the first identity between the velocity of propagation of a solar prominence and an electrical discharge in 1941, when at a lecture he heard of Evershed's photograph of a solar prominence that had reached a height of a million miles in an hour. He writes, "I thought, 'If that isn't about 3×10^7 cm sec⁻¹, I'll eat my hat.' It was, as a little mental arithmetic, confirmed on an envelope when the lights went up, established -- and I was in business as an astrophysicist." He thereupon published privately *A New Approach in Astrophysics and Cosmogony*, copies of which several cosmic heretics came ultimately to possess.

Galaxies were seen by him to be structurally determined as electrical fields. Magnetic fields spring up around cosmic flares and bolts. In cosmic discharges, matter aggregates along the discharge channel, and in this process of electrical breakdown "one can forget about the force of gravitation, as every arc welder knows." This discovery Bruce attributed to Bellaschi of the American Westinghouse Company in 1937. Jets and balls of hot gases are formed in the process. Bruce also applied the notion of pinched-off discharges under extreme pressures to the extinction of novas. Juergens and Milton pushed Bruce's electrical interactions between stars and atmospheres into stellar interiors, the greatest step in obviating the need for gravitational theory.

V. lacked the capacity to give and take; he would disrupt any on-going thought processes to call all hands to shoo the chickens out of his backyard. Those heretics, like Rose and Vaughan, who opted to exercise their intellects in his garden, found themselves becoming over-specialized in certain crops, interpreting Venus tablets and calculating conceivable orbits under conventional restraints. This is only to say that such heretics became unfortunately limited despite

their eminent suitability for larger tasks; they were also diligently occupied, as was the solaria binaria trio, in developing the larger network of heretics and playing firemen for V.'s fires (some of which were arson).

The progress of quantavolution in the astrosphere required an electrical model. Fortunately it could profit from a considerable advance along the whole front of electromagnetic studies which was occurring in conventional science, as well as from the work of the heretics themselves. But one ought not forget that the theory of quantavolution in the atmosphere was sustained too by heavy inputs from faraway field: myth analysis, paleontology, and critical geochronology.

Deg's assurances that the fossil voices of myth and legend were speaking truths of the skies kept the theory from flying off to join the conventional dogma that change could only happen hundreds of millions of year ago. They also blocked the hopeful theory that comets and meteors could take the place of the planets.

In paleontology we have this remarkable logical position, perhaps exposed for the first time by Professor Roy in explaining why astronomers should prefer a longer rather than a shorter period of celestial stability:

Most celestial mechanics -- orthodox *and* informed -- would say that we *suspect* (it's probably no more than a hunch) that the solar system is stable over hundreds if not thousands of millions of years, but we cannot prove it by the methods of celestial mechanics that are available to us today. We have to go to geophysical, astrophysical and selenological evidence -- and there, of course, we are again on ground which has been disputed by those who advocate the very short time scale. The fossil record would appear to have been laid down in the rocks over the past two thousand million years, and in those fossils we have very complicated animals. If the orbit of the Earth had changed drastically in that time, then conditions on the orbit of the Earth would, it seems to me, have been such that those creatures could not have existed. In addition, one could say that, even if the orbit of the Earth had not changed in that time, but the Sun's output of radiation had changed dramatically, then again the fossil record as we know it could appear to be 4

1/2 thousand million years; similar methods appear to make the oldest lunar samples of that order of magnitude in age. Theories of the energy output of the Sun make it appear, from a consideration of the helium/hydrogen ratio, that the Sun has been operating with much the same output as it does today for something like five thousand million years. And so on..

What Roy is saying here is that, for no other reason, a long term stability of the solar system is acceptable because it has taken so long, according to the fossil record, to evolve life and its peculiar, complex structures. Further the rocks are datable by radiochronometry and the Sun is datable by its self-burnup rate. This is nice: here we have the queen of sciences, to which the other sciences had looked for *their* assurance, abandoning its throne and asking for refuge among the fossils of the rocks and the furnaces of the Sun.

Effectively, however, the quantavolutionists had spotted this cross disciplinary mutual rescue society, and had begun to launch assaults against the positions of the other disciplines as well. Juergens had fully disestablished the thermonuclear theory of the Sun, so far as some heretics were concerned, and substituted (with Cook) a galactic electric- collecting model.

So far as the fossil record is concerned, Bass in 1978 accords Cook the honor of having achieved the main victory over radiochronometry. (The old catastrophists, such as Price and V., had done the job on conventional stratigraphy and erosional gradualism in geology.)

In a footnote that should be a placard Bass writes:

... If I believed those long-term radioactive dates in the fossil record and elsewhere, I probably would also believe that the Earth has not changed its position for thousands of millions of years. However, in another book, *Prehistory and Earth Models* (London, Marx Parrish, 1966), Dr. Cook has had the audacity and temerity to take on the entire historical, geological and geophysical establishments, and has reviewed in great depth and detail every radioactive dating method, short-term and long-term. After several years making up my mind, I have come to the conclusion that Melvin Cook is right and has

established that there are enormous and inescapable fallacies in the uranium, thorium and lead dating methods; and I don't think it can be maintained that the surface features of the Earth have been in their present form for more than 30,000 years.

Deg had supported Juergens in several works, and had relied heavily upon Cook in attacking the full range of dating tests offered in support of great ages of time. I have not yet introduced the several other contributors to the demolition of time measures. They appeared in the pages of *Pensée*, the *Creation Research Society Quarterly* and the *SISR* for the most part. The attack requires hundreds, not a dozen, writers, however.

But still there must be a elite, leaders of the republic of science, like Robert Bass. Everyone got a lift in spirits with his appearance upon the scene, a stocky dark man, bespectacled, a convert to Mormonism it appeared, with a weakness for women which, Deg reflected, was in keeping with history and not incompatible with his experiences of Mormon friends who came out of the West to the University of Chicago in the 1930's. Bass was associated with Brigham Young University, where, paradoxically, catastrophists were unwelcome in the sciences; a story goes that Bass forgot to sign and return his contract, lost his tenure, and, in order to retrieve it, was asked to agree to submit to pre-censorship of his publications, which he refused. Bass was covered with the medals of scholarships and degrees and when he showed up, it was like a troop pinned down by continuous fire greeting a marksman with just the right gun.

Bass took aim at the brain center of the opposition, the reliability of planetary motions, and fired. The shot was on target. Blasted was the astrophysics of orderliness. His troops cheered. The opposing line continued firm; hardly a surrender or desertion. It seemed that the facing army lacked a brain center. It operated just as well by rote.

CHAPTER ELEVEN

CLOCKWORK

Deg 's Journal, Naxos, July 3, 1973

The animation of the night skies is both poetic and heuristic. Each meaning enhances the other and creates a third set of meanings that are beliefs. These beliefs join the stream of myth, color, and shape it, change its direction somewhat, make its fundamentals more difficult to understand. Cosmopoeia is the imagined form of stars, a guide for students and navigators by sea and land, the astrologer 's subject of story, the marking of the passage of bodies and the occasion for anniversaries of related events, be they births, deaths, or disasters. All of these functions are important to humanity. But that they flourish should not be pretext for diminishing or denying the occurrence and greater importance of erratic for diminishing or denying the occurrence and greater importance of erratic and special heavenly changes.

Similarly, the world as we see it in the "normal" processes of constancy and incremental change is a true and real world. The tides flow, the sea suddenly beats the shore, the rains wash down soil and the winds abrade rocks. This everyday vision lulls us into somnolence about natural forces, or when aroused, to a discrete excitement about tornados, volcanoes and earthquakes. Like the animation of the skies, the ordinary experience of nature is a reality that is also a screen and a censor, concealing and prohibiting the colossal, historical and potential behavior of nature.

As it is with the skies and earth, so it is with life. The recent fixation of species, based ultimately upon an operational definition involving interreproducibility, gives a truth that must always have been real: gradual changes occur; species can develop in isolation, by occasional mutation. But all the time that biology can beg, borrow, or steal is not nearly enough to present us with the fantastically organized and behaving conglomeration of animals and plants of 1973. The validity of

received evolutionary theory must become minor, while the heavier reality of catastrophic change and origin of species by potentiation comes forward.

It was inevitable that Deg should end up in defiance of billions of years of time. He could hardly lie on a beach unless he was exhausted from swimming and diving. He knew and disliked the stereotype of the American as restless and impatient, so he cultivated various devices and appearances that would let him seem to be casual and unconcerned with waiting upon the world. Since he was raised without the time-consuming liturgies of religion, religious routines were not a common means for stopping his time or feeling it. Sports, smoking, drinking, eating time. More than all of this, he played games against time. He wanted quick results in everything he did; but the world is not constructed to provide results, much less to provide them quickly.

The same urge to quick results inclines one toward intellectualism, because so much can be solved in the mind and the world of the imagination can be rich and malleable; fat gobs of time can be reduced to frizzled specks, and one can leap over far spaces and epochs. However, intellectualism is also opposed to both physiological and mental time-control in that it forces one to be physically inactive over long stretches of time; research and writing are termite mounds of time and a single footnote, a single bad line, can drive one to despair.

Sometimes I think that Deg was one of Alfred Adler's pure compensatory characters, who set himself very often to do precisely what he was unfit to do because of his unfitness. If under such circumstances he was not destroyed by the contradiction, it was because he often escaped into the activities already noted but also into sex, travel, brief adventures, commitments to thing extraneous. Most of all, and too important to call an escape, was his taking on two or more large tasks at the same time, so that while to the outside world he appeared to be proceeding carefully along one line, at a measured pace, he was in fact speeding along other lines and then doubling back to the first line of engagement.

Paradoxically, the intellectual who is so fretful of time's arrow

hastens but to sit and stare upon dead written pages, to pitch his nervous system and organs upon his several moving digits, gaze at the stars, watch the rats run, listen, observe, and discuss only that world that his mind will accept for consideration -- all of this consuming such enormous amounts of time that those who in turn observe the intellectual cannot be blamed for thinking him mad for his dissociation and hatred of reality, his obsession, his wrestling with details, his fear and guarding of his own thoughts, his ruthless hunting down of words and meanings, amounting in the end to the squandering of the very object of his anxiety, time itself, time in the thousands of hours of which every minute, he insists, counts dear, and if this lunacy is not sufficiently oxymoronic, the time-saving time-waster can dedicate himself to time-studies.

Perhaps one-fourth of all Deg 's work on quantavolution over the year dealt with time. Perhaps a quarter of the three thousand pages that he wrote were concerned with or governed by calculations of time. Before he had entered the field he had been possessed by problems of time and had written but not finished what was supposed to be a lengthy philosophical and psychological poem on the subject. By virtue of the tricks I have already alluded to, he would escape the psychiatrist 's verdict of obsession, but in fact he was obsessed and his impatient and striving character often led to pitched battles against time; it was the most uncontrollable element in life.

He beat time as a child by being precocious, stripping off three years of schooling, and he became the youngest member of his graduating class at the University. But then time reacted smartly at war and he felt the full poignant irony of "Hurry up and wait" the life of the soldier. He nosed his jeep into many destroyed towns where clocks were stopped; hanging crazily, sober and still, or startled faces starting from the rubble -- they were all wrong. Are all clocks wrong? Madness about time was a disease of the poets, literati and humanists; turn to scientists, and 99 out of 100 are perfectly satisfied that they are measuring an absolute, an ever-so-old process; they are like the bureaucrat who is content to keep the entrepreneur waiting, because his check comes in regularly no matter what, while for the businessman time is money. For these scientists, there was something called the relativity of time, which

was reserved for their Sunday outings.

All of this joins in with Deg's anti-authoritarianism and republicanism (which goes back to sibling rivalry) and gave him his ideological stance confronting time . If authorities would say time was long, well then he would be pleased to discover time to be short, and thus more containable and controllable. There was a contradiction here, however, but it can be explained away. Deg had always been a darwinian, but might this not have been because Darwin was anti-authoritarian, anti-theologian, too, while trying to be nice to the traditional believers? Deg was exactly like this, against the scriptures as authority, against church authority as such, but then respectful and even loving towards the many "nice" and "gentle" believers he met. How could he join the theologians, the short-time creationists? Well, he didn ' t really. He found them to be the most active critics of macrochronism. They were experienced microchronists, who knew the history of the defeat of microchronism well because it was their history.

The problems of time came in two batches. First there was the historical batch, epitomized in V.'s *Ages in Chaos*. Second, there was the geological batch, which could also be epitomized in V.'s *Earth in Upheaval*. Let us see what V. did with time in both regards.

V. aligned and connected Jewish and Egyptian history which had hitherto gone along on separate tracks. The alignment settled upon the Exodus at about 1450 B.C., the Biblical date tied it into the end of the 13th Dynasty of the Middle Kingdom of Egypt with Hyksos invaders as the Amalekite enemies of the Biblical Hebrews. He begins the splendid 18th Dynasty of Egypt at the time of Saul and David. King Solomon he places alongside Queen Hatshepsut of Egypt, and has her, as Queen of Sheba, visiting his court. And so on.

The reconstruction attempted in his volumes on later time, as I have already indicated, fell victim to the scholars of the "British Connection."

Dropping by 500 years the accepted chronology of Egypt after the

Exodus, and holding the Exodus at -1450 meant that all dates elsewhere, whether of the Near East, Greece, or finally Italy, which had been set by coordination with Egyptian artefacts and occurrences, required resetting by 500 years as well. In Greece, a gap which had been closed only by creating a barbaric "five hundred years of the Dark Ages," was promptly nominated for elimination. A grateful rush of scholars to profit from the new chronology did not occur; the Greek scholars were frozen to their Positions until the Egyptologists (all 30 of them) would admit the loss of the five centuries. Then they would follow suit. Similar scientific lags continued in the other ages affected by V.'s reconstruction of Egyptian chronology.

When did the mistaken chronology begin? V. traced the major error to Manetho of the third century, B.C. as reported and adopted later by scholars. Manetho was eager to prove to the Greeks and Asians the superior antiquity of Egyptian civilization. Berosus followed suit, exaggerating for his Assyro-Babylonian country by tens of thousands of years. Eratosthenes, soon afterwards, took up the cudgels for his Greek compatriots and moved Greek dates backwards by approximately the length of the "Dark Age." The motive of ethnocentrism thus played a large part in the beginnings of modern chronology, as it did in V.'s stupendous reconstruction itself. But it was not at all clear that the ancient chronographers following Manetho were wrong, for their errors were covered up by a heavy burden of refinements and rationalizations up to the present time. If V. had written nothing else in his life he would have deserved the highest accolades for his essay on "Astronomy and Chronology."

Soon after his first attack upon Egyptian chronology was published, V. sent a copy to Etienne Drioton, Director General of the Service for Antiquities of Egypt and received shortly one of the most nearly perfect replies an author could wish for, and, for that reason alone, as a model for my readers, I reprint it here. (My translation is from the French original.)

Cairo, May 29, 1952

Dear Doctor,

You were kind to have had me sent your beautiful book, *Ages in Chaos*, which I received this morning, and which I have read nearly in entirety, so exciting and interesting is it.

You have certainly jostled -- and with what vigor! -- many historical tenets of ours which we regarded as firmly established. But you do it with a total absence of prejudice and with an impartial and complete documentation, which is most sympathetic. Your conclusions might be argued at every step: whether they are allowed or not, they will have posed anew the problems and compelled a fundamental discussion of them in the light of your new hypotheses. Your beautiful book will have been, in every way, very useful to science.

I thank you warmly for having sent it to me and I pray you accept, Dear Doctor, the assurance of my sentiments of cordial devotion.

Etienne Drioton

V. received few such letters concerning *Age in Chaos*. Actually, a number of archaeological discoveries were made in the years following *Ages in Chaos* which tended to corroborate V.'s reconstruction of time. One of the most important of his priorities for testing was at the town of El-Arish, between Egypt and Israel, where he believed might be uncovered the capital of the Hyksos, Avaris, and, if so, then there might be demonstrated the further correspondence of Biblical and Egyptian history in revealing that the city fell to a join Egyptian-Judean army, one led by an Egyptian Prince (Ahmose?) and the other by King Saul. This excavation has not been accomplished.

