of the one-copy-only policy of many libraries—I am cochairing a Metro Task Force on Copyright with John Cory, a former director of the New York Public Library. Metro is a consortium of 105 libraries of all types and sizes in the New York Metropolitan area. The Task Force, composed of librarians and publishers, visited a number of libraries to study their photocopying practices. I was influenced in my testimony before the 5-year Review Committee of the US Copyright Office in Anaheim on 8 October 1980 by what I learned from those visits and from discussions with the Task Force. The Guest Comment in the November 1980 issue of physics today to which Martin responds is based on my testimony.

The Task Force recognizes that much of the photocopying now being done in libraries is exempt under Section 107 (Fair Use) and Section 108 (Reproduction by Libraries and Archives) of the new copyright law. However, when a library's photocopying goes beyond these exemptions, then, according to a statement that came out of the Task Force's discussions: "If fair use is the exception, then fair compensation should be the rule." I subscribe to that philosophy, particularly for what I consider to be the republishing going on in large libraries with which I am personally familiar.

H. William Koch
Director
1/22/81 *American Institute of Physics*

More on Velikovsky

Carl Sagan in his chapter of *Scientists Confront Velikovsky*,[1] S. F. Kogan in her recent letter (September, page 97) and all other writers on the subject that I know about, fail to mention an important feature of the situation: There are ancient but extant astronomical data which prove that a major part of Velikovsky's tableau is wrong. Ironically, these data come from a source that Velikovsky himself relies upon for an important point in his argument.

According to Velikovsky,[2] the last catastrophic near-collision of the earth with another body was with Mars, and he concludes that this event happened on the night when the Chinese *Annals of Lu*[3] record a "rain of stars." This date is -686 March 23. One of the results of this near-collision, still according to Velikovsky, was that the length of the month changed almost discontinuously from 36 days to its present value of about 29 ½ days, a change of about 560 000 seconds.

The *Annals of Lu* also contain the records of 34 solar eclipses, with dates ranging from -719 February 22 to -480 April 19. Two of the records do not correspond to actual eclipses, so they probably contain accidental errors in writing the date. The remaining 32 records, however, correspond accurately to eclipses that we calculate from modern data. The reliability of these records, which contain only 2 erroneous records out of 34, is remarkably high for records of this antiquity, which were copied many times before they reached us.

Three of the eclipses come before -686 March 23. Of these three, the most valuable one for our purposes is the eclipse of -708 July 17, which was recorded as being total in the capital of the ancient Chinese state of Lu (latitude 35° 40′ north, longitude 117° 1′ east). I calculate from modern theory that this eclipse was indeed large, and it might well have been total, at this point. The agreement between the record and the calculation puts an upper limit to any change in the length of the month that might have occurred in -686. As I have shown,[4] this limit is about 15 seconds, far less than the change of 560 000 seconds that Velikovsky claims, and there is nothing in the data to suggest that there was any sudden change at all.

Thus Velikovsky's claimed near-collision with Mars in -686 either did not occur, or if it did occur, it did not have the effect that Velikovsky deduced from his historical research. If his research led to such erroneous results for the latest period, when the records are most reliable, there is no reason to trust his research for earlier periods, when the evidence is much more tentative.

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3. *Annals of Lu*, author unknown. There is an edition, with a translation into English by J. Legge in *Chinese Classics, volume 5*, Hong Kong University Press, Hong Kong, 1872.
4. R. R. Newton, *The Moon's Acceleration and Its Physical Origins, volume 1*, pp. 180ff, Johns Hopkins University Press, Baltimore and London, 1979.

Robert R. Newton
Johns Hopkins University
9/24/80 *Laurel, Maryland*

THE AUTHOR COMMENTS: Robert Newton bases this criticism on an ancient eclipse report. Lynn Rose of the State University of New York at Buffalo, who has published several articles on ancient astronomical records, was kind enough to supply the following remarks:

R. R. Newton criticizes Velikovsky

continued on page 72

letters

continued from page 15

on the basis of an ancient Chinese eclipse report, and complains that "all other writers on the subject that I know of" have failed to mention such matters. This only shows that Newton is unfamiliar with the relevant literature. Velikovsky himself discussed various eclipse reports (*Harper's*, June, 1951), and my own article in *Kronos*, IV, 2, discusses the very clipse report that is cited by Newton!

