letters

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At present the editors seem to be allowing almost anything. Perhaps they are under the illusion that all the *et al*'s represent horrendous lists of names that would sap their budgets. In fact that is not the case, and they are acquiescing in a sloppy, dehumanizing practice. This practice has spread from groups of fifty down to groups of two in particle physics; surely it will eventually spread to theoretical references, and to other fields of physics. The editors should set a policy that will check or regulate this practice before it becomes too ingrained. I don't look forward to a time when we reference only the experiment of Michelson *et al*, or the paradox of Einstein *et al*.

Of course there *is* a problem with references containing 20–50 names, and a suitable editorial policy would have to deal with that problem. One policy would be to insist on full citation unless a group volunteered for designation by an acronym, as the Pluto collaboration seems to have done. My own suggestion would be to adopt a maximum, such as ten or fifteen, for the number of names to be listed, with any excess designated *et al*. This number could properly be different in journals with different purposes, such as *Physical Review Letters*, *Physical Review*, and *Reviews of Modern Physics*. The numbers suggested are consistent with the number of names that a generation of physicists has found it possible to cite fully, and would meet most of the objections mentioned above. One could also save some space and effort by suppressing initials in long lists. In any case, some policy should be adopted before following the path of least resistance forecloses the question.

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Sagan versus Velikovsky

Sometime before the recent death of Immanuel Velikovsky, author of *Worlds in Collision*, George Abell in his review (August 1978, page 56) of *Scientists Confront Velikovsky* singled out Carl Sagan's "Analysis of Worlds in Collision" as a devasting refutation of Velikovsky's work. Sagan has published this same "Analysis" a few more times since then. Therefore it is only fair to subject his four appendices—which include most of the quantative and scientific arguments, and which are usually taken on good faith—to a short scrutiny.

After Sagan presented his "Analysis" at the AAAS Annual Meeting of 1974, probabilities of 10^{-23} to 10^{-28} were widely quoted as those calculated by Sagan for the planetary encounters described in *Worlds in Collision*.

It is easy to demonstrate that if we apply Sagan's own calculations, not to six actual impacts (that Sagan's calculations assume actual impact is made clear only in his Appendix 1) but to relevant distances of approach, the probability is very different.

In his text Sagan wrote (page 62—all paginations are from *Scientists Confront Velikovsky*):

"The calculation is performed in Appendix 1 that a single 'comet' (with aphelion near the orbit of Jupiter and perihelion inside the orbit of Venus) should take at least 30 000 000 years before it impacts the earth" and therefore "the odds against it in any given millennium are 30 000 to one. But Velikovsky has not one but five or six near collisions . . . If the probabilities are independent then the joint probability of five such encounters in the same millennium is on the short side of $(3\times10^7/10^3)^{-5} = 4.1\times10^{-23}$. For six encounters . . . the odds rise to . . . 7.3×10^{-28}."

But Velikovsky referred to near collisions, not to actual impacts. It is obvious that the odds for a "near collision" are much better than for an "impact" collision. It is also obvious that the encounters described in *Worlds in Collision* are not independent events but interdependent and partly periodical events.

What then is a correct probability? We will resort only to Sagan's "Analysis" and we will follow Sagan's appendix 1. At the end of the appendix (page 98) Sagan gives the following instructions: "Note that . . . an approach to within N Earth radii has N^2 times the probability of a physical collision. Thus for $N = 10$, a miss of 63 000 km, the above values must be reduced by two orders of magnitude."

This, the reader is left to calculate, would give a probability of 1 in 300 per millennium for an approach distance of 1/6 the distance to the Moon. But instead Sagan goes on in the appendix to give his reasons for insisting on keeping the 1:30 000 probability for actual impact (page 98):

1. "The book after all is called 'Worlds in Collision'."
2. "Also it is claimed (page 87, *Worlds in Collision*) that as a result of the passage of Venus by the Earth, the oceans were piled to a height of 1600 miles. From this it is easy to calculate backward from simple tidal theory that Velikovsky is talking about a grazing collision: the surfaces of Earth and Venus scrape!"