V. paid attention closely to developments in carbon dating, for here was one of the places which he thought might give him a quick and decisive victory. He corresponded with experts, beginning with Libby, founder of the C14 system of dating. His pathetic and persistent efforts to achieve a dating of 18th dynasty objects were put into a manuscript called "Ash," selections from which were published in 1974. To Libby he writes (October 7, 1953), "I also

assume that if analyses of organic objects dating from the time of Hatshepsut, Thutmose II, III, or Amenhotep II, Akhnaton were made, the results will indicate a *reduction* by as much as 500 years from the conventional figures; and over 650 years for objects of Seti or Ramses II or Merneptah." At the same time, he suggests the dating of Pleistocene fossil beds and petroleum deposits, predicting a late date. Libby was unhelpful, but said petroleum datings by C14 had shown "great antiquity."

Now V. begins a circuit of frustration. Finally a German admirer, Ilse Fuhr, who was later to publish a fine work dealing with comets in early times, with courage and persistence obtained 25 grams of three different bits of wood from the tomb of Tutankhamen. V. was delighted and expected the results to show -820, not the conventional -1350. In another letter he did worry over the effects of original atmospheric contamination of the samples owing to a catastrophe. The University of Pennsylvania laboratory performed the tests and came up on the middle, between the conventional and heretical dating. Bruce Mainwaring had used his strong ties with the University to help arrange the tests.

Seven years later the British Museum tested reed and palm nut kernels of Tutankhamen's tomb and emerged with dates of about 846 and 899 B.C., both of which dates were never published and then seemingly lost or misplaced them. Other dates of the 18th Dynasty appeared in time, not so definite and reliable as to dismiss V.'s claims, but not such as to please him.

By this time, Deg had read Melvin Cook's article of 1970 in which, retrocalculating the C14 in the atmosphere, using the rates of Libby, Cook figured that the atmosphere would have had to have been constituted (or reconstituted) some 13,000 years ago. Deg's deduction was that a series of catastrophes would have created the same effect. Further, Deg observed increasingly wild fluctuations as well as a secular swing of the C14 dates from "known" dating and bristlecone pine dates as time marched backwards, and, without straining the discrepancies overly much, he could conclude that carbonating would be both invalid and unreliable before 3000 years ago, which ushered in the Venusian Age (in his terminology).

Deg was further impressed by the studies of John Lynde Anderson and George Spangler, which he read in 1974, not long after their publication, that challenged the very constancy of the radiocarbon component of the atmosphere. Thenceforth he paid small heed to earlier radiocarbon readings, whether they seemed to support or oppose his theories. On the other hand, V. who had expected salvation in C14, could not readily denounce the system afterwards, and played on occasion the game of using C14 dates when convenient to do so, nor did he ever renounce C14 in principle.

To this day, Deg has not been able to understand how V., having succeeded in restructuring the chronology of Egypt to the end of the 18th Dynasty, could then have made further drastic changes needlessly, displacing forwards the great Kings Ramses II and III. Deg had so much confidence in V.'s ability and so little knowledge of later Egyptian history that he accepted the new chronology *in toto* as it came to him by word of mouth, by hasty readings of manuscript pages, and by the published volume of *Peoples of the Sea* when it appeared in 1977, after many years in manuscript and printer's proofs. Very soon thereafter doubts were heard coming out of the "British Connection," from persons whom Deg had come to respect. None of the Americans around V., nor V. himself, had met any of the British and were inclined to put on airs or to rant against them.

Deg did not try to follow the controversy, which was based upon close historical analysis. He thought to wait until the dust would settle. He was made uneasy by a lurking contradiction in V.'s position. The great catastrophist seemed to be putting aside catastrophism in ordering the centuries. In early 1972, William Mullen had written in *Pensée* that

Two assumptions from *Worlds in Collision* are taken as fundamental: first that no chronology using retrograde calculation of the positions of heavenly bodies is reliable earlier than -687; second, that the principle clue for synchronizing histories of ancient nations should be the break caused in all of them by the catastrophic events.

The second point is at issue here. Deg agreed with Mullen. For example, he made the following note :

It is interesting that in one of his articles Isaacson, doubtful perhaps of the strong basis for celestial connection, ventures that V. 's reconstruction of chronology can be separated from catastrophism. This I think not to be so. First, V. would never have revised chronology so boldly if he had not discovered the key to chronology in two parallel accounts of the same disaster -- one in the papyrus Ipuwer at the end of the Middle Bronze Age of Egypt, the other in Exodus. Second, the evidence of catastrophe is what explains the end of the Mycenaean civilization and ties it directly into the Archaic Greek culture that succeeds it, both in the 8th-7th centuries, and then ties both of *these* into the Biblical accounts and many other accounts of the same disaster at the same time. In short, it is catastrophic theory that sired the revised chronology of V. and if the genius of that reconstruction is extraordinary, it is the effect of hereditary genius, a "fall-out" of genius from a single elemental key idea, as Juergens has written. I say this while reminding myself that the Exodus disaster was the key, but the motive came in the desire to reverse the order of Moses and Akhnaton: to recapture Moses and monotheism for Israel. Not that V. cared for monotheism in itself. But since the world regarded it as an invention of paramount importance, he was ready to fight for it.

Not until 22 December 1981, do we find Deg at the denouement of his doubts; writing to Derek Shelley-Pearce (S.I.S.) in England, Deg says:

The Glasgow Chronology is in full swing, it appears, with John Bimson (*SISR* 5:1) and Martin Sieff (Workshop 4:2) pushing it mightily. And the readers, no doubt, a bit giddy.(....)

I am glad to see that Claude Schaeffer 's work has come into its own with Geoffrey Gammon's article in *SISR* 4:4. It is one of only several general studies of value in cultural quantavolution. Gammon approached two points that he might have developed more fully. First, the best benchmarks of past ages are catastrophes: cultural quantavolutions coincide with natural quantavolutions. For a century scholars have been playing at quantavolutionary theory unwittingly by using catastrophic age-breakers. It reminds me of how some early geologists tried to dismiss the word "strata" because that implied discontinuities, and discontinuities implied you know what...

The other point to stress is that the end of so many settlements around -1200 (conventional dating) indicates that this date actually falls between -780 and -680, that is, the Martian period. Gammon seems to shunt aside this evidence when, with his mind perhaps upon Egypt, he says, regarding the destructions that ended the Late Bronze Age, "the evidence that these may have been due to natural causes rather than the agency of man remains scanty." (p. 107)

Perhaps Velikovsky did the same, in order to progress with his idea of further shortening Egyptian chronology; that is, he abandoned his fix on the Martian episodes. To me, the term "*Peoples of the Sea*" is a euphemism for the Martian-Moon-Venus disturbances, a kind of reductionism. Wars, movements of people, and social turmoil are expectable in natural disasters and are a concomitant and effect of them. To show that they happened certainly does not prove that extraterrestrial events and general catastrophes did not happen, but the contrary. Applying the term "Peoples of the Sea" to a construction of a fourth century Ramses III is already a warning sign of trouble ahead; one cannot move Martian events to the fourth century; one may not give Ramses III a special "*Peoples of the Sea*" of his own. The Glasgow chronology may find its clincher by research of Martian period disasters in Egypt, possibly finding the evidence around the time of Merneptah or Ramses III (...)

He goes on to write:

As Sieff says, "By placing the 19th Dynasty so late, Velikovsky ironically obscured the cause for these destructions which he himself had found." The reasons why he did so are also obscure. Granted that my offhand remarks should carry little weight, surely some scholar who understood the catastrophe-culture-history interfaces must have read and disputed this part of the reconstruction of history. When Velikovsky was writing this book with the others still to appear, was he by-passing his own catastrophic benchmarks to complete a descriptive history postulated on different grounds? When the Glasgow Chronology began to surface after his relevant book, soon two books, were in print, I heard recriminations and ducked out. I should have given more attention to this breakup of the consensus around him, but there were too many intimations of the "Love me, love my dog." kind, for which science has no place. I am going to have

trouble with this matter when I come to it in the course of writing "The Cosmic Heretics."

There were to be four volumes of *Ages in Chaos*. The first scored a large success with a group of competent heretics. The second and third volumes, not treating of catastrophe, but of chronology and archaeology, failed to persuade most of the heretics and their dates were soon replaced by a new reconstruction that tied into the first volume very well.

The reviews in the orthodox media were bad, usually attacking V. for the wrong reasons. The fourth volume was held up indefinitely by Elisheva and her daughters. Deg advised that it be printed, even if it held a basic flaw, because V., though increasingly doubtful, intended that it be ultimately published, and because V., though increasingly doubtful, intended that it be ultimately published, and because V., even when he was wrong, was more instructive than most people when right.

None, among the anti-heretics, seemed to notice that V.'s supporters, supposedly so slavish, had quickly and thoroughly analysed and rejected two thirds of his general theory of Egyptian chronology. Indeed the opponents would still proceed as before, talking of his cult and his clique. There was restraint among the heretics in attacking V.'s newer books, and *Kronos* hardly attended to them at all. Evidently, the heretics could also ignore books that they didn't like. Or is this what one ought to do with books that are neither catastrophic nor correct?

For a catastrophist to limit his concerns is difficult. Once you have the planets misbehaving, you must acknowledge that it may have been their wont in earlier times as well. V. decided that he had better investigate the earthly effects of prior cosmic disasters; if prehistoric catastrophes could be demonstrated to have occurred, then historical ones might become more believable. So he wrote *Earth in Upheaval*. V. did not set up a timetable of catastrophes. However, he adduced more evidence that the -1450 to -687 periods suffered grand natural disasters, and he introduced doubts ranging

backwards. He paid little attention to the burgeoning science of radiochronometry aside from carbonating, nor did he ever exert his powers in this area. To strengthen the case for late catastrophism, he brought forward instead the studies of others on glacial melting rates, sudden ocean level drops, very recent alpine orogeny, rapidly drying lakes, waterfall cutbacks, late fossil assemblages, surprisingly recent C14 datings, the simultaneous devastations of civilization (using Schaeffer), excavations of warm-weather life forms and human settlements in impossibly cold zones of today, Indian traditions of orogeny and other quantavolutionary events, changes in magnetic orientations, and the large-scale ash levels on ocean bottoms.

He did not know Otto Schindewolf's work, then appearing, which tied the great periods of biosphere destruction to cosmic events and consequent radiation storms. He followed Dunbar's *Historical Geology* in examples of very early disastrous effects. He advanced the idea that coal was formed from biosphere masses propelled and dumped by huge tidal waves, without specifying which waves and when, and used Heribert Nilsson's studies of German coals to prove his case. He relied heavily, too, upon the early English catastrophists. He used also the work of American creationists.

In a few lines, he expressed his feeling that the uneven lengths given to the ages were "basically wrong;" The remark is strange, cryptic, confused. He "does not suggest either a lengthening or a shortening of the estimated age of the earth or the universe," and then adds irrelevantly and naively that a religious mind should not be upset by great ages. It was all rather humanistic and old-fashioned.

Deg found that the accretion of evidence of catastrophes was much easier than the application of a time scale to them. V. had not set himself to demolishing the new techniques of radiochronometry, possibly because he believed them valid, possibly, too, because he felt that he could obtain the right to his catastrophes down to Noah (6000-9000 years ago) without contending with radiochronometry, which does not begin to operate, except for C14 and certain tests still in the realm of the exotic, until 100,000 years back.

Also V. had done practically all of his writing before the issues of radiochronometry came forward, before several of his supporters engaged in its study on their own accord, and before the creationists had worked to discredit it.

Deg set himself two tasks. One was to set up a model of past catastrophes, hence of the ages. The second was to classify and survey all existing techniques of measuring geological time, and to state the grounds for believing them invalid. He had always to bear in mind that one of them -- he ultimately included over fifty measures -- might be valid, even if grossly valid, and thereupon would seriously damage his model of natural history and at the worst render the model only an intriguing metaphor. He was surprised repeatedly as he went from one test to another to discover that none existed without a flaw or a question, either of which might be fatal to its validity or reliability.

His major teacher was a man he had not met, Melvin Cook, who went on a rampage among the uranium-lead, potassium-argon, and other tests, pointing out inconsistencies, contradictions, incompatibilities, and arbitrary assumptions. Cook was not an exoterrestrialist. His attacks are almost all from the materials of geology and chemistry. His exoterrestrialism, such as it is, comes in estimating intakes and outputs of gaseous elements from the earth 's atmosphere.

Perhaps the valuable critics of radiochronometry number no more than a score. Deg could name a half-dozen besides Cook whose work he regarded as heroic and essential to establishing and maintaining his perilous stance. I mentioned Anderson and Spangler on Cl4. There was reliable Juergens who showed theoretically that the electrical environment could effect enormous changes in radiation rates, such as to annihilate time. There was N.J.G. Sykes who, in a simple test published in the *S.I.S.R.*, gave grounds for believing that a changing magnetic field would augment or diminish radioactive decay rates. Then, too, there came Roy Mckinnon, also writing in the *S.I.S.R.*, and Thomas G. Barnes, writing in 1977 on the recent origin and decay of the earth 's magnetic field.

R.V. Gentry and his team repeatedly showed, to everyone 's

astonishment, that extremely short-lived polonium halos occur in the absence of parent uranium, evidencing that the host rock was formed very quickly. Coal was examined that seemed to have formed in days instead of millions of years.

Deg began to treat the longer-range radioclocks as he did radiocarbon dating, an indicator at best of relative time, and vulnerable to the kind of electro-chemical turbulence that is inherent in natural catastrophes that begin with disorders in the sky. Essentially this freed him to consider together all factors that could have left some indicator of time upon or around a specimen rock or site. Since no technique appeared by itself to be a tamper-proof, independently set, and auto-operative clock, every technique or test had to take its place in the group of indicators of time, some of which were carried into the setting to measure its time and others of which were inherent in the geology and circumstance of the setting. All too often, geophysicists came to believe that there is scientific validity in what is a purely administrative and industrial axiom -- that tools and products should be standardized in as few forms as possible -- and therefore they assumed that there must be some true superiority in a tool like potassium 40- argon 40 radiochronometry because it can physically be applied to any strange igneous (and now metamorphic) rock that is carried into the laboratory.

Deg came to rely, too, upon some very general ideas in concluding that the time of the world and of the ages may have been very short. These had an air of philosophy or, worse, homespun reasoning about them that is infuriating to technicians intercepted on their way to their laboratories and machines. For example, Woodmorappe's painstaking survey, published in the *Creation Research Quarterly*, of the successive occurrences of the earth's several eras, as denoted by its surface rocks, shows a preponderance of discontinuities through the series of eras. Also, the macrogeography of the Earth seems to call for a giant micro-chronic integrated episode.

Inevitably, then, the mind was jostled to close up time radically in the period between hominid and man in the face of evidence that the hominids were human-like, and very little time was required to achieve a culture. Thus, microchronism lent itself to Deg's theory

of *Homo Schizo*.

Then, upon arriving at the notion that the earth had been recently ravaged, Deg began to wonder how the earth could have survived for very long if it had begun to suffer one after another disaster through four billion years; this led two ways; first, to shorten time in order to admit the fact that the earth still exists and has a biosphere even if, like the old grey mare of the song, "she ain't what she used to be," and, second, to postulate, even then, some backward limit in earth history to a beginning of the period of disasters, and thereupon he asked himself what might have been the first great catastrophe to threaten the world, and what started it -- giving him Super-Uranus, and a binary system in throes of disintegration, a baseline of perhaps 14,000 years for the first great destruction, and an initial electrical explosion arising naturally from a pre-existing electromagnetic system.