This report is from the *Spring and Autumn Annals*, which Newton prefers to call the *Annals of Lu*. Neither the place of observation nor the time of day is given for this eclipse, which lessens its value for Newton's uniformitarian retrocalculations. (Newton is plainly wrong in his claim that the eclipse "was recorded as being total in the capital of the ancient state of Lu"; the *Annals* do *not* specify the capital.)

The question that must always be asked regarding such reports is: Were they based on observation or were they derived by calculation?

"Ironically, Newton himself has provided strong reasons for taking various ancient reports as calculated rather than as observed. In his *Ancient Astronomical Observations*, pages 67-68, he has admitted the difficulties involved in using these Chinese sources in particular. And in his more recent book, *The Crime of Claudius Ptolemy*, Newton accuses Ptolemy of calculating many of the observations that Ptolemy attributes to himself and to his predecessors. These "fabricated data," as Newton calls them, include eclipse reports, some of which are assigned to the eighth century, nearly nine centuries before Ptolemy.

If Ptolemy could extend his calculations back eight or nine centuries, surely the relatively late compilers of the *Annals of Lu* could have retrocalculated some of the "observations" that they included in such works. Ancient and modern scholars are two of a kind when it comes to the ease with which they retrocalculate and "correct" or "fill in" the older texts, solely on the basis of uniformitarian presumptions.

If Newton presumes to disprove *Worlds in Collision* on the basis of one dubious ancient astronomical record, how does he explain the many unexpected predictions which were projected from the theory of *Worlds in Collision* in 1950 and which the space age proved to be correct?[1]

Concerning Venus alone—how would Newton explain the high proportion of Argon 36 found on Venus, its retrograde rotation, its high temperature, and the fact that more heat energy is radiated up from its surface than down from its clouds[2] except by admitting Venus to be a young planet as Velikovsky claimed in 1950?

While Velikovsky did not predict the Argon-36 finding, it corroborates his theory, while forcing the accepted models of our solar system into contradictory ad-hoc explanations.

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1. V. Bargmann, L. Motz, Science **138**, 1350 (1962).
2. R. A. Kerr, Science **207**, 293 (1980).

S. F. KOGAN

12/5/80 *Haifa, Israel*

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PHYSICS TODAY is to be congratulated for having the courage to print S. F. Kogan's incisive letter (September, page 97) exposing the vacuousness of Carl Sagan's highly vaunted appendices which supposedly contain the quantitative debunking of Velikovsky's *Worlds in Collision*. Kogan's detailed analysis, though not exhaustive, is a fitting follow-up to C. J. Ransom's earlier general indictment of Sagan's analysis (page 81, December 1978).

Naturally, discrediting Sagan's calculations does not automatically mean that Velikovsky's thesis of "colliding" planets is correct, but it does make one stop to ponder these questions: 1) Why didn't a widely-acclaimed scientist (who expounds the need for the public to be shown "good science") prepare a logically consistent, physically correct refutation of *Worlds in Collision*? 2) Why didn't one scientist or science writer point out any of the *unequivocal* mistakes when they commented on Sagan's analysis? The list of those commentators includes: George Abell, Anthony Aveni, Richard Berendzen, Richard Berry, Robert C. Cowen, Lester del Rey, Terence Dickinson, Martin Gardner, John Gribbin, Robert Jastrow, E. C. Krupp, Roger Lewin, Euan MacKie, Patrick Moore, Michael Rowan-Robinson and James Trefil. Only two, Dickinson and Jastrow, subsequently had the integrity to acknowledge any of Sagan's errors. 3) Why has not one scientist in the thirty years since *Worlds in Collision* appeared formulated a *valid* refutation?

Kogan's criticism of Sagan's "cooling calculation" might be reinforced with the findings of George R. Talbott. In *Kronos* IV:2 (Fall 1978), Talbott showed that in 3500 years, with large-scale volcanism, Venus could cool by radiation from a candescent state to her observed surface temperature. This cooling curve, specific to Venus, provides strong presumptive thermodynamic evidence for the validity of Velikovsky's thermal history of Venus. A current cooling rate on the order of 0.01K per year would be undetectable with the precision of Pioneer Venus measurements.