Sagan's first "reason" needs no comment. The second "reason," phrased as it is, can mislead the reader to think that Velikovsky claimed such a number. Consider the section in *Worlds in Collision* called "The Tide" (page 87 of the Pocket Book edition):

"The Midrashim contain the following description: The waters were piled up to the height of sixteen hundred miles, and they could be seen by all nations of the earth.' (Ginzberg Legends, III, 22). The figure in this sentence intends to say that the heap of waters was tremendous."

So not only did Velikovsky not claim this number cited in Ginzberg's translation of the Midrash, he clearly explained it as meaning "tremendous". (In the original Aramaic text three hundred "milin" are cited, which Ginzberg translated into sixteen hundred miles. 300 is explicitly noted in Jewish tradition as a number used for exaggeration.) It is interesting to compare Sagan's Appendix 1 with the following paragraph from Sagan's text, where in another connection—purporting to prove Velikovsky mistaken on terrestrial—tidal problems—we found that Sagan himself recognizes that (1) no impact was ever described in *Worlds in Collision*, (2) that the tides described were a few miles high, and (3) tides hundreds of miles high would be produced by an approach of tens of thousands of kilometers (about 10 Earth radii), in contrast to his statement in the appendix that tides hundreds of miles high would be caused by no less than a grazing collision. On page 67 Sagan writes:

"Velikovsky believes that the close passage of Venus (or Mars) to the Earth would have produced tides at least miles high (page 70–71). In fact if these planets were ever tens of thousands of kilometers away as he seems to think, the tides both of water and of the solid body of our planet would be hundreds of miles high. This is easily calculated from the height of the present water and body lunar tide, since the tide height is proportional to the mass of the tide-producing object and inversely proportional to the cube of the distance."

Assuming the tidal calculation in the text to be correct, it seems that it is easily calculated that when Velikovsky wrote of tides miles high, a distance, not of tens of thousands, but of hundreds of thousands of kilometers would be a sufficiently close approach to cause tides of this height (a few miles high).

What would be the probability for such a near collision?

According to Sagan's instructions we have already found that for a distance of tens of thousands of kilometers (10 Earth radii = 63 000 km) a probability of 1 in 300 per millennium applies.

However, if we consider the probability for the more relevant distances of hundreds of thousands of kilometers, we find accordingly that for:

189 000 km (30 Earth radii) it is: 1 in $30\,000/30^2 = 1$ in 33.3;

and for:

315 000 km (50 Earth radii) it is: 1 in $30\,000/50^2 = 1$ in 12.

If Sagan still insists on raising the probabilities to the sixth power he should first correct for the factors he admits neglecting but does not correct for (gravitational attraction, planetary motion). At least the factor of three, which he derives from Opik's more exact calculation (page 98), should have been corrected before he raised 30 000 to the minus sixth power; after all $3^{-6} = 7.3 \times 10^{-4}$.

Correcting for this factor of three we find that for every comet within the orbit of Jupiter and Venus, the probability per millennium is: 1 in 100 that it will come within 10 Earth radii (1/6 the lunar distance), 1 in 11 that it will come within 30 Earth radii (1/2 the lunar distance) and 1 in 4 that it will come within 50 Earth radii (still only 5/6 the distance to the Moon).

In Appendix 3, Sagan discusses the prediction of *Worlds in Collision* that Venus should be hot and calculates the heating of Venus by a presumed passage near the Sun and the planet's subsequent cooling by radiation to space. He obtains a ridiculously low temperature—79K. He then writes: "I find it odd that Velikovsky does not attribute the temperature of Venus to its ejection from Jupiter . . . but he does not" (page 79).

Sagan then calculates the temperature Venus would have been heated to if ejected from Jupiter: "incidentally this would appear to be a good Velikovskian argument for the high temperature of the surface of Venus but . . . this is not the argument."

But in *Worlds in Collision*, on "The Thermal Balance of Venus" (page 371) we find:

"Venus experienced in quick succession its birth and expulsion under violent conditions; an existence as a comet on an ellipse which approached the sun closely; two encounters with the earth . . . with a thermal effect caused by conversion of momentum into heat . . . the core of the planet Venus must still be hot."