When Milton and he sat down to discuss the system before the age of catastrophes (now compressed into the Holocene of 14,000 years), they found no need in their binary system, with its highly productive, enormous, magnetic tube, for more than a million years to accomplish all that was new under the sun. Their model of the solar system probably included errors of great magnitude; it might have major system failures; and it might even be basically wrong: both he and Milton freely acknowledged this; but they were ready to race it against any other model in the field.

Having spent much of his life in building (not inheriting) a science, that of the study of political behavior, Deg did not take kindly to inference or statements that he did not know what science was all about. He replied sarcastically on occasion that indeed he did know what science was about and it was up to no good.

When *Chaos and Creation* appeared, he sent a copy of it to the University of California physicist, Walter Alvarez, in appreciation of the study his team had published, exhibiting the existence of an iridium layer that might have fallen out from a meteoroid explosion, contributing to the demise of the dinosaurs. He took the occasion to ask "whether you remain convinced of the validity of radiometric dating, granted the possibility of catastrophic radiation and heavy

subterranean heating."

Alvarez replied, "In answer to your question: I consider radiometric dating to be an excellent tool that gives reliable dates. The systematics are well understood in all except the current frontier areas, and serious practitioners are well aware of the possible sources of problems and how to avoid them."

From which answer, we may all take heart. In accepting kindly the book, Alvarez wrote "It helped me appreciate clearly the difference between the basically anti-scientific, Velikovskian approach and the way a scientist would seek to understand nature." Need I say more?

PART FOUR

CHAPTER TWELVE

THE THIRD WORLD OF SCIENCE

For a decade from the appearance of *Worlds in Collision*, no quantavolutionary circle existed in the world. V.'s correspondence with his readers was voluminous. Immanuel and Elisheva were socially active for several years, but no scholar who could be said to be of catastrophist persuasion was a frequent correspondent or friend. In July 1956, Claude Schaeffer, author of the monumental comparative study of archaeological levels of destruction wrote Velikovsky his appreciation of receiving from him a copy of *Earth in Upheaval*. V. had used Schaeffer's work in preparing the book. In 1957, Immanuel and Elisheva visited with the Schaeffers for a week at Lake Lucerne, in Switzerland. Schaeffer did not agree with any part of Velikovsky's ideas except what Schaeffer himself had printed before V.'s work had appeared, that periods of sudden destruction had befallen Bronze Age Civilizations.

Two decades later, Deg and Anne-Marie Hueber visited Schaeffer at his home near Paris. Deg wanted to update Schaeffer's inventory of sites, and they had corresponded briefly on the matter. Schaeffer had offered Deg the materials of his files about which he had written to V. many years before. Then he had spoken of "new confirmations of the reality of these crises on a continental scale which I have tried to analyze. I would be glad if I could write now immediately the contemplated second edition of *Stratigraphie Comparée* in two volumes, for with the new confirmations these Crises could no longer be questioned...so striking are proofs and so accurate the dates established by the new discoveries..." V. had not told Deg of his correspondence or of Schaeffer's intention of moving forward. V. had passed up a rare chance at statistically demonstrating his theses. Nor had he exhorted others to undertake work with Schaeffer. Deg had to suggest the idea to Schaeffer as if Schaeffer had never been aware of the possibility.

Schaeffer was ready to collaborate. It was clear to both men that V.'s reconstructed chronology was not be at issue. Their aim was to confirm the ubiquity and internal cohesion of Schaeffer's set of catastrophes. Deg was made aware of Schaeffer's doubts of V.'s chronology, especially that coming after the 18th Dynasty of Egypt, doubts that were even stronger with Madame Schaeffer, who at one moment was with the group and at the next was out of the room tending to her visiting family. Deg conveyed his belief that the catastrophic sequence of Schaeffer could slip forward nicely, using the same intervals, to fit the scale that he had drawn back to the neolithic age, which included V.'s fifteenth and eight century disasters. Thus Schaeffer's sequence could serve both the conventional and the quantavolutionary calendar.

Deg sought funds for the research from the American Geographical society, without success. [The proposal is carried in *The Burning of Troy*.] He tried to reach Schaeffer in Paris in 1983. Schaeffer had just died.

With the appearance of *Stargazers and Gravediggers* in 1983, a reader might see how barren was Velikovsky's personal and scholarly life during the 1950's of the very people who were capable of or were independently pursuing studies in quantavolution. The characters in the book are mostly his opponents; few friends and supporters appear. The only persons of catastrophist persuasion mentioned were Alan Kelly (but on nothing to do with his catastrophism) and Claude Schaeffer. Alan Kelly, and Frank Dachille who was his collaborator in *Target Earth* (1953), lived far apart and they worked alone.

In American biology, Goldschmidt and Simpson knew there had been quantum jumps in paleontology and presumably their students acquired some inkling of the anomalies. In circles espousing Biblical literalism, the work of Price and others was discussed. There must have been other catastrophist scientists of the 1950's in America and England, but to this day Deg has not been able to name any. The existence of perhaps half a million readers of V.'s books meant little so far as research and writing were concerned. Some bootleg teaching of catastrophism was occurring, especially

among fundamentalist Christians. In Germany there were Schindewolf and Nilssen in paleontology, as I noted elsewhere in these pages.

Significant differences came with the sixties. The civil engineer Ralph Juergens left his business in the Midwest and moved to Hightstown, near Princeton, so as to be near Velikovsky and to use the libraries of the University. Warner Sizemore, a minister and graduate student of philosophy appeared on the scene at the same time. Stecchini, historian of science and unemployed professor, was already there, indulged by his wife Catherine, a star teacher of young writers at Princeton High school. While teaching at the University of Chicago in 1950, Stecchini had signed a letter of protest to Macmillan against the treatment given Velikovsky's book.

When Deg met V. and decided to publish his story, there was none else in sight. They thought of Eric Larrabee, but none would be paid to write, and Larrabee was busy with unrelated affairs. Since Deg could not do the whole job himself, Velikovsky recommended Juergens, then working for McGraw-Hill as a scientific editor, and Deg and V. persuaded Stecchini to do an historical portion. Thus, all the effective resources of V. amounted to three men who could and would write about his case in depth. This was the first time any cooperative group had engaged itself in the study of V.'s problems. It was also the first time that V. realized the values and capacities of voluntarism in America. He was, however, cunning about the media. For instance, as soon as the *American Behavioral Scientist* was in the mill, V. could persuade Larrabee to write an article for *Harper's Magazine*. Larrabee was spurred into action and the article came out two months before the *ABS* issue appeared.

V. was inspired and a new outlook, that of a movement, of helpers, even of collaborators, dawned upon him. Before then he had been a lone wolf in his field of study. Now he had friends who talked his language. Sizemore began to organize locally and to suggest that others organize in other places clubs or study circles under the name of "Cosmos and Chronos." V. referred often to these ghost legions. Sometimes they sprang to life to extend invitations to V. to speak at various places, or they were used as a letterhead

denomination when rebuking critics. It was, for example, on 'Cosmos and Chronos' stationery that the Philadelphia disciple and high school teacher of psychology, Robert Stephanos, addressed the Franklin Society in seeking to arrange a lecture invitation to Velikovsky. When the Society reconsidered and hastily closed its gates to V., it brought a certain public disgrace upon itself.

Inspired though he was by his association with new and competent men, V. himself could not be organized by them; he could seek only to determine all of their activity, without becoming controlled by them. Time and time again, spurts of organization occurred, with excellent initial results, but thereafter the efforts would slump and expire. The most successful organizing and activity was done out of his reach, in Canada, England, and in Oregon. He was too immense to allow himself even to be the leader; for a leader implies followers who are assigned responsibilities, are allowed judgment, employ initiative, and can be trusted. V. allowed none of these. There was to be no control over this leader; he was superman, distinct from the following, distinct even from a field of science for he refused to call it by a name, such as catastrophism. He would deny such allegations and not even perceive the distinctions. Nor would others, because it was unbelievable. It was nonetheless true of him. Among the types of activists of a movement there may be distinguished: the theorist, the researcher, the publicist, the agitator, the organizer, and the fund-raiser. A movement is oligarchic to the degree that the functions are concentrated in a few hands; it is bureaucratic to the degree to which the oligarchy assigns and restricts these tasks to specialists; it is democratic to the degree to which anyone can do whatever one pleases. *Pensée* was an oligarchy, *Kronos* developed beyond oligarchy into autocracy. The S.I.S. was an oligarchy with high turnover and open access. The cosmic heretics as a total aggregate were anarchic, and formed and transformed plastically, so that one could perceive the aforesaid stable organizations, then glimpse pairs, trios, bands, circles, and groups in process of becoming (such as C. Marx's small Basel group that embraced Professor Gunnar Heinsohn of the University of Bremen, and Milton Zysman's Toronto band, and Luckerman's small Los Angeles operation). The attentive public shaped itself over the period into *ad hoc* opponents and task forces (such as the

AAAS panel), into members, supportive audiences, subscribers, book buyers, gossipers, fund-donors, materials-copiers-and-circulators -- reflections indeed of the several functions, anarchically undertaken.

An instance of the highest type of voluntarism came with Alice Miller, a San Francisco librarian, who put to herself uninvited and uncompensated the task of indexing intensively the works of V., and V. made the necessary arrangements to publish the book. The few scholars who obtained this work could now search to their heart's content for the fullest play and nuances of ideas (where such fullness existed) and for contradictions and errors. The first operation to be performed in serious criticism in as index; the memory of a reading or two rarely sets up written material adequately for analysis. Would that every high school student who today is being hastily introduced to a computer would be instructed in the philosophical logic underlying the indexing of content. Deg longed for an Alice Miller for his Q Series; his indexes were inadequate, even more than V.'s, because his work contained a larger proportion of abstract materials, which are harder to index. He found, for instance, that searching for "monotheism" in V.'s own indexes was useless; in Alice Miller's the idea came forth nicely, even beyond what V. might have wished to expose.

We return to Deg's favorite pastime of counting, listing, and categorizing, and to his figures of the numbers involved. They are impressive for they may be exponential. Despite the casualties, the deaths, the desertions, the languishing, and the waywardness, and counting parallel little groupings and isolated active scholars, by the end of the decade of the sixties there were perhaps thirty true scientific catastrophists who had come up by the non-establishment route into the field of quantavolution, and by the end of another decade, there were fifty more creative workers in the field. Shadowing these, watching intently, and supporting them were several hundreds of others, close in.

Shadowing the cosmic heretics, too, were a new group, union-card holders of the establishment, who are distinguished most readily by their denial that they are or ever were sympathetic to Velikovsky or any other quantavolutionist, or that they have ever sought or do

now seek any ties with cosmic heretics. And these were equal and greater in numbers, carrying out the revolution by partial incorporation, the process whereby a revolutionary movements, as it advances, meets an opposition that has already been infected by and has adopted in part the principles of the revolution. It is at this point that most successful movements subside or are destroyed; their heirs are their enemies.

As one can see, if workers number, say, 15 in 1 decade, 30 in another, and 80 in the next, a doubling process may be occurring, against all predictions that might be based upon resources available, unchanged state of the opposition, and so on. At this rate, with 150 to 200 in the 80 's and 400 in the 90 's taken with the activists who lend support to their views, the quantavolution viewpoint should enter the millennium primed for a large role in scientific thought. At the same time, it should be borne in mind, there will be attrition and desertions, doubling, and trebling the numbers of quantavolutionists outside of (but beginning to merge with) the establishment. But the threat of nuclear warfare to all civilization overshadows projections of science. One is tempted, in all of this speculation, to recite Keynes' ironic words, not about short-term economic policy but about short-sighted world politics: "In the long term, we 'll all be dead."

Be it admitted that Deg, publishing a special issue of the *American Behavioral Scientist*, had a perfect subject and extraordinary materials in the Velikovsky affair. But why should he stick with Velikovsky? Let Velikovsky say his piece and then be done with it. What of next month 's issue of the magazine, and the month after? The journal needed continuous attention. What of the state of political science, and of higher education, if which he had always been so critical? What of the state of the nation, *ibid*? What of his family staggering into adolescence in the disturbed and unruly Princeton atmosphere? What of his meager fortune, skating on a thin monthly bank balance and a home mortgage? And his friends, the women and men who had been no more conversant with Velikovsky than he himself? And his book contracts: especially the *American Way of Government*, a good textbook in need of

revision, whose care would lift his finances from year to year and carry his name around to hundreds of college communities. And the radical book on behalf of congressional supremacy that he was writing?

What of his reputation, that, in line with the customary in academic careers, should now begin to rise to a peak, abetted by the constant "mending of fences" and "nursing of the constituency" ordinarily pursued among scholars in his circumstances? Or should he not now throw in his fortunes with a political party, Democrat or Republican, it mattered not, for in both he had "friends in high places." Close friends welcomed his participation in Barry Goldwater's camp and in Hubert Humphrey's; this would appear strange unless one understood that subjectively Deg was confident that he was his own man, and that he could find equal opportunities in both camps to exercise his skills and ideals, which, to put them in several words, were: decentralization, basic income guarantees, voluntarism, legislative rule at home, and representative government for the world. The American party system, however, no wise shared his bent for change.

In all of this and through it all, why did Deg continue to involve himself with Velikovsky's problems? Did not he have enough problems of his own -- larger and more serious and worse? Did he not have as grand and earth-shaking ideas himself? Most of all, if he was to spend a great deal of time in promoting somebody, and it was not to be "the next President of the United States" then why didn't he build up his own reputation?

He had had mean reviewers, scornful ones, too. His books had not sold very well, he had not yet won any considerable prize, no Pulitzer, no National Book Award. Still he could drum up audiences at colleges around the world. Bill Baroody wished that he might tour the country on behalf of the reconceptualized *American Enterprise Institute*, addressing public issues and garnering funds in the end. He was in mind as a political campaign manager here and there in the nation. He was offered the job of heading the social sciences division of UNESCO in Paris (and refused).

Why should he waste his time on a political campaign in science, especially one that had already been victorious *in principle* (Jastrow, Polanyi, Sagan, Motz, Neugebauer, Kurtz, Hadas, and dozens of other personages had sooner or later pronounced themselves against the ill treatment of Velikovsky). Did not Elisheva insist to the end that he had opened up the final phase of Velikovsky's public appreciation? Was the establishment of the motions of Venus so important? Or the evidence of ancient catastrophes on Earth? Or the likelihood of collective amnesia, a common enough idea of wise men of all ages? Must the world of science sign line by line in agreement with Velikovsky's book -- the ultimate wish of a cult? No, none of this was so important. Well, what then? Was he sexually deprived? Did he identify Velikovsky with his own father? Many more motives offer themselves. Can one ever know? Why bother to ask, too? Yet it is a question that was asked at scores of lectures, receptions, meetings, and in personal discussions, a question that came out of the interest that people felt in their own motives, out of curiosity about what might be construed as altruism or some other form of abnormal behavior. It's Alfred's *halva*, Nina would say, meaning the joke about the man who loved sweet "Turkish Delight" and would turn the conversation to it at the slightest cue.

Deg behaved as he did partly because he had enjoyed enough successes in other matters and success bored him. Deg did not attend to promoting his academic career because he was already a tenured professor, "heavily published" as they say, and where was there anything further to be gained; universities and colleges seemed ready to succumb to stupidity or insane revolts, but not to total self-evaluation and reform. They were, with governmental help, becoming ever more bureaucratized and inane.

Besides he found self-promotion an embarrassment, all the more as he watched his acquaintances climb the rows of ladders inclined against decrepit edifices where committees and trustees held sway, and important research was kept in a corner like a bastard. He was not adverse to fame. To the contrary, he expected it to be "handed to him on a silver platter," to use one of his mother's expressions. Subjectively, he desired glory; objectively, externally, he had to scorn it.