At the same time, Talbott exposed the fallacy in Sagan's "cooling calculation" in which Sagan unwittingly merely equated the heat radiated in 50 minutes at 6000K with that radiated in 3500 years at 79K and called it a "cooling calculation" while ignoring the essential parameters of specific heat, mass and surface area. Also, it might be pointed out that the graph of Venus' radio brightness temperature versus time that Sagan uses in his main text to ridicule the idea that Venus is cooling is an irrelevant plot of raw data uncorrected for wavelength, antenna calibration, semidiameter of Venus and phase angle.

Velikovsky presented a fertile hypothesis whose ramifications in science, ancient history, philosophy and religion are far-reaching, though generally unappreciated. Nonetheless, Velikovskian themes are regularly investigated in *Kronos* (P. O. Box 343, Wynnewood, PA 19096), a quarterly entering its sixth year of publication.

C. LEROY ELLENBERGER

Associate Editor and Executive Secretary

10/1/80 *Kronos*

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The critique of Carl Sagan's "refutation" of Immanuel Velikovsky's theories of worlds in collision published in the September Letters (page 97) largely misses the point. Sagan's calculation of probabilities may indeed be very wrong, and given the poor definition of the problem a wide range of such calculations could be made. But we are not dealing with *a priori* probabilities; Velikovsky did not propose that Venus and Mars *could* have close encounters with the Earth, but rather asserted that these near collisions did in fact take place at well defined times in human history. As physical scientists, we should ask whether the evidence supports the reality of these events, not what their probabilities are.

What is the evidence for the encounters with Venus and Mars in about 1500 and 800 BC? The physical indications of the cataclysmic events cited by Velikovsky should be abundant. Does the volcanologist see evidence of widespread eruptions near those dates? Do tree-ring analyses indicate a major disruption of climate then? Is there paleomagnetic evidence for fluctuations or reversals in the Earth's magnetic field? Do the ocean sediments preserve records of global temperature changes, or the coasts display the scars of flooding or large changes in sea level? Are there meteorite craters on Earth dating from these times? Do the lunar rocks returned by Apollo indicate widespread melting of the lunar sur-

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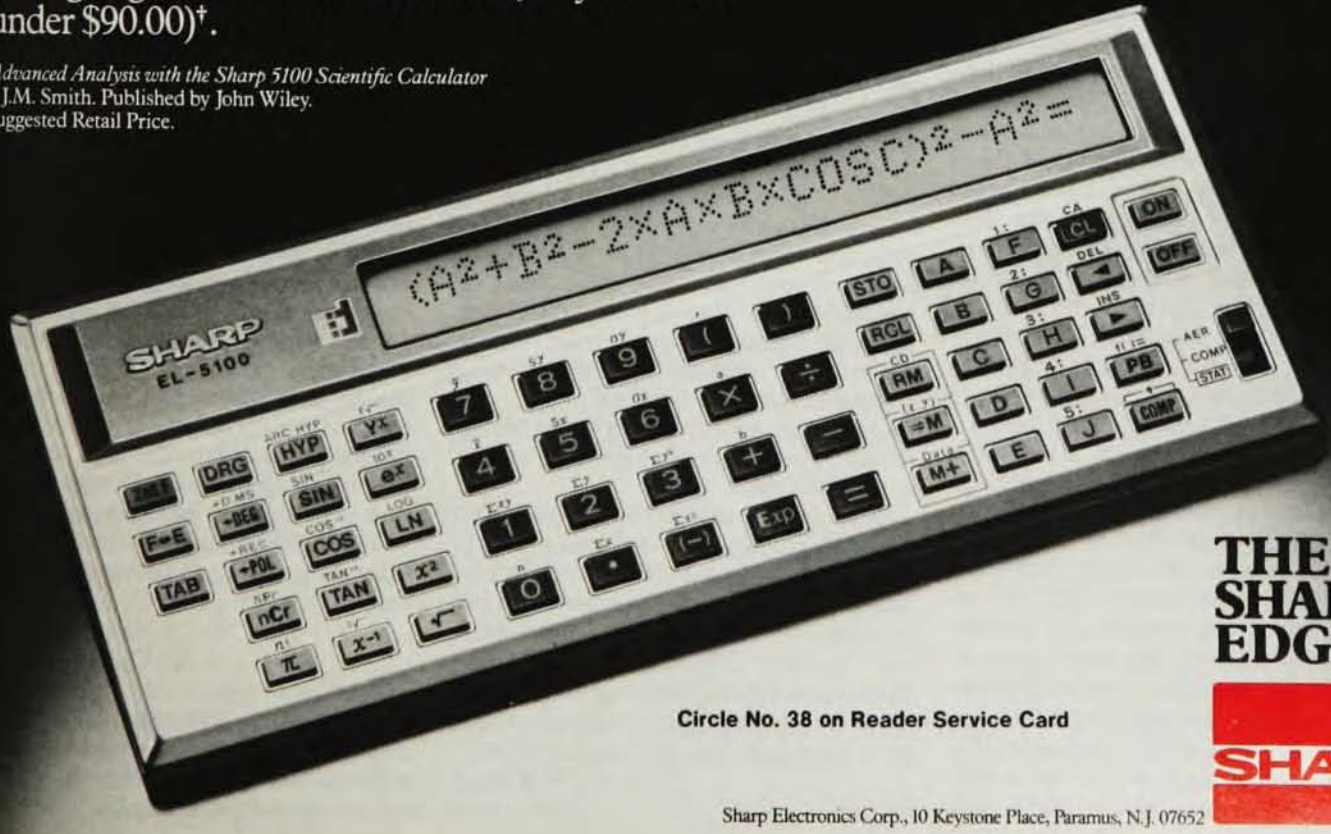