As we see, Velikovsky did indeed mention the ejection from Jupiter as the first among three or four other reasons (only one of them being close passage to the Sun) for predicting the elevated temperature of Venus.

Thus by calculating Venus's temperature of ejection, Sagan verified the first reason Velikovsky gave for predicting Venus to be hot.

In Appendix 2 Sagan demonstrates that the claim of his predecessors (Payne-Goposhkin, *et al.*) that if the Earth were to slow down everything not attached would fly off is wrong. In a short calculation he shows that were the Earth to stop rotating, within a little over an hour, not even stalactites would break, let alone things fly off the Earth—an objection to Velikovsky's work that had been used by Asimov very recently and earlier by Sagan himself.

He also calculates that the heat generated would not be enough to melt the Earth (an argument still used by some of Velikovsky's critics) but would cause an average temperature increment of 100 K. Actually this increment should be much lower, because Sagan forgot to include in his calculations the latent heat that would have been absorbed by the melting ice and evaporation of water of the seas and oceans.

Sagan concludes (page 64): "The oceans would have been raised to the boiling point of water, an event which seems to have been overlooked by Velikovsky's ancient sources."

It is Sagan who overlooked a whole section of *Worlds in Collision* called "Boiling Earth and Seas." By overlooking even the "Contents" of the book he was analyzing, he unwittingly helped demonstrate that ancient writings refer to actual events, and thus supplied evidence for what he considered the "nub" of the whole issue. In the introduction Sagan had written (page 48):

"In the 4.5 billion year history of the solar system, many collisions must have occurred. But have there been major collisions in the last 3500 years, and can the study of ancient writing demonstrate such collisions? That is the nub of the issue."

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More on Esperanto

Inspired by the letters in the recent issues, especially that of Bruce Sherwood (July 1979, page 9) I have tested the relative ease of learning Esperanto. I grasped its grammer within a week just from its key, which is available from the Esperanto Associations in every country. Within a month, since I had received it, I was able to write my first letter in Esperanto of an unrestricted complexity. I had not been able to achieve this in any of those languages I am reasonably competent in, even after a year of study.

I write this letter in response to Thomas Wood's comments (November 1979, page 74). Do his ideas reflect a growing isolationist tendency in the US? Certainly in that case Esperanto is the best educational personal investment for the bulk of the US population, especially scientists, to minimize their burden of learning foreign languages. I come from the very heart of Europe where to learn foreign languages is a life necessity for everybody. However, for short-term travel, such as tourism or international scientific conferences, Esperanto is the best candidate because of its neutrality. I join the declaration of the 85 Japanese scientists who suggested that Esperanto become the language of all scientific publications.

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Stretching longevity

An additional characteristic of J. S. Garrow's longevity parameter W/H^2, discussed by L. X. Finegold (June, page 78), (W = mass in kg; H = height in meters), is that it indicates a clearly more efficient route to lower values of this important vital statistic than the traditional methods of diet and exercise for those of us whose current numbers are above optimal. Because

$$\Delta \log W/H^2 = \Delta W/W - 2\Delta H/H,$$

a given fractional change in height is twice as effective as the same fractional change in mass. I have joyfully abandoned my diet and taken up stretching exercises, in anticipation of the many extra years that will accrue after I am able to call Wilt Chamberlain "Shorty."

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Detectives of art forgery

When I read Stuart Fleming's April story (Detecting art forgeries, page 34) I was disappointed to find that although investigation of "Blakelock" paintings was featured prominently in the article, Maurice Cotter, the leading scientific investigator of R. A. Blakelock's works, was unmentioned. I was acquainted with Cotter's work from the beginning, since shortly after neutron-activated radio-autography began to be used to authenticate paintings,[1] Cotter mentioned to me his desire to use the method in a study of Blakelock's work. In fact, the very first Blakelock painting so analyzed was done by Cotter on a painting (authentic) from his own collection in 1969. In the following years Cotter and his collaborators analyzed 40–45 "Blakelocks."[2] The paragraphs on Blakelock in Fleming's article could be taken as an abstract to reference 2. Also no reference was made to the group[3] that used radio-autography to study illuminated manuscripts by "The Spanish Forger" even though the work