He was having his last words on Congress and the executive force, an appeal for the preservation of republican government that went against every major political and economic interest in America (and that communists and socialist when in power also and even more rampantly suppressed). He was, as I said, uninspired by the political movements of the moment, and even more so as they developed through the sixties and seventies of the century. The kindling problems of his family would burst into flame but he had no intention of becoming party to a decade of adolescent rebellion of the kind that ruins the best years of many Americans' lives. Besides, did he not have such splendid plans for going en masse to Europe for a year to teach the children foreign languages and escape the menacing youth and drug culture of Princeton?

But look particularly to the controversy surrounding the Velikovsky matter: was it not exciting? The ideas at stake were of the highest order. Not only in sociology: for what sociology is more important than the sociology of knowledge (*Sozialwissenschaft*) that he had cut his eyes teeth on with Mannheim, Wirth, Shils, and Leites, and which was really the theme underlying his first book, *Public and Republic*, where ideas of representation were shown to be unconsciously operative and externally effective over hundreds of years and many different political generations?

Also there was excitement in the substance of this strange new kind of science. Scattered about but eager to stay in touch were dozens of intelligent people interested in one or more of the hundred fields upon which quantavolution impinged. More exciting and elevating than yachting, the horseraces, gambling, cocktail parties, tourist travel, religious routines, better than the eviscerated or wrongheaded politics of the times. In the final analysis it was the unlimited firing of sky rockets in all directions that held Deg to the course of quantavolution and bound him to his friend Velikovsky.

There was the intransigent personality of Velikovsky. Even some opponents, Robert Jastrow, Walter Sullivan and Motz, for instance, found him fascinating. He was always there, the tallest mountain in Princeton and anywhere else, so far as Deg could observe.

A series of entries from Deg 's Journal, most of them from the year 1968, show what I mean. But first a letter from Velikovsky to Deg, before the *ABS* issue of September 1963 had made its impact, to show that V. had no intention of letting his new friend escape his camp by crossing the ocean:

August 16, 1963

Dear Professor de Grazia:

It was very good to have a letter from you in Paris. I like to hear that you may come to the States in October. No old castles here, no ancient arenas, but you will be most certainly engaged in some skirmishes in the tournament for which the scene is being set. Larrabee 's article produced certain effect (I assume it was mailed to you) and the foundations of the establishment are being loosened. (...) A few papers started to comment on the issue, one or two colleges invited me to speak before their students, much discussions going on without reaching the printed page, and I am emerging from the "shadow of darkness." (...)

I wish I could bring to our side a few prominent scholars and scientists. I write to de Madariaga about Lord Russell whom he knows. You may say again, 'Cabot', but visualize the effect on the closed scientific ring of one such renegade.

I wish to think that Mrs. de Grazia and your children are enjoying their many new impressions, and the old villa makes them feel that theirs is part of an old heritage. Turgenev wrote someplace that two urges live in a human soul -- a striving for far away lands and a longing for the homeland and home. Mrs. Velikovsky joins me in wishing all of you good health and animated months ahead.

Cordially Yours,
Immanuel Velikovsky

PS The mail brings an envelope with copies of letters received by Harper's. Menzel of Harvard Observatory writes a 17 pages letter, unfair, emotional: he exposes himself to embarrassing statements of fact. A battle of letters started. At the present, the response runs 50% against 50%. Therefore any articulate supporter -- or opponent -- should enter the fracas, the earlier the better. Mobilize your friends! -- I. V.

A year later, Deg was not only still in the camp, no matter where he was, but he was suffering privately the annoyances of the camp. His journal of September 1st, 1964 from London is relevant. He is on his way to the University of Gothenburg, Sweden, to lecture on American politics and will from there go to Marina di Massa where his daughter Catherine will be wedded to the best-looking boy on the beach, Dante Matelli.

Left for London at 10 AM. On way to airport penciled a crude note to Velikovsky, finally telling him bluntly of my feelings towards him. I said, "Dear Immanuel, I am writing this on the bus to the plane. Last night I went again over the letters and material for Rabinovitch, to the detriment of many pressing affairs. I finally decided to send out nothing at the moment.

"You will receive the page proofs on the Margolis critique. Please make only absolutely necessary corrections (I do not care if you offer to pay for them.) Issue is already late.

Please do not call my office *or* the printers. Your inability to let go of anything will be the ruin of our friendship and of the magazine. Sincerely, Alfred".

I handed the letter to a passenger agent just before stepping aboard the PanAm Clipper. It culminated a day of annoyance and desperation that began when I courteously called Velikovsky to say goodbye. To those who know him well, the history of the next 24 hours was to be clear. He wanted to rewrite letters, call lawyers, discuss imbroglios, in short, utterly and without conscience disrupt my carefully measured out and urgent last hours before departure. And worse, he succeeded.

This hardly matters. The friendship, the campaign, continues, and V. is still the mastermind. When Deg goes abroad in 1966, V. has ideas of how he should spend his time in Israel and Egypt:

Feb. 14, 1966

Dear Mrs. de Grazia: Please *do not* send this letter to Alfred if he already left Italy. *Im. Velikovsky.*

Dear Alfred:

I received your note written before leaving for airport. Should you visit Jerusalem you may wish to give personal regards to President Zaluccan Shazar -- our friend, especially of Elisheva, of many years. He will be glad to hear that Elsheva is active as sculptor and as a chamber-musician (as good as ever); and Elisheva wishes him to know of the change in the attitude of the scientific world to my book with many discoveries of the Space Age; the fact that I am invited to speak at Yale, Princeton, Duke, Pittsburgh, Wisconsin, Oberlin, Brandeis, etc., is an indication.

I wish you good weather (pleasant driving, good new friends, and many invigorating experience).

Regards from Elisheva and my regards for Paul and John.

Yours,
Immanuel.

[P.S.] It would be good if at the Cairo Museum you could obtain some organic object of the time of Ramses II or Ramses II (or of both) for radiocarbon test (better seed, mummy swathing, leather, papyrus, linen -- and not wood, if possible) at the lab of the University of Pennsylvania (Dr. Elizabeth Ralph.) To apply to Dr. Isnander Hanna (Director at the Lab at the Museum). The material needs to be sent from museum to museum with all the precautions. By far better not to mention my name.

If any difficulty, I shall try to obtain the samples by asking Dr. Ralph to write to Dr. Hanna.

Deg ' s Journal, January 18, 1967

Phoned Velikovsky tonight. Elisheva came on the wire too, at his request. I told them what I was doing to institute a Foundation. He was quite subdued. He is not used to having anything taken out of his hands. Both were happy, I could tell, at the thought of something they had talked so much about moving so quickly to a climax.

Anti-Velikovskianism ' s first line of defense is the impossibility of his theories. Then, I suppose, if proved right, it will be said that he was a simple scribe: he read an inscription which told

what happened. That position will not endure, either, for he worked in a superhuman way to piece together the shattered mosaic.

Deg's Journal, November 15, 1967 9 P.M.

Immanuel called met at twilight to tell me Stephanos had called his attention to the Nov. 3 issue of *Science* magazine wherein Professor R. Eshleman of Stanford University, Electrical Engineer and Co-Director of the Stanford Center for Radar Astronomy had raised briefly the question whether the baffling puzzle of Venus being 'locked-in' to Earth might be answered by the Velikovskian hypothesis of an historical collision of the two bodies. A year ago *Science* refused to accept an advertisement for one of his books. "who knows, Alfred, whether the Nobel prize, which has had a poor record very often, might not come." I said, "Immanuel, your biography is your triumph. You do not need these foolish prizes."

Deg's Journal, 1/4/68 [Providence]

At 2:30 I left the ribald company of Mike N., N., Jim Kane, Al Saglio, Tom Yatman, and Edwin Safford at the Spaghetti House to visit Prof. Otto Neugebauer at Brown University. His office is in an old red brick house next to the new Library and has an entrancing scholarly air to it, closed into the basement, holding several tables, everything with a century old appearance that I too should find a perfect atmosphere for quiet study and work. O.N. was somewhat suspicious of me, as well he might be, knowing that I sponsored a special defense of Velikovsky's work. However, like most true intellectuals, once engaged, his defenses were down and he spoke vociferously, indignantly, said he couldn't waste time on the foolishness and trickery of V. but proceeded to amplify at great length, his little blue eyes peering directly into mine and his slight but determined German voice carrying effectively, even colloquially, his arguments. He disputed hotly the idea that there had been or was any conspiracy against V., (I stated that I too disagreed with V. on this point), and he felt that V. was employing the tools of propaganda and sophistry against him and others. Who can deny this, too? But there seemed to be little reason to go into the political aspects of the controversy, inasmuch as O.N. could not know, more than V., the dynamics of this process, and I essayed questioning him upon several critical issues concerning Babylonian tablets.

He declared twice that he had "no investment" in the words of the tablets and could take or refuse any interpretation, depending only upon its truth. They were only a minor interest with him, not even "minor," less than minor.

He said he had not read Stecchini's interpretations of Kugler's work (and declared offhandedly but vigorously that much had been learned since Kugler's time anyhow). He declared that the observations in the Venusian tablets of Ammizaduga came from erroneous reportings of lunar movements that, in turn, had been used by the Babylonians to measure the movement of Venus. An amateur, he said, would transfer his ignorance of the ancient reports into a wrong interpretation that it was Venus, not the Moon, that was moving erratically. He declared emphatically that from their beginnings around 700 B.C. there were no unexplainable irregularities. (He kept reasserting, and I had to stave off as not relevant to the argument, which was the empirical facts re the tablets, that the whole V. thesis was mechanically impossible, that any 10-year old schoolboy would know how the Earth would be destroyed by anything approaching a collision with Venus, and so forth). He said further that there was little or no reporting of any planetary behavior in a scientific way prior to about 700 B.C. (I didn't press for the exact date) that, for instance, there was no reporting of Saturn before 400 B.C. Earlier records are largely the oracles which deal with sun, moon, and a bright star (which *could* have been Venus, since it is the brightest and hence would oppose V. 's theories of the non-existence of Venus before ca. 1500 B.C.) He asserted further that Egyptian chronology was perfectly established, on the basis of the Egyptian lunar calendar (based on a thirty-year cycle) that carried back to the very earliest times. He claimed that the whole V. affair showed the basically anti-intellectual atmosphere of the population.

I asked whether it did not show also the failing of the establishment of science to perceive its "public problems," and offered the opinion that if he, and others such as Harrison Brown, had dealt with V. 's work more seriously, there would have been no prolonged vicious aftermath, to which he grudgingly acceded.

Then he added that there should not be such an accent on "going to the moon" so that billions were being largely wasted, for which sums the whole of Mesopotamia could be dug up

down to its virgin soil. Then said he, we should have all of these problem solved. To which I agreed.

I asked whether someone should not set forth the thirty or sixty principal factual theses of V. and find specialists on each topic to criticize V. He had mixed feelings about the idea (first taking it personally, of course, "I don 't have time for that!") holding that V. 's ideas were too vague to discuss, that this would prove that the "conspiracy" actually did exist: that there would be too few to undertake the job in certain areas (such as his own of Assyriology and Babylonia); but that it might be a proper way to get to the heart of the matter. He was, on the whole, quite negative re the general problem and hostile to V. As I was leaving, he said: "I just received a letter from Chandrasekhar of the University of Chicago. He is the physicist. He asks whether we shouldn 't do something about the *Yale Scientific Magazine* issue of V. I replied that there was no use to it."

I walked out into the winter snow-threatening afternoon and down the streets of exquisite old structures of Providence 's East Side to Mike 's house, thinking of what I had learned and of the beauties of this old part of town.

1. N [eugebauer] is convinced V. plays a tricky game: "He couldn 't answer my colleague 's questions at a Brown University meeting, but said he would reply to them the next day. Then he didn 't appear."
2. He believes V. to be a foolish and wicked amateur.
3. His direct assertions concerning the Venusian tablets should be worked into a direct encounter with V. 's words (...)
4. N appeared uncertain about Kugler, and unconvincingly dismissed him.
5. N is persuaded that V. is arguing in a great circle, using established theories as grounds for criticizing deviations and unknowns and for proving the deviations accord with his theories, then destroying the established framework without perceiving that his interpretation of the deviations is itself dependent upon and sponsored by the established theories. N. did not say so, but this kind of problem is fundamental to all theoretical change: man is dependent for what he sees on what

he has been taught to perceive, so how can he prove wrong what he has been taught, if his new vision is wholly dependent upon being preceded by the old one ?

6. I feel the need to organize an 'Anti-Velikovsky' symposium where highly reputed scholars are asked to address themselves to a meaningful segment of a carefully prepared set of questions that test the whole fabric of V.'s theories. Logically V. cannot dispute this procedure. It would, I think, cause him to be angry with me. So be it.

Deg's Journal, January 20, 1968

I have been visiting with Velikovsky once or twice a week since November, and have reread *Earth in Upheaval* and *Ages in Chaos*. Since I have been heavily occupied with the theory of activities of the federal government, the American Government text revision, a plan for a business company should I decide to leave the academic world, and so forth, I indicated to V. ten days ago that I could not organize the magazine that we had always talked of publishing. Then, for some reason, a week ago, I thought "We must start a foundation for V. and his work." I asked Richard Kramer to initiate the papers for organization of a corporation not-for-profit in N.J... settled on PO Box 294 and my home as the address, and decided to ask Juergens, Stecchini, Kramer, and Herb Neuman to join me in the first Board of Directors. I called each man to invite them aboard and received their prompt acceptances.

Deg's Journal, March 2, 1968

This morning I am resolving to withdraw myself as much as possible from Immanuel's campaign for honors and recognition. A full eight hours went to him yesterday; it is too much, considering what I must, do for my own work. In its way, it deserves the same kind of attention V. gives to his and I give to his. My intellectual children may be scrawnier but I cannot turn them out to starve in the cold. I give up lectures that, just like his, might explain my ideas and bring me income, as for example one that I turned down today for \$100 and expenses before an audience of civil service officials in Washington. My ideas go undefended, many aspects of them go unexpressed. I do not give them the tender, fierce, loving care that every man's respectable notions deserve. Let's see whether I can behave by this resolve.

Deg's Journal, March 3, 1968

March is come cold and blustering. Jill and I rode our bikes to Mom's where Ed and his young friend, Margaret C... were visiting. We arrived frozen. M.C. has just returned from 2 weeks in Boston, under the tutelage of a Yoga guru. I say to Ed, in greeting, 'Ah, here is the "slim, elegant Sicilian!"', quoting Norman Mailer's autobiographical novella of the "March on the Pentagon" that is printed in the current *Harper's Magazine*. [Edward organized the legal defense of the arrested protesters.]

Jill says, of Margaret, 'Girls who have had trouble with their fathers work it off well. Girls who have had difficulties with their mothers do not.' She cites Jung on the point. And we string out many examples. It is probably true, even as an unrefined statement. I ruminate: so important, so simple are basic truths. What conceals it and them? Great truths and discoveries are not hidden by their complexity but by jamming of our ideological cognitive, and perceptive machinery.

Velikovsky, the other night, quoted me Butterfield's comment that the very young can understand principles of science and nature that have baffled the greatest minds of history. I think V., who is in essence a philosophical realist, uses this idea in only a limited way. He means that the young haven't had their tender minds distorted by unfact. It is more importantly to be understood that the mind is structured in each generation to receive some truths and reject others, or better, some half-truths. Both V. and perhaps Butterfield unjustifiably abstract the mind from its context. It has, for instance, been pointed out by numerous defenders of classicism, such as neo-Thomists, that we believe the ancients foolish or unperceptive of truth because of our partial and current truth-idolatry; freed from contemporary ideology, we can understand truth as the ancients discovered it and agree with them.