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face during the past few thousand years? All of the above should be the case if Velikovsky is correct, but the answer in every case is a resounding "no." In the face of such evidence, arguing about what might or might not be possible seems rather pointless.

DAVID MORRISON
University of Hawaii at Manoa
10/27/80 *Honolulu, Hawaii*

THE AUTHOR COMMENTS: David Morrison is right that Sagan's probability calculations are not only "very wrong" but also irrelevant.

Immanuel Velikovsky put it very succinctly, in his answer to Sagan: "It should also be borne in mind that the basic subject to be addressed by opponents is 'did it happen' and not 'how likely was it' " (*Velikovsky and Establishment Science*, Kronos Press, 1977 p. 34).

The following quotation from Velikovsky's answer to Sagan is applicable to the questions raised here by Morrison (*V and ES* "Preface"):

"... Sagan seems not to know of the existence of *Earth in Upheaval*, where the geological, paleontological, and climatological evidence of global catastrophes, is presented, and especially of the global changes of 34 and 27 centuries ago. Sagan asks '... And what of the archaeological and paleontological evidence? Where are the extensive faunal extinctions... and where is the evidence of extensive melting in these centuries...?' But the 300 pages of *Earth in Upheaval* are an unbroken record of exactly this."

To answer Morrison's questions, by way of example, we will quote from the section "Dropped Ocean Level" from *Earth in Upheaval* by Immanuel Velikovsky (Doubleday, 1955, p. 181–183):

"R. A. Daly observed that in a great many places all around the world there is a uniform emergence of the shoreline of 18 to 20 ft. ... Daly proceeds: 'Marine terraces indicating similar emergence are found along the Atlantic coast from New York to the Gulf of Mexico;...along the coast of eastern Australia; along the coast of Brazil, southwest Africa, and many islands of the Pacific, Atlantic, and Indian Oceans; in all these and other published cases...the emergence seems to have been simultaneous on every shore' (Daly, *Our Mobile Earth*, p. 178)... 'In thirty-odd years following Daly's first paper many further instances have been recorded by a number of investigators the world over, so that this recent shift is now well established.' (P. H. Kuenen *Marine Geology*, 1950, p. 538)...it is not the result of a slow change; in such a case we would have intermediate shorelines...but there are none...Kuenen writes: 'The time of the movement was estimated by Daly to be probably some 3000 to 4000 years ago' (ibid)."