Deg's Journal, April 30, 1968 A.M., en route to NYC

Half of this past warm flowering weekend in Princeton has been spent with Velikovsky or on matters related to him. We spent Saturday afternoon going over materials that might be suited for the proposed book "V. and his Critics" that I am discussing with Kluger of Simon and Schuster. We spoke also

of the foundation for Studies in Modern Science, which I have organized. He named eight major problems that are critical to his theories, and I am taking them into consideration in the memorandum which I am preparing on the program of the Foundation. Bob Stephanos called me on Friday night upon my return from NY to tell me that Mr. Mainwaring of Philadelphia, an admirer of V., intended to help financially. Both V. and I had written letters to M., who runs a family manufacturing firm and is, I hear, a person of some intellectual stature. V. was naturally pleased. He talked on and on, I edging him back to a subject from time to time.

Sunday evening, V seized the initiative and called Prof. Philip Hammond of Brandeis U. to ask about his possible interest in excavating at El Arish for signs of the siege of the Hyksos fortress by the allied armies of Saul and Thutmose, about 1050 B.C. in V. 's chronology. The digging would be a crucial test of the V. theory of ancient history. Hammond, who had given indications of sympathy years ago, appeared enthusiastic. He offered to go El Arish with two assistants if we could organize the expedition.

After learning this from V., I called David Dietz to ask whether he would still be interested in taking part in the expedition. He was. Yesterday, Monday, I asked Harry Hess of Princeton University Geological Department to serve on the Board of Trustees of the Foundation. After some demurral (later, V. would be mystified by his hesitation since 'Hess definitely agreed to join.' but I was not mystified.) Poor Hess who is one of the busiest man alive with his Space Board, Mohole and other activities, couldn't take the leap into the cold water without encouragement. So I purred gently, sympathetically, and finally he said with a hopeless smile "Aw hell, OK, put me on"! (...)

Deg 's Journal, May, 1968

N [ina] and I met at the Museum of Modern Art at six yesterday after discussion with Kluger, of Simon and Schuster. A surrealist exhibition was on. Max Ernst, Nadelman, Matisse, Ram bear up very well. Picasso rarely becomes human enough to excite me. His lines are cold and cruel. De Chirico 's colors seem shabby now. It was a brave moment and said a lot.

We drank beer and ate cheese and crackers in the garden of the Museum, which filled with grey rosy lights as the sun set.

Rodin's Balzac, seen from above, is stern and emotionally stirring. A Picasso She-goat is my great love.

Back at Washington Square, N. prepared a light supper at her place and accompanied me to my work. I talked to Velikovsky at length, recounting my conversation with Richard Kluger and explaining my plans and hopes for the expedition. As usual, he was difficult to converse with but excited more than I've ever felt him to be before. I told him that I thought we should film the El Arish episode from beginning to end. and he was fully agreed. I wonder, or course, continuously, whether we shall find what we are after beneath the town -- the siege evidence and artifacts of Saul's army, the Egyptians, the Hyksos.

I hung up the phone and went to work sorting out materials to be used in my Reader on American Government N. said "Velikovsky can never finish his work." "Nor can I!" I replied. "He has thirteen books to go, when we last counted them. I am as badly off." She asked me what I had to finish: "You have done so much." "Not at all," I said, impatiently. "We do not measure ourselves by other men, but by an absolute criterion of what we might conceivably do." And then I ticked off what I imagined I might yet do:

the publication of my collected papers of the past
the American Government books
another book of poetry
several novels, mostly autobiographical
a philosophy of science
"the new political order"

and whatever would intervene, such as the El Arish story and the government operations study, and who knows what else: editing the *Velikovsky and His Critics* book, for example (...)

I spoke to Sebastian about other matters on the telephone during the day. We are concerned about the troubles that Eddie is having over the custody of the children in divorcing Ellen (...)

Bus told me of a quarrel between Renzo Sereno and his wife one time over a lady, possibly a mistress, of Renzo. "The only reason you like her is because she thinks you're great," declared the wife. Bus and I breathed reverently over this gem for a minute of ATT long-distance time and charges. What has come over womankind? What do they imagine to be the foundation for a man's love and devotion, even charm, even

presence?

After a day of labor selecting readings for my American Government Reader in the company of Eric Weise and John Appel, I entrained for Princeton, snoozing aboard, and arriving happily into the fresh air of the countryside. John, Carl, and Chris were all in excellent mood, the one fixing things on the old Cadillac, Carl playing his Beethoven pieces, and Chris shooting baskets. Mom came to dinner, bringing some freshly picked and cooked wild cardoons.

At nine I biked to Velikovsky's home, Francie loping alongside and for two hours, while she stretched comfortably in the middle of his parlor, we talked and argued over who should do what about books, magazines, and the ever-growing prospect of the expedition to El Arish. Prof. Philip Hammond caught me by telephone soon after I arrived from N.Y.C. to reaffirm his interest. I asked him whether he would, in addition to his usual excavation reports, accept co-authoring of a popular book on El Arish that I was proposing to Simon and Schuster and he accepted promptly. I like the sound of him, though we have not yet met.

V. was difficult. He holds out things and then pulls them back. He wants to do too much himself. I try to take responsibilities off his shoulders and he fights to keep them and even to take new ones. He wishes to discuss every small decision, to control every document. He is elated over our plans but becomes more demanding and even a little more paranoid as events speed up. He has a poor sense of organization and scheduling where other human beings are involved. His own immense mental world can grab and hold everything and shake it out in marvelous patterns, but the world of affairs has its own ruthless laws, that treat all men equally, and that make their own patterns.

Now came time for the Foundation to form and the incorporators met to elect themselves and additional members to the Board of Trustees, and to transact business. R.P. Kramer, L. Stecchini, R. Juergens and Deg coopted Horace Kallen, Harry H. Hess, A. Bruce Mainwaring, John Holbrook Jr., Robert C. Stephanos, and Warner Sizemore. The date was June 2, 1968, a day that would not go down in history.

Deg was chosen President and other preliminaries were disposed of. Then the ill-fated excursion to El Arish, where the capital of the Hyksos supposedly lay buried, was taken up. Everyone knew already that Mainwaring and Holbrook had put up some funds, that a Dr. Hammond had been approached to lead the group, and a contract had been drawn up. Deg set forth a budget, even the minimal costs of which were well beyond the pledged resources of group. Besides the preliminary soundings at El Arish, papers on the "hydrocarbons" of Venus and its temperature changes were to be commissioned, a publication was to be prepared, preparations to receive and use V. 's archives were in order, a magazine was to be inaugurated, and besides there were provisions for work on collective amnesia, dating systems, magnetic polarity, evolutionary theory, the psychology of catastrophe, electromagnetic cosmic models, and the reception system of science. A happy set of prospects indeed, every one of which the foundation was to fail to inaugurate, much less carry on to any extent. The case of El Arish will suffice as an *exemplum horribilis*.

In June, A. Biran of the Israeli Department of Antiquities wrote to Deg saying:

Indeed there is much interest in the archaeology and history of the area but unfortunately it is not always possible to satisfy this curiosity. Even I with all my interest and curiosity have not yet been either to Kadesh Barbea, Mons Cassius, or Qantara...

July found Deg in Naxos, ready to go to Israel if needed, and John Holbrook had gone to Israel to seek permission to begin a site survey at El Arish. Deg is getting a variety of inputs from his assistant:

July 10, 1968

...I spoke with Velikovsky today. He told me that Holbrook had arrived here yesterday. A copy of all the correspondence is on its way to us. The gist of it is that Holbrook saw Biran and Dotan, the chief archaeologist, and that the Israelis would like to see more solid support from Americans. Biran said that FOSMOS seems a bit fly-by-night to them. Another

problem is that they don't want to grant foreigners the right to dig in occupied territory. But apparently they have softened a little, and if they could see something more established in support of the dig, well then... So Holbrook is going to ask somebody at Yale about it, a Professor Popo.

I read your report of the Natural Museum with interest. I will probably get to the Met sometime this week. The figure you described on the one vase are usually interpreted as Amazons, and I am going to compare the costumes with those of the Busiris vase, out of curiosity. I think there is also a book on Greek arms, with should have something in it about helmets.

I am sure you are enjoying Greece -- it's so wild, beautiful, clean and clear...

Meanwhile John Holbrook is grinding his gears in Israel and is addressing a set of marvelously detailed letters to V., a copy of which he then sent to Deg.

Holbrook writes to V. on July 10, 1968:

Now I am in a bit of a quandry. First, I have no reason to doubt Biran's word that the military situation in the Sinai area prohibits any extended work at El Arish at this time. Second, although I shall certainly see Dothan when he returns from the field at the end of the week, I cannot pledge the support of the foundation to the extent of \$50,000. Although we have great hopes for it, the treasury of the foundation is still a bit empty. That being the case, I can only explore the possibility of organizing an expedition to El Arish at some indefinite time in the future (when military situation permits) on the most tentative basis. Much will depend upon what I learn from Dothan. At the very least, I hope that I shall be able to get a look at the site before I leave.

One other matter deserves mention. There is no way of telling the extent to which opposition to your work played a role in the rejection of our proposal. There were other reasons for rejecting it.

Latter Holbrook ventures an opinion on the actual site:

Quite frankly, although I am sure that a complete archaeological survey of the Wadi El Arish and its vicinity might be extremely useful, I am willing to bet that the first trench which is dug in the area which I have described above, the northern quarter of town, will not be found empty or unrewarding.

Little could be done with the El Arish party, upon which V. had set the highest priority (and did for the rest of his life and rightly so, says Deg). The failure was bad enough, but to Deg the most disagreeable part of the episode was the way in which V. began to find grounds for opposing Hammond after he had agreed on his competence and leadership qualities, and had invited him to lead the operation. V. soon convinced himself, and then Holbrook, that Hammond was pro-Arab and would be *persona non grata* to the Israeli authorities, until they were actually approaching the Israeli saying in effect "We know how you must feel about Hammond, but we are aware of this situation and are taking care of it," whereupon the Israeli, in the case of President Shazar, said, "What are you talking about, who is Hammond?"

Deg's Journal, October 20, 1968

Velikovsky and I talked for the first time in a week yesterday afternoon and again last night. He leaves for a grand lecture tour of Texas today. We have counseled him not to go to California to talk, a little later on, because he would become tired and he absolutely should finish *Peoples of the Sea*. He continues to add new data to the work, which is slender still though, like a stick of dynamite.

We argued over the final contract details of *Velikovsky and His Critics*, which I am not keen to do anyway, given my poor financial state and other projects of greater personal importance. He wanted us to guarantee mutually that we would not submit the final manuscript without his approval, in effect. It is of course a perilous idea, for he hangs onto everything and cannot suffer any criticism. I drew up an appropriate missive, but added words to the effect that we would also be jointly responsible if Simon & Schuster publishers sought damages from us for non-delivery of the

manuscript. As I suspected, he balked, and talked of legal formalism. I laughed and expostulated "But you want everything, complete authority and no responsibility!" It is the same with the Foundation we are creating: he wants it to follow his every wish, but does not think that he should be identified with it.

He then said, "All right, Alfred, we will agree just among ourselves, without a paper. You will not submit it without my approval."

"O.K."

And then we went on to argue over the student strike movement, which he fears will undermine authority and disrupt education. "A tiny minority has no right to interfere with the majority who want to study." I told him that minorities are the media of change in any field. I asked whether, if the French students had not rioted in May, there ever would have been the Faure reforms of last week, "No matter!" He would change his mind. I can always win a argument with him on politics, by citing his own case and the history of modern Israel. On these two great contradictions of order, stability, and authority, much of his life is built; they make all of his defenses of authority and majorities vulnerable.

"What do you think of Onassis?" I asked to change the subject. "Who?" Onassis, and Jackie Kennedy. "Oh! I tell you that I think it is a second assassination of Kennedy." Beautiful, I thought, either way. His idea is the same as that of all the maudlin sentimentalists, Kennedy-dead worshippers, the sanctimonious, the suttee-ists. My way, it is revenge for a not too great love, followed by the maddening experience of suffering all of this cant and sick reverence. All of these mass-media addicts were hoping she would end up with a crew-cut college sophomore from Princeton. So she picks the ugly old Greek pirate, and I am personally pleased. The Hollywood and Madison Avenue brainwashed crowds have their fairy tale exploded once again. I know that people live off of these fairy tales; that is what makes valid history and rational politics impossible for them. Perhaps I should feel sorry for the great boobery, but I am diabolically pleased with Jackie 's revenge upon them. And upon JFK too, with his harrowing political life and difficult character and mistresses. What is there to insult in his memory, I ask myself, and what business is it of old ladies and shopgirls to define her husband. "Onassis, I don 't know

the gentleman. Probably they like each other. I wish them happiness." *Basta*.

We returned to majorities and here is how he defined the Jewish majority in Palestine. "Over history, the dead of the Jews are a majority in that country. They live in that tradition wherever they are," Voting the dead to make a majority, like the Confederate southerners do, or the bosses of "rotten boroughs" in the northern cities. *Grussgott!* What would V. say to these majorities and so many others that are alive, as well. But Israel is the *idée fixe*; facts are the dependent variable. Indeed, as I have known for as long as I have known him, the *idée fixe*, the highly conventional, traditional literal interpretation of and respect for the Biblical passages: from this conservative position spewed forth in all directions the most radical theories.

Deg's Journal, October 25, 1968

Reflecting upon the failure of our infant foundation to launch an archaeological expedition at El Arish last summer, I think it may be well to set down my view, which contrasts somewhat with that of Velikovsky and Holbrook. V. was too willing to accept rumors about Prof. Philip Hammond and placed too strong a weight upon adverse facts. V. had no right, as I told him bluntly, to destroy Hammond's possible role as leader of the expedition on grounds that Hammond was pro-Arab and that he had a mistress who would accompany him. Holbrook, whom I regard highly and even warmly, with all his youthful arrogance, was too ready to accept V.'s evaluations and then afterwards the position expressed by the Israeli authorities, to wit, that we could not afford to support the diggings and that the political situation was dangerous. I felt that we had gone so far in our adventure that we ought to have let Hammond himself battle with the Israeli. He might, I think, have outfaced them and dragged in his crew and equipment over their grumpy dispositions. I doubt that we would have uncovered anything of great significance in a few weeks, but we would have planted our flag. We would have moved on from there.

Deg's Journal, November 2, 1968

Met with Velikovsky this afternoon. He is back from a triumphal tour of lectures in Texas. We argued over plans for the foundation. Juergens was present. I asked him pointblank to pull out any materials he might have that others had sent him

and might be used as articles for the proposed journal. He did so. [There was almost nothing.] I asked him also to pull together all his address lists and to let us place a man in his house to built up a list of friends with whom we might communicate. He agreed. I was most pleased. I borrowed V. 's manuscript on *Peoples of the Sea* to read again, and left with everyone in cordial spirits. What a difficult man but what an enormous grasp of everything, intellectually and physically!

I must set some probability theorist to work on some of V. 's proofs. They are strong as they stand in their conventional historiographical form. But an application of mathematics would do much more, e.g. the chances that the Greek letter on the backs of Ramses III 's tiles might be some 'flowing' or shorthand hieroglyphics.

The Foundation spent the fall of the year, following the El Arish fiasco, in some small constructive matters and in self-destructive self-appraisals prompted by V. 's misgivings, Ralph Juergens addressed the Board of Trustees extensively on November 13, writing *inter alia*:

1. ...He [Velikovsky] is concerned that funds collected, as it were, in his name, as gifts intended to further his own researches, will be diverted to other purposes. Among such other purposes he includes such FOSMOS projects as the Institute in Connecticut, the journal *Cosmology* (...) To the doctor 's way of thinking, only two projects thus far discussed would be legitimate applications of such donated funds: a) the El Arish dig, and b) the hiring of Princeton graduate students to carry out library and/or laboratory research under his direction.

2. Dr. Velikovsky is aware of our plans to launch a direct-mail campaign early in January and he is offended at not having been consulted in the preparation of mailing pieces. (...) He insists, at the very least, that literature sent out make absolutely clear to the reader that he is not the power behind the foundation and that he will not be a recipient, direct or indirect, of any funds collected by the foundation.(...)

It seems to me... that some rather fundamental misunderstandings remain to be cleared up, not only between Dr. Velikovsky and the Board of Directors, but perhaps also among members of the Board. In the first place, there is confusion as to the purposes of the foundation. It may be that

Dr. Velikovsky has never seen a copy of our by-laws, which seem to make the point that the foundation is to serve as a clearinghouse for a variety of information, not all of it necessarily related in any obvious way to Dr. Velikovsky's work. This would appear to leave us free to tread ways not yet probed by the Doctor. And of course we thus face the danger of becoming what Dr. Velikovsky would call a clearinghouse for cranks. But our statement of purpose at least broadens our horizons to the extent that we cannot think of our organization as a 'Velikovsky' foundation.

Or can we? The confusion seem rooted in the fact that we members of the Board, almost to a man, have been brought together through our common desire to see his work get a fair hearing. Do we really intend to operate a "Velikovsky" foundation in spite of our more abstractly stated purpose? If so, we must accept certain consequences, e.g., foregoing a tax-exempt status and placing absolute veto-power-quite properly - in the hands of the Doctor. If not, I suggest that we make haste to disillusion ourselves and Dr. Velikovsky.

On November 22, Deg writes a harsh letter to V.:

November 22, 1968

Dr. Immanuel Velikovsky
78 Hartley Avenue
Princeton, New Jersey 08540

Dear Immanuel,

As you have no doubt expected, your succession of favorable and unfavorable comments concerning the progress of the Foundation has created a crisis of morale among the Trustees. For years you longed for just such an organization to dedicate itself to the testing and propagation of your theories, and now that we have constructed it you are undermining it.

You trust nobody, delegate nothing, and have, partly therefore, no capacity for administration. You also do not wish anyone to speak in your name but wish help to drift down like manna to dispose of as you desire. Actually, we shall be trying to do both things -- administration and help in spite of you, if you do not disrupt the process.

The Board of Trustees has unanimously pledged itself to an independent course. Whatever the Board of Trustees believes to be useful to the advancement of science, it will seek to foster. It cannot bargain with anybody. If it chooses to do one thing rather than another, it does so, not out of friendship to you but out of respect for the work that you and others like you have done.

In order to make demands of others, both inside and outside of the Foundation, I have to make demands of you. You should cease making accusations against the Board, even if only among the inner circle. You should cease bargaining over your Archive and the materials that you do not intend to personally use, and let the Foundation work with a copy of them as soon as it can arrange to do so. You should accept what we can offer you (or reject it) in good spirits, knowing that we are doing our best in a complicated setting over which we do not have complete control and that some times we must obtain indirectly what we cannot gain directly.

The men on the Board are your friends. If you have better ones, let them step forward and we shall welcome them. The men on the Board are not the best scientists in the world and, if you know better ones, we shall welcome them too. The Board has to finance the Foundation's activities in whatever ways it deems appropriate. If you have the names of persons who, you believe, might contribute to its work, we shall be happy to receive them. If you wish to reserve the names of certain individuals or groups for your personal solicitations, please let us have their names and we shall not approach them, whether in your name or in the name of the Foundation. If you disagree with the policies of the Foundation, we would value your opinions. But you cannot have a veto over anything that the Foundation does.

If you do not wish to relate to the Foundation in all of these ways and want to dissociate yourself from the Foundation, I believe that you should do so, either by a personal advertisement in a journal or by letter to all those of your acquaintances who matter. I shall then put a resolution to the Board to the effect that the Foundation will go ahead with its philosophy and plans. If the vote is positive, we shall go ahead with its philosophy and plans. If the vote is positive, we shall go ahead; if not, we shall dissolve the Foundation, an action which will disappoint me and give me immense relief at the same time.

Of course, if you do not desire to take any such measures, I would assume that you are basically pleased with our work and will work in tandem with us.

With warm personal regards, as always,

Sincerely,
Alfred de Grazia

V., Deg learned from Elisheva and Ruth, was upset. Then he proceeded to put some of the blame upon Juergens, where it most certainly did not belong.

Dear Ralph:

Yesterday morning, as you know, I received a rude letter from de Grazia with unfounded accusations and it shocked me. Suspecting some provocation, I called you. You disclosed to me that already on November 13 you have sent a memo to him and to the members of the Board of FOSMOS. Next I was surprised to read the memo and its content being your interpretation of a discussion we had at one of our meetings. I wonder why you have not checked with me on the correct presentation of my views or at least mailed me a copy of the memo. Giving it yesterday to me, you gave me also a covering letter. Your intent was good -- you must have suffered observing that I am under wrong impression based on oral declarations made to me, whereas the Board assumes a different policy; and it is good that you brought the situation into the open.

Your memo, however, is full of inexactitudes; knowing you for pedantically accurate, I wonder at your rendition of our conversation. The only explanation I would know, is psychological: your opposition to the idea of the Foundation -- or only to the dichotomy (you use the term 'duplicity'), and that can be a subconscious urge during your writing. (...)

The sentence in your memo that obviously outraged de Grazia who repeats it is "veto power." Nothing of the kind was spoken between us or between anybody else. There is a wide gulf between a "veto power" and being kept in the darkness, as several instances in this letter testify. (...)

If time permits, I shall also put in writing what I exactly expect from the Foundation. As to yourself, you know how I value you; you are also at this time the closest. To you I always opened all my files. I wish you would be the one to organize my archive. I never promised Alfred anything concerning the disposition of it, though we discussed its lodging at Princeton University. Most offensive to me is his reference to my "bargaining" I never responded to his many approaches...

Juergens then writes to Deg and passes along a never-sent but typed letter to Deg from V. with the hand-written notation "This transcript of a letter drafted was not mailed nor typed -- it dates from probably 1967. I.V. November 26, 1968."

Dear Alfred:

Yesterday evening when I was already preparing for sleep I had your telephone call. Elisheva listened too. You told us of your plan to incorporate a foundation for studies in modern science. At your last visit about a week ago you first mentioned of some step taken by a partner of yours to charter a search along the lines pioneered in my books, thus to exploit possibilities now neglected because of the inertia or ever opposition of scientific groups or the entire scientific establishment to new approaches and especially those embodied in my work. You told me yesterday of the founding committee that you intend to convoke in a few days -- two names out of the business world, unknown to me, but also Livio and Ralph, and a few more. You indicated that I should at some point assume honorary presidency of the new venture. A new publication should be one of the projected activities. Organizing of my archive, another project.

I was through with my sleep at 3 a.m. when Elisheva that did not yet fall asleep came to discuss the project. Her thoughts and mine (crystallized by the sleep) were very similar.

The positive in your plan needs not be recapitulated by me for you. But here are the adverse conditions.

For over a quarter century, since 1939, when I came to this country and dedicated my time to research in ancient history, I carried the material load of existence and study and writing with their concurrent expenses entirely by myself. This, at the end, gave me great satisfaction since alone and a stranger in the

land facing since 1950 the concerted opposition of faculties, scientific societies, and scientific publications, I now find myself in a changing climate, even though animosity in some circles, or among some individual is even more vitriolic than before, but this can be recognized as defense mechanism.

Should your Foundation and money drives be instituted, the following will occur:

1. My adversaries who tried to present me as a charlatan but could not point to any improper action on my part, would be supplied with ammunition -- a money collection [sentence unfinished]

2. Scientific organization like American Philosophical Society or scientific publications, like Science of AAAS show recently some change of heart; this mimosa-like attitude would be very sensitive to any activities [sentence unfinished]

3. Also many of my friends and followers would experience some shock if they should feel that a monetary pursuit under whatever guise accompanies my work and I would feel embarrassed.

4. I am most averse, even afraid of being made affiliated with other, so numerous, unorthodoxies. Through these years I am under an incessant barrage of such proposals to study the works of others, and in some instances what is known as lunatic fringe. The Yale Scientific issue caused a flow of letters to the editors from various individuals with appeals to have their theories given similar handing to that given to mine. I found often in letters given claims that the writer is in the possession [of ways] to prove me right (as if I failed in this) or to improve my work by modifying it.

There are, no question, other worthy unorthodoxies. But I wish to continue my progress not burdened with the defense of others, like say, the organon theory of the late W. Reich. A foundation for studies in new [word missing] cannot close door to new ideas; I, however, cannot and wish not to become a pope all malcontent.

5. Organizations, like foundations, from the start or after a while, institute salaries, incur liabilities, oblige itself [sic] for grants etc., and should the organization be intimately connected with my name, it may disband under conditions of insolvency,

after a promising start, causing an irreparable damage to my cause.

6. The small organization of Cosmos and Chronos groups is given to my close supervision and I fell quite comfortable in separating my scholarly pursuits from the work assigned to Cosmos and Chronos extending it to [sentence unfinished].

I know that S. Freud and to even greater extent C. Jung made use of donations, usually by their ex-patients, to establish schools of their respective modes of psychoanalysis or for publishing magazines. But their activities were not in the form of solicitation of funds.

In the morning after your call I drafted this letter to let you know how I feel.

Deg ' s Journal, November, 30 1968

Yesterday was one of those fine mornings when most things seems to go wrong, but I didn ' t much mind. The mail brought a batch of documents from Ralph Juergens -- the gist of which was that Velikovsky was deeply perturbed by my ascerbic letter to him of ten days ago. V. had promptly asked to see Ralph ' s memo describing V. ' s thoughts. Then V. wrote a letter indirectly answering mine, and implying that Ralph has misstated his position, etc. V. added a newly typed version of a letter that he said he had once written me but never mailed, full of forebodings concerning my establishment of the foundation, together with a letter from Arens of Gimbel ' s of Philadelphia, also full of doubts about the wisdom of proceeding with a foundation. All of this was to justify V. in the face of my attack. I know V. ' s pattern of responses so well now that I could tell there was nothing new about the whole business. He writes everything down to have it on paper for some future strategm. He warns against everything to be ready to be proven a prophet should things go badly. He cannot let go of any power over things or people, but plays upon every means of entrapping and embroiling them, sucking them in and pushing them off as he feels the one way or the other in his succession of mobilizing-for-action and trust-nobody moods.

I phoned him and visited him in the afternoon. I brought him the copy of *Etruscan Tombs at Sesto Fiorentino* which Prof. Nicola Rilli had inscribed to him, and he surlily carped at every point of Rilli ' s development that I brought out. ' Very risky, '

'I don't think much of him from what you tell me.' 'He does not seem to be a scholar.' 'He has very little evidence for what he is saying.' We finally got to the sensitive subjects of the flurry of documents. He claims his position has never changed. I said, 'Very well, you need not have anything to do with the Foundation, but if you wish to write articles for it or refer people to it, or receive support from it, you are welcome.' He agreed. (He will of course not keep his agreement, but will intervene at every opportunity.) I offered also to turn the Foundation over to him completely and let him designate someone to carry it on, but he refused that. I said, 'Please name those men and foundations whom you do now wish us to approach for support.' He would not do that. I promised that his name would not be used in support of the Foundation, which satisfied him. I know what he would like to see happen: the Foundation helping him in every possible way, but he criticizing it constantly for its faults. And provided it does not demoralize others, I do not mind. I have from my first meeting with him concluded that I should do what I thought he basically would want and weather as best as possible the glooms, the negativism, the wounded shouts, the suspicions, and the ingratitude.

We drank a glass of dry white wine (the Israeli wines are becoming excellent), and he showed me a few late letters, as he usually does. With some emotion he declared that, for all I have done for him he was going to give me sooner or later the whole history of the case -- the reception of his ideas by science and the public. I didn't feel as grateful as I should, for I need nothing so little as another pile of documents and a book to write, though it be the richest such case archive in history, and I thanked him. I prepared to leave, bidding Elisheva goodbye, and he stepped into the next room to get something. When he came out. I stepped close to him and said 'You know, there is nothing that you can do that will drive me away.' He said 'I will read you a line of poetry that you wrote' and quoted "the most opposed I will most believing be." 'Not a bad line,' I said, smiling, and bid them goodbye again.

Deg's Journal, December 1, 1968

The Foundation Trustees met today and perused the volume of recent correspondence relating Dr. V. to FOSMOS. They agreed that his conduct was sick. Still Juergens and Stephanos are under his thumb. I pointed this out and questioned whether

the Foundation should not slow down its program for a year until everyone clarified their position, especially Dr. V. But we decided to move ahead anyhow, and suffer V.'s conduct as well as possible.

The more I think of his behavior, the more indignant I become. Every kind of evidence comes out in his letters, actions, and the experiences of others. Today he told Juergens that the Foundation should get another box number, because he wishes to go ahead with his absurd, presumptions, and self-glorifying Cosmos and Chronos 'Clubs' (of which, in truth, none exist). Day before yesterday, he tried to buy my loyalty by the gift of his papers and documents on how science received his work. 'only for you, not for the Foundation.' A great collection, but I wish it for others to use, not myself. He is incredibly obtuse on some matters, I try to love him for his faults, but they are too numerous and large to embrace.

On Dec. I, the Board of Trustees met in Princeton at Deg's home, without the important presence of Mainwaring and Holbrook. Nor were Kallen and Hess, who played no part in these proceedings anyhow, present. Juergens carried a new letter from V, to the Board, divorcing himself from the Foundation, which, as he asserts, he had never been married to in the first place but with which he is hoping for good relations nevertheless.

I repeat the following from the Minutes of the Meeting:

"An extensive discussion developed around the subject of the Foundation's relations with Dr. Velikovsky. Juergens reported that Dr. Velikovsky was of the opinion that FOSMOS' aims and activity were to deal only with such work as concerned him directly and as he might approve, and that FOSMOS was changing its direction since its inception.

The President moved that, after examining the record, the Board resolve that the Foundation had not deviated from its original aims, which remain unchanged and are reflected in the following description offered by Stecchini, plus the subjects of 'Communications of Science' and 'Science of Science':

The Foundation is concerned with conducting and aiding in the investigation of theories

- A. That the geophysical and astronomical history of the planet Earth has been characterized by sudden changes;
- B. That these changes have taken place in historical times and, as such are documented by historical records, archaeological findings, mythological traditions, religious practices, and scriptures; and
- D. That these changes have affected the human psyche and Affect contemporary social behavior."

Afterward, Deg addresses V. once more, to tell him that the Foundation agreed with him and had always pursued the course that he now was advocating.

And then Deg receives a rather surprising letter from Stephanos who now becomes the instrument of V. in a new way; he lists his benefactions from V. as if he were under hypnosis, and declares:

...I must state that I find your letter to him [Velikovsky] misdirected (it should, perhaps, have been addressed to another), and in its tone, totally unjust and unwarranted. I believe it could be damaging to the interest we all claim to share, the acceptance of Dr. Velikovsky's work, and capable of great personal harm to him and to his good name.

Since I was privileged to receive a copy of that letter (...) I want and do here deny its content as my experiences allow, and respectfully request, as a member of the Board, that you write a retraction to Dr. Velikovsky as soon as possible...

Deg replies to him:

Dear Bob:

I am afraid that your letter to me of December 5 and the circumstances of its preparation tend to confirm the contents of my letter of November 22 to Dr. Velikovsky.

It also indicates that Dr. Velikovsky should probably not have circulated a personal letter.

But thank you for your concern. I am sure that all will end well.

Sincerely yours,

Alfred de Grazia

It did end well enough, except for poor Stephanos. The Foundation moved along cautiously, doing only small projects such as disseminating materials on the *Velikovsky Affair*, supporting Eddie Schorr's work on the Greek Dark Ages, and soliciting memberships. It was disturbed by a new attitude that V. had taken toward Stephanos, hitherto his most faithful and welcome disciple. He seemed to believe that Stephanos had encouraged persons from the lunatic fringe to become followers of V. and was giving them inside information of V.'s activities and archives. V. wished to dissociate himself from Stephanos and expected the Foundation to do so, too. Sizemore stuck up for Stephanos in private conversation with Deg, who sensed no great loss should Stephanos resign. Then he saw Sizemore's point -- Stephanos should not be sacrificed to V. -- and did nothing. Stephanos resigned anyhow. By the following Spring, Deg was withdrawing, too, as this Journal entry of April 19 seems to indicate.