For reversals of the earth's magnetic field we could quote from the section "Magnetic Poles Reversed" (*E in U* p. 143–147).

For sediments, see the section "The Floor of the Seas" (*E in U* p. 104–107), and for later sediment findings see Velikovsky's article in *The Velikovsky Affair* (University Books, 1967, pp. 241–243).

For sudden climatic changes, see chapter XI "Klimasturz" (*E in U* pp. 173–187). In this chapter there is also a section on "Tree Rings." For recent studies on tree rings H. C. Sorensen's article in *Pensée* (vol. 3 no. 2) is very instructive.

The last melting on the moon was discussed by Velikovsky in his memoranda to H. H. Hess of July and August 1969, and in his article "When was the Lunar Surface Last Molten?" (Both reprinted from *Pensée* in *Velikovsky Reconsidered*, Doubleday, 1976).

Morrison's criticism of Velikovsky's work, without reference to *Earth in Upheaval*, is a measure of the serious consideration Velikovsky has received from those scientists who "confronted" him; for, like Sagan, Morrison participated in the volume *Scientists Confront Velikovsky* (Cornell University Press 1977).

In *SCV* Morrison had brought only astronomical arguments, the main one being his objection to the credit Velikovsky received for his advance claim that Venus would be found to be hot with internal heat because of its recent ejection from Jupiter. Morrison, like Sagan, insisted the heat is due to a greenhouse effect. Since this is a cardinal point in the controversy, it is worth noting that Morrison has not brought this up again. Is it because, as Kerr summarised in *Science* (18 January 1980): "The much balyhooed greenhouse effect of Venus' CO_2 atmosphere can account for only part of the heating, and evidence for other heating mechanisms is now in a turmoil. When Pioneer Venus' probes looked at the amount of radiant energy passing through the atmosphere each one found more energy being radiated up from the lower atmosphere than enters it as sunlight."

Since Velikovsky wrote *World in Collision* (1950), Venus was found to rotate retrogradely, to be 900°F near its surface, and to have inexplicable amounts of argon-36, which indicate that Venus was "formed differently." All these findings contradict existing models. Is it not time that they be tried on a model of a young planet hot from within?

S. F. KOGAN
12/4/80 *Haifa, Israel*

Junk mail

I'm writing to drum up recruits for a costless campaign to stamp out junk mail. It clogs the mailbox and the postal service and irritates the hell out of me. I've gone from dutifully reading it or throwing it away unread to my present stratagem of stuffing it back in the postage-paid envelope enclosed and shipping it back to the sender. (Or, if there's no postage-paid envelope I put on a one-cent stamp and make them pay the postage due!)

I fantasize the effect on the junk tide if millions of people—or just every physicist—did the same. Or do you have a better idea for dealing with this malaise of the mailbox?

HENRY BLOSSER
11/19/80 *East Lansing, Michigan*

Juicy particles

To each of the four fundamental forces in nature (strong, electromagnetic, weak and gravitational) there now corresponds a physical theory (chromodynamics, electrodynamics, flavor dynamics, geometrodynamics). The third of these is an abhorrent etymological anomaly, which I propose to correct. Frederic Peachy of our classics department informs me that the Greek work for flavor is γεῦσις, whence *geusidynamics*, which, by the customary rules of Anglicization, is to be pronounced "juicy-dynamics".

DAVID GRIFFITHS
Reed College
11/24/80 *Portland, Oregon*

Meeting complaint

As a recent participant in the APS Plasma Division meeting in San Diego, I strongly object to the official pressure of the Fusion Energy Foundation at this meeting. Their attacks on opponents of nuclear fission power plants are unbecoming and counterproductive. By questioning the morality of people who have legitimate concerns about the safety of nuclear power plants, they are applying ethical standards to a problem that should be judged on its technical and economic merits. If the Fusion Energy Foundation is allowed to participate in future APS meetings, then in the interest of fairness a group with opposing views should also be invited.

REX GANDY
University of Texas
11/26/80 *Austin, Texas*

THE DIVISION CHAIRMAN REPLIES: Rex Gandy's points were well taken, and allow us to clarify the current division policy on peripheral activities at our