On occasion Dr. V and I have discussed a biography in dialogue form. But the three occasions on which we went to work with a tape recorder were disappointing to me. He becomes stiff, even more aware of his role and audience, and though I try to break through with my informal comment, he remains fixed like a peasant before a camera.

I have not seen him in several weeks. My own problems with women and children are many and my book *Kalos* cries for completion. Immanuel's magnificent self-centering is not consoling or even rational, under the circumstance. I have ceased completely to work on FOSMOS, in part because of the foregoing, but also because the members of the Board were not up to editing a Bulletin, or raising funds. Bill Dix [Director of the Princeton University Libraries] told me, too, that the Velikovsky's during V.'s illness of December, had sought to give (with tax deductions well in mind) V.'s archive to Princeton University. Yet FOSMOS was to have been the beneficiary.

Holbrook took over active management of the Foundation, working out of his new office in Washington. He did not succeed in developing it well, and, by general agreement, it was dissolved several years later.

V. was doing well enough as his own majordomo as we discover when we read Deg 's Journal of October 7, 1972 in Princeton:

I borrowed Jill 's bicycle and rode it to the Velikovsky 's. Francie, whose memory of me hardly dims with my long absence, loped alongside. Velikovsky was issuing directions to a University representative on how to set up the stage for a forthcoming lecture to the Graduate School Residence Hall Club. He spared the man no detail, prescribing publicity releases, and his desire to have his full first name spelled out rather than I. Velikovsky (is there a wish here to conceal the I, egoist, or the normal desire to spread out one 's own name, as he said?). He requested that all his books and even a copy of *Pensée* dedicated to his work be on sale at the University Store beforehand; asked that two parking spaces be kept for his car and that of his daughter; wondered, since the British Broadcasting Company would be video-taping the show, whether the President of Princeton might not come if invited; denied a suggestion that a local radio station broadcast the speech but insisted that provisions for a televised relay into an adjoining hall be provided for people who could not crowd into the banquet hall. He stipulated that some announcements reach New York and Philadelphia so that disciples might come from those places to hear him. The young bald impresario left the Presence dizzy with details V. is many things but he is also a master impresario. He has had to be; his overwhelming need to be recognized for what he is can only be satisfied by mobs of admirers under instructions which, given his detachment from the Establishment machinery, only he can provide, or by some wonderful stroke of recognition, a great prize like the Nobel Prize, the Fermi Prize, or an invitation from a head of state to deliver a series of lectures. I believe that he would then retire from his promotional labors and give himself over to finishing several important books.

I thought so yesterday as I watched him masterfully, but yet exhaustingly, promoting himself and his work, and later privately conveyed this thought to Sheva, when he had gone up to nap. For when the door closed on the graduate club representatives, he sat back, listened to me for a few minutes,

ate an apple, and began to doze. I enjoyed the chance to talk to Sheva; she can tell me less flamboyantly all that has happened on their trips and where all the characters of the drama of recognition are at the moment -- Mullen and Schorr and Bucaloe and so on. I borrowed a book and biked home to Mom. After dinner, Immanuel called to apologize for falling away from our conversation and I assured him that I was delighted that he could sleep well and hoped that he would always behave in exactly the same way. I had mentioned to him that I contemplated a little book of forays into myth, science and our adventures over the past decade of our friendship; he wondered how I could write it without his archives. I can imagine how I might, but if he would dig into them a little, my work would be greatly improved; I did not, however, suggest that he give me materials. I shall show him the table of contents when it is sufficiently elaborated. Then, if he wishes, he may find some material that would help me.

Deg is living in New York City, and only visits Princeton on occasion now.

Deg's Journal, October 23, 1972

I telephoned Velikovsky at 10 PM to see how he was. He was well. We talked of the book I intended to write. When I said that I was investigating *Hermes* he warned me against starting to repeat his work of 20 years. I guess he'd like me to ask for his files and then trap me into an endless affair. I said, don't worry: I have only in mind making several penetrations in depth, at widespread points, to show the method that should be followed to mine the ore. He said that he couldn't "approve" my book unless he read it. Of course. And no doubt there are some bouts ahead. In general, he likes the idea that I will write the book.

Then I gave him some firm advice. I said "you must finish *Peoples of the Sea* and the *Ramses II* volume promptly and publish them. You must not lecture and run around. Ten people can go around lecturing about you but only you can finish these books. Furthermore, you must not work on the Einstein book, or *Stargazers and Gravediggers*, or *Ash*. These can be finished by someone else. You must write something, if only 30 pages, on your theories of what happened in the skies before Venus in 1500 B.C." He agreed, "You are right!" He added, however, that he must write his autobiography because

nobody knows him really or how he did his work. He only let out a few facts here and there. Alright, I responded, add that to your required list, following the ante-Venusian article. But that 's all. "You 're right!" he said again, with unusual accord. And so we left the matter, saying good-night.

P.S. V. told me that Harlow Shapley had just died at a nursing home in Colorado. After reading the extensive obituary in the *New York Times*, V. concludes that Shapley, always a great self-promoter, had seen to it that the *Times* possessed his own account of his life. Thus Shapley hurls his last insult to V. from the grave.

Again on November 9. Deg exhorts him:

Had long telephone conversation with Velikovsky. He was in a grim mood, I tried to cheer him up. I also read him the list of chapter titles for my projected book. He said a few approving things but generally he was critical, full of admonitions. careful of his own sources of information, making no generous or even modest offer of assistance, wondering how I could have any new idea (though he did not say this explicitly) when he had them all, and in some manner had *published* them all.

I don 't know how he expects ever to encourage serious efforts to follow or parallel him. He beseeches this from the world but then denies in advance that they can either be original or important.

I tell him to move rapidly on his theory of the pre-1500 catastrophes -- to publish at least a synopsis of it, lest he accuse even his supporters of plagiarizing him. All I know of this work are a few remarks of John Holbrook relating essentially the truth of the Greek theogony -- Uranus, Chronos (Saturn) Jupiter.

I am telling V. that if he doesn 't do something soon here instead of parading around the country *he* will become a successor instead of a predecessor of someone else, Further, *his* predecessor will probably do a poor job because V. has withheld his information and assistance.

And he is concerned whether V. will be elected to greatness:

Deg ' s Journal, November 72

I.V. is running for election. The office he wishes to achieve is premier of 20th Century Science. I believe that he has as good a chance as anyone up to this time of winning the election.

However, I am not a campaign manager. And though an election in science is unfortunately like a political election -- in that a campaign biography should be written that will show the candidate in gorgeous lights -- I feel I must pass up the chance to win glory as a publicist. My interest in biography is as Conant [President of Harvard University and chemist] once put it: to find the full meaning of science through its means of creation.

Immanuel V. as I see and know him is here, and you must understand to begin with the fact that no person can fully know another one.

Problems of health depressed V.:

Deg ' s Journal, December 22, 1972

Called V. He is gloomy, The doctors told him that he must go away to rest. His days are full of calls, visits, correspondence -- too much to handle; his writing lags. I invited him and Elisheva to New York for a day of rest and walking around the museums. Maybe. I also suggested he might go to Yucatan and see the ruins there. He doesn ' t "want to be carried around by the tour buses." "Let the buses go without you. Stay at hotels. Then provide and make your own daytime itinerary." He wondered when I would be in Princeton. I didn ' t know, I told him I would think of what he should do and would call him back .

The "Apollo" Program suffers severe cutbacks;

Deg ' s Journal, December 23, 1972

Called Stecchini. He is feeling better after a gradual six months ' recovery from an old back injury. He said V. may be depressed by the closing down of the Apollo Moon project which, whatever its premises and procedures, had brought forward some support of his views. The signs of volcanic activity are still being reported, though their time of occurrence

is naturally placed conveniently far away -- 100,00 years, 500,000 years, their freshness suggesting "recency," but recency being defined arbitrarily on the lengthy geographical scale. If 100,000, why not 3000? No answer. No question, in fact, by anybody, save the Velikovskians. Cape Canaveral (Kennedy) is already being dismantled. The scientific community did not rise to the occasion, said S. "I didn't rise, either," I said. "It was a great waste of world resources." He half agreed.

Deg worries both about V.'s health and his attitude towards a friend:

Deg's Journal, December 26, 1972

Called V. again yesterday. He is more cheerful, but says his diabetes is moderate, not light. He is grumpy over the stricter diet he must follow. He asked me about all my children and I recited their whereabouts and conditions of life. He asked whether he could help me. I should have said, "Yes, let me read your pre-Venus notes and correspondence." I didn't. He wouldn't; not now. He would ask me to show that him all of my ideas. I would do so, but he might well not reciprocate and even though his materials must be better than mine on the whole, he might very well absorb them and simply look the gate on me by putting me onto this or that matter stretching on endlessly. He cannot help himself. He is authoritarian. And he finds it difficult to think that anyone in the world but himself can supply anything but a few details nor indeed should until he has breathed his last word. This kind of game seems bizarre between friends, but the reason I am perhaps vulnerable to shock by its exposition. As certainly as the sun shines (sic!) he would reject my work repeatedly, absorb all that he had not known, and accuse me in the end of plagiarism.

V. begins to exhibit alarming symptoms:

Deg's Journal, February 10, 1973

Velikovsky Visit-V. not well at all. Extremely nervous, thin, paranoid cryptic references, taciturn jerky movements from time to time. Is diabetic. Asked him whether 10 years of good work might reconstruct 10,000-600 B.C. He didn't have an opinion. He said he doesn't know whether deluge was 4000 or 9000 BCE.

Deg's Journal, February 1973

Called Velikovsky at 5 P.M. Says he is feeling better, but is having troubles with "people." Has matter of importance (ominous tone) to talk over with me. If I want to hear it, I must come to Princeton tonight. I tell him it is difficult. Won't tomorrow night do. Maybe. "Who is it?" I ask. "Can't I help." "You come." etc. All remote, intimations of disaster, confusion of personal and the world and of all past with the present. I try to talk of article about Mars. 'The author believes in all miracles except yours.' He's not sure he read it. But uninterested really. He is involved in his personal huge caravan of suspicions, lawsuits on his house in Israel (so Ruth tells me to make clear his references), forebodings of catastrophes, possible suicidal impulses (my enemies wanted their martyr; now they have it.) Nina hands me a note as she overhears me. "Do not try to get abstract conversation. He is trying to talk about himself." But he is uncommunicative. Finally, I leave it that I may come tonight or in the next couple of days. He is reluctant to close but finally I end the call.

Called Ruth Sharon. Father not feeling well. Diabetes out of control. She tells me not to go to Princeton. He will be better and there is nothing I can do. I tell her I fear he will regress irretrievably. She cannot answer to that. She says he may even resent me later if see him in weakness. I tell her I am more concerned with whether he will be helped now if his situation is serious. Maybe she and her mother cannot suffice to pull him out. I ask her to call her mother and if they want me to come to call me.

8. p.m. Ruth calls me back. She has talked to her mother but her father hung onto another phone throughout the conversation. She says, however, that he was feeling a little better and was thinking of driving out to purchase several articles. So I should call and give my regrets for not coming.

8.15 I called V. Sheva came on the extension phone. I said I had not finished my proofs that had to go to India and asked him to excuse me if I did not come this night. He assented. I said further that I did not wish to see him before I could show him an outline of my work on pre-history. He replied that he would have no time to read it, for he was so behind in his reading. Sheva interrupted gracefully to say that it was short piece and I hastily agreed, saying that it was only a page or so.

He said nothing then; I uttered a few additional inanities and hung up with the promise to see him soon. He sounded at a bit stronger of voice.

V. then recovers:

Deg's Journal, April 4, 1973

I phoned V. this morning and found him much improved since my last call before leaving the country. Three weeks in the hospital had somehow restored him. I said, "Life without a telephone to bother you was good for you." "No I had telephone. I took my calls."

Anyway, he is better and will drive perhaps to Youngstown, Ohio, for a speech next week. He is working of Ramses II again. He is pleased that Carl Sagan is writing an article for *Pensée* on Venus. He agrees that I shouldn't bother with book reviews for *Pensée* but should present a significant paper. Maybe I shall get down to preparing one.

He is hopeful. He speaks of Particular tasks. He has even begun rearranging some files. It is a great relief.

Bill Mullen is getting ready to move from Princeton University to a new appointment at Boston University. He is glad to be away from V.'s moods. He writes to Deg:

August 12, 1974

...The summer has been curiously unproductive and jammed as far as Velikovsky is concerned. He has spent virtually all his hours talking about what he is not accomplishing and bemoaning the magnitude of the battle against his enemies on all sides. I've contributed only bits of help here and there, otherwise being forced to concentrate on preparation of this fall's course. Eddie [Shorr] has been of tremendous help, spending day after day in the library going through *The People of the Sea* with a fine-tooth comb. But here too the result has not been of the kind to cheer Velikovsky up since Eddie has found many minor errors which need correction. Nothing that shakes the reconstruction, just a lot more nitpicking work that really has to be done if the book is to be spared the dismissals by Egyptologists on the grounds of inaccuracy which are feared. In short, be thankful for the serenity of Naxos. Al, since little

would have been gained by being close to Princeton this particular summer (...)

But V. reorganizes his forces and this time calls upon Irving Wolfe, who graciously responds by addressing Mullen, C.J. Ransom, Juergens, Rose, Steve Talbott and Milton:

Dear Alfred,

I visited Velikovsky last week, along with Lynn Rose and Earl Milton. We discussed several matters with him, among which were

- the number of books he 's working on at once
- his archives and related issues
- he wants people to submit and keep submitting articles on or arising from his work to scientific journals, whether they will be accepted or not -- setting up a Newsletter, about which several steps are being taken -- public recognition for advance claims and theories.

You will be familiar with most of these matters already, but I 've drawn your attention to them because I think we need to get a number of people thinking about them and coming up with solutions because Velikovsky can use help in all these areas.

With regard to the last item above, here is an example -- the recent discovery of substantial quantities of argon and neon on Mars seem to puzzle scientists, as an article in *Science*, June 21, 1975 indicates. Yet Velikovsky predicted argon and neon on Mars as far back as 1946. Key scientists must be given the facts -- dates of original advance claims, letters, confirmations, etc. -- and urged to write the major scientific journals. Velikovsky feels he 's too busy to do this himself each time, and so I 've offered to handle it for him, telling him, telling him that, wherever a case like this arises, he 's to send the relevant document to me and I 'll compose a covering letter and send it all out to the right people.

This is where I need your help -- I want to make up a master list of key people, perhaps divided into two or three categories, to whom such things can be sent as each occasion arises...

Deg could imagine the huddle at 78 Hartley Avenue, planning the counterpropaganda campaign, the "truth squads" as the Republicans and Democrats had come to call their counterpropaganda teams. Next year, Wolfe was calling for an "alarm system" which he had worked out with Milton in Canada. It was to be a network, highly sophisticated, with members divided into generalists and specialists, with squad leaders who would call upon their assignees to respond to the alarm. Wolfe had been called by V. to activate the system, as he had promised the year before, and V. nominated as a test alarm the publication by Doubleday of *Immanuel Velikovsky Reconsidered*, which should exercise the network to produce reviews, letters, and public discussion.

This meant helping the Talbotts who were otherwise blacklisted by V. and several of his circle. "Regardless of what any of us feel about the Talbotts," wrote Wolfe, "I agreed because Velikovsky asked." (Actually, I doubt that Wolfe ever felt antagonistic towards the Talbotts himself; the plea was for others.) "He (V.) may feel that he wants to aid the success of that book because it will affect his own case." So the Talbotts and the inner circle were momentarily in bed together again, an event that had not occurred since the Talbotts' *Pensée* had collapsed. The results were not remarkable, and after a time they got out of bed.

There came a lull in attempts at general organization; V. continued to turn his attention and the minds of his several collaborating followers to the AAAS affair, a story to be told later. It is noteworthy how much time was taken up with all the maneuvering, research, writing, and wrangling connected with a single sitting of an AAAS panel in San Francisco, much of five years of V.'s time and of the time of several others, the time too of Elisheva, but who counted that? -- more hundreds of hours blanked out; there the tragedy is marked, for she was a sculptress and musician of consequence.

She never complained, so I am reporting Deg's complaints on her behalf, unsolicited. Moses would have been pleased with her self-sacrifice; Deg was no Mosaist. When she lay dying after a long illness, and he had not seen her for months, he thought to write a

poem for her.

Then came the infatuation of V. with Christoph Marx, and following upon Marx' return to Switzerland, V. addressed Lynn Rose, who was perhaps feeling both grumpy about the affair and pleased that suddenly V.'s attention was turned elsewhere. However, V. was writing in a euphoric mood, and one could see the alarm bells ringing around the world.

The letter to Lynn Rose is dated May 11, 1977, and I summarize it.

Marx was to be "a central figure" on the European continent: Isenberg sends a paper he gave to a conference of science editors and V. urges him to send it to the major hostile magazines -- *Nature*, *Science*, *New Scientist* and the *Bulletin of the Atomic Scientist*, "as coming from the convention" ... A letter from Langenbach, a supporting attorney working in the Harvard scene... A call to William Safire of the *New York Times*, a self-designated "great fan" to get advice... An announcement that Juergens has resigned his engineering job and would probably now work for him, V... A hope to teach a course in Egyptology at Princeton University... A report of Deg's taking issue with Lustig of the *Encyclopedia Britannica Yearbook*... Last minute changes to the English edition of Ramses II...A carpenter-mason is building a room for guests and Elisheva's music... A letter from the widow of maligned Harvard supporter, Professor Pfeiffer... Mainwaring will be sending a complete file of all C14 communications with the British Museum and the University of Pennsylvania museum... A conversation with Holbrook, once more in Washington... A gift of Czech rights to Jan Sammer who helped so well with Ramses II... Some minor foreign rights also to his early copy editor Marion Kuhn, now ailing... Reporting plans to sponsor publication of Alice Miller's Index to his works... Detailing the distribution of 1000 free copies of *Kronos* to College libraries, financed by Jerry Rosenthal... Denouncing Steve Talbott for recommending in a pamphlet that all subscribe to *The Zetetic Scholar* which has recently defamed V... Urges that the five former associate editors of the now defunct *Pensée* "should make a common statement and try to teach the subscribers of Network (Talbott's serial pamphlet), deluded into believing that the Network is an organ to defend and

protect my work... Dr. Gowans of the University of Victoria "comes back to the fold" after consorting with the likes of Dietrich Muller of Lethbridge... An exchange of letters with Jacques Barzun... Reports that *Peoples of the Sea* just released had already outsold *Earth in Upheaval* (11 printings since 1955) and *Oedipus and Akhnaton* (12 printings since 1960)... He resists Doubleday's efforts at putting *Peoples* into a book club as an alternate selection... Ramses II is to be delayed once more, this time by the publishers... He is happy that his British publishers, Sidgwick and Jackson, have given full prominence to his *Peoples* while somewhere in the nether pages "Patrick Moore is modestly displayed for his '1978 Yearbook of Astronomy,' and has to take this pecking order, he being the author of 'Do you speak Venusian?' presenting me as a King of Fools"... More letter exchanges... He doesn't want Rose to be distracted from their plan to write together "The Grand Ballroom" dealing with the AAAS affair which was already the subject of several books and many articles... "....The hammer of the builder sounds like a song... do you know that my real vocation is in architecture, and the years that I visited the Library on 42nd street, I regularly visited also the room with architectural journals, watching for a chance to compete for a plan and construct a public building?"... "Keep well, act strong, Lynn."

V. was obviously in fine fettle. The Mastermind was back. He had a great deal going for him on two continents now, it seemed.

The euphoria subsided. The resistance to all of his ideas continued unabated. It seems that he could say nothing that would be right in the eyes of his opponents. His growing disenchantment with Christoph Marx was not compensated by new faces. (New ideas were out of the question; proofs were wanted, and defense.) He had now close to himself principally Greenberg and Sizemore; for them *Kronos* was not fun and games anymore. On June 3, 1979, Sizemore writes Deg, "This issue is going through hell -- trying to get V.'s approval on Lew's article about the latest probes."

By now I believe that you and I Know enough of the principal

characters here to venture a more fundamental answer to the question which I dealt with unsatisfactorily at the beginning of the chapter: why did Deg stick with V.? It appears that the two men were close to each other even when separated and out of touch. I conclude that there was a familial relationship being reenacted between V. and Deg. It was not father to son, but older to younger brother. In significant ways V. was of the character of Deg's older brother Sebastian, and Deg was relating to him as he had to his brother throughout life but especially from two years to twenty years of age.

It was as Lasswell somehow discovered, a sibling rivalry between Deg and Sebastian, more intensely activating for the younger than the elder. No matter what Sebastian did, he couldn't put down his younger brother; and his younger brother, while trying to outdo him, was absolutely fond of him and set him up as a model for others, to be surpassed only by himself, and he was determined all the while that none was going to put down Sebastian so that there was a strong protective impulse going incongruously upwards -- material and demanding -- rather than downwards as one might expect.

V. had two older brothers, neither of whom he saw after 1921 and with whom communication was rare, if only because the "Iron Curtain" barred East from West and he said once to Deg, speaking of his scientist brother, Alexander, I would not want to jeopardize his position over there by reintroducing myself into his life.

And Sebastian and V. were of the same rawboned, tall and handsome physique, unlike Deg's more compacted form and features, both were umbrageous, too Both felt that Deg could do anything he set his hand to, but that he was always off on some wild goose chase when you needed him.

There were of course differences. However the song goes: "I want a girl -- just like the girl -- who married dear old Dad," no girl is ever quite like mother: and so with siblings, no two sibling relationships are quite a like. The major differences were two: like Deg, V. was fantasmogenic: he day-dreamed much and often and duelled with the universe of nature and men in his mind. Sebastian

was not a dreamer. And, further, V. was there, in place, at home; for seventeen years Deg knew where to find him at Hartley Street whose number he could never remember, and that he would be welcomed like a brother, which, no offense intended, he could not always count on from Sebastian.

I think that the crux of the relationship, that which proved its psychogenesis, was the fact that Deg, unlike so many of the cosmic heretics, could be constantly critical of V. without risk to his affection for V. Then, too, while V. would never let Deg take away his toys, nor admit that he was equal, he would not stop him, short of outright usurpation of his position and place, which Deg in any event would never wish to do. Indeed, one of Deg's main virtues and weaknesses in human affairs, if it can be called that, was that he would often win a contest, but could never administer the *coup de grace*. Neither V. nor Sebastian lacked this capacity except in the case of their younger brother.

Sebastian never became friendly with V. but supported him quietly, just as he never committed himself to Deg's efforts on behalf to V. nor to Deg's quantavolutionary ideas. He engaged himself mildly one time in their futile effort to obtain an honorary doctorate for V. at Rutgers University. Another time, when Deg was abroad, Sebastian perhaps prompted by his wife Lucia, thought of getting V. and Elisheva together with the Director of the Institute for Advanced Study, Carl Kaysen, Ambassador George Kennan, and their wives. Perhaps V. should be invited to join the Institute (which would in fact have been an ideal place for him and ideally in keeping, too, with the Institute's professed aims). Elisheva and Immanuel were irritatingly preoccupied with the menu for dinner, however, and settled finally for a visit during the cocktail hour, which went off nicely.

Deg's communication lines generally thinned out in the years 1976 to 1983. Even his lateral communications in quantavolution dwindled as he pressed to break through with the several large studies underway. Here he is writing from Naxos to Professor Ernst Wreschner in Haifa on December 21, 1976:

"I am returning from three weeks in Mexico as a guest of the government. I attended the inauguration of Jose Portillo as President, gave a paper at a special conference on the 400th anniversary of Jean Bodin's Six Books of the Republic (author of my least favorite doctrine -- absolute sovereignty), and visited a number of Olmec, Maya and Aztec ruins and sites. It has been a good trip and I found a considerable interest in translating my political works and even some surprised involvement in my questions about mythology and catastrophes. I did not find the lost tribes of Israel but perhaps learned something of pre- "Atlantean" survivals. I also had a car wreck (I was not driving), had my wallet stolen by a large fat Indian lady with an overpowering smell that put me to sleep on the bus alongside her, and then later on my little camera as well -- before I could turn around, the pickpocket had dived into the marketplace mass.) C'est la vie.

With luck, by late spring I shall have a general manuscript ready on the holocene destructions and human development and will send you a copy. I hope that my present letter finds Ella and yourself very well and in good spirits. I have resigned all teaching at NYU and am now free to give my time to research and perhaps sometime to a visit to Israel, unless you meanwhile visit here. (...)"

Deg showed his materials on *Homo Schizo* to Harold Lasswell who approved their significance. Deg wished he might get the famed polymath involved in seeking the origins of the human mind, even in contemplating quantavolution, for Lasswell was as much a fantasmogene as Deg. But not long afterwards, Harold Lasswell climbed into the bathtub of his apartment overlooking Lincoln Center, suffered a stroke, and spent two helpless days in the tub before his apartment was entered. His friends rallied around and attended the cheerful but addlepatented great man until he died. Deg hoped he had not been unkindly critical when they had last been sitting at Lasswell's place, drinking whisky and looking down upon Manhattan, for he had been suddenly seized with impatience when Harold spoke of a great new understanding overcoming the medical profession owing (by inference) partly to the introduction of techniques for better human relations in complex technical situations (in which he was playing a part, as always) inasmuch as Deg felt like raging -- not only against the system of medical care,

but also against the world at large for its frightful bungling.

When I went back in time for Lasswellian material related to quantavolution and the heretics, the latest was from November 4, 1972, when Deg 's Journal reads:

I met Harold Lasswell at the University Club 7 and after two Scotches and 'what have you been up to' and 'what are families and friends doing,' we taxied to Washington Square, where Nina prepared dinner. She pulled out all the stops of her culinary organ and enthralled Harold with poached whitefish and freshly made mayonnaise, stewed hare, spinach and egg salad, Port-Salut, stewed pears in brandy, and a variety of wines and cognac. We talked until after midnight.

He is looking as he has for thirty years. Still grey and pink, still ranging all over the world and talking upon every subject; the chasms of unintelligibility when he swings into Lasswellian sentences from time to time still enchant me. It was Nina 's first exposure to them and she couldn 't decode them.

He described his unexpected walk many years ago up a set of 18-inch spikes hammered into the walls of Santa Sophia in Istanbul. He had a hangover from a night of drinking sweet Turkish liquor and could barely save himself from nausea, vertigo and panic. How I know the feeling. He talked too of a ride in a military plane from Paris to Vienna after World War II, where he sat on a metal bucket seat with two other men and watched a cargo of coffins creep through their bonds toward the freedom amidstships.

We talked of economists and he expressed his pleasure that the social sciences were being recognized for Nobel Prizes, particularly Ken Arrow and Samuelson, but his subtle manner of speaking, which one must watch carefully, indicated he was a little hurt that he who had achieved so much for the social sciences had not been recognized with such a prize. I agreed with him, without mentioning the matter; what a corrupting influence the Nobel Prizes are; they pretended to omniscience, in whose name, on what grounds; what presumptuousness.

He is now working on a Policy Sciences Center, promotes a world university, heads a Rand Corporation Board, etc. He was delighted with my stories of the University in Switzerland and would have gone the whole evening on the subject.

His mentioning Arrow and Samuelson came when I reflected upon the betrayal of human economics by the economists. I explained my struggle with Scott-Foresman over publishing a chapter on economic policy and especially on a guaranteed income. Harold says that A. & S. and others just published a statement indicating their adherence to such in principle. I should use it to back up my attack on the subject.

I mentioned my advice to Velikovsky to publish now instead of awaiting the 'no mistake' nirvana; H.L., who feels a certain competition, insisted that I was right, that V. wanted to be God, that it was unscientific, that no man could expect his work to stand free of error indefinitely, that the courage to err was the glory of a true scientist.

Lasswell spoke of a book called *Chariots of the Gods* by a Swiss, who apparently believed in the depositing of inventions upon Earth by superterrestrial beings. I thought this was a modern version of the gods of the Greeks descending at will upon earth bringing discoveries as well as evil. I added that I am pursuing a theory that the flowering of certain early metal ages came in consequence of the showering of metals upon earth from comets and meteorites.

Probably I should add a chapter to my book on the descent of the Metals. If the metals are heavy, they should have sunk to the core of the Earth's molten mass, never to surface again. Why should in theory the earth's crust contain them? For none says that the turbulence of the crust descends to greater depth.

Before our last cognacs had been finished, we spoke of the family system, Nina presenting the nostalgic view of the extended family, Harold asserting that the blood family has little to offer any longer, while admitting her argument. He described his early family -- he an only child, but with numerous relatives, now scattered from the Midwest to California and Florida, those graveyards of American families. I had been urging him earlier to write his Autobiography; he is silent about his past to an abnormal degree. He is noncommittal. Perhaps he prefers to remain a Great Man of Mysterious Origins. Very well, but a good autobiography is worth more than a large question mark.

Washington, 1979

In Memoriam

HAROLD D. LASSWELL
(1902-1978)

Harold! Greetings!

*Snifting bubbles, are you, this season,
in the land of the tall drinks?
Are they pouring you doubles?*

Come back to Chicago, Vienna, Nanking.

*Sounding like we know it all,
in tones serene as your very own,
We slump in low divans
and hunch over brown tables
Spilling smoothly the news about how
you walked upon the Earth once.*

*Welcome back to Washington, New York and New Haven;
your train is set to run on time.*

*You said straight what you saw
Without hee-haws, oinks, or meows
No winks, curtsies, or knotted fists
No cow-eyes, or stony gaze.
Viel Blitzzen, kein Donor,
No "Ho-ho-ho."*

*Pleasant, agreeable Hero of our times,
"if-then" propositions cornucopiously emitted.
Two pounds of value-sharing for all men alive.
Mix one pound of deference, a dash of income,
well-being and safety added to taste,
Be generous with enlightenment.*

*Now that you 're not in it.
More Seasoning is needed.*

*some of the gusto is gone.
In-put, out-go.*

*Hearing the world 's secrets and ours nevermore,
You heard them all, and those to come
that we must explicate ourselves.
Thanks for configurating the North pole
under your gray hair, behind your glasses,
in your midnight coat. You gloves are too thin.*

*Come home again, if you get the chance
The New Year is here.*

So long, Saturn!

Deg ' s Journal, November 18, 1980

It ' s cold outside. I received a letter from Gilbert Davidowitz ' sister telling me that my letter to him arrived but that he had died ' of a heart attack ' last July. Poor lonely mad scholar. He was only fortyish. He must have committed suicide. Never an academic appointment. Nothing published. Brilliant worker in the origins of languages. I immediately wrote Charles Lee [Director of the State Archives of South Carolina, one time President of the American Society of Archivists] who will be startled to hear from me after 38 years, explaining my memorandum on the archives of the dying and their total loss to our culture. I feel extra sad about Gilbert, because he was so alone and so incapacitated for everything except the history of languages. But what a fine capacity. If he might only have known when dying how I like and admired him. He must have known. But he needed just then to be told so.

**[Click here to view
the next section of this book.](#)**