thing one might discuss with a visiting Russian should one appear (but the exchanges are over!). The scientific exchange program between the US and USSR has been one of the most visible aspects of detente to the American physics community. A legacy from the Nixon administration, this program had taken many years to reach the level it had attained last year and was a more solid achievement in American–Soviet relations than any step taken by the Carter administration. Two years ago (long before the Soviet takeover in Afghanistan) it was clear that the exchanges were in jeopardy. There is doubt about the technical gains from many parts of the program, but the exchange of people could be of great diplomatic value in increasing mutual understanding and would have been more so had it expanded. (My feeling is that this value of the exchanges was limited by their formality and by excessive maneuvering over who should participate that resulted in delegations whose members were often determined by other than scientific relevance.) Personal contact and experience beat newspapers for learning about other countries and the scientific visitors from the USSR come from a relatively important class there. Conversely, the visibility of the Soviet Union to concerned foreign visitors is itself is valuable insurance for dissidents. These visitors are now fewer. As Aleksandr Lerner, a Soviet activist for Jewish emigration, noted recently, the Olympic spectators would not be interested in dissidents anyway. The extremely strained relations between the US and USSR now make *sustained* protests by physicists difficult, both because of the absence of personal contact by large numbers of people and because of the Soviet government's evident determination to resist outside influences. We should not reduce further the possibility of communication with Russians who may share our concerns.

I recall a story of one Russian exchange visitor who paid a visit to the family library after dinner in an American home. An hour and a half later it was time to take him and his colleagues back to their hotel so his host went in to get him. There he was, reading *Progress, Coexistence, and Intellectual Freedom*, by A. D. Sakharov. Host and guest said nothing about it, but perhaps that guest has a better idea of what Sakharov is talking about than his colleagues back home who have never been to see us.

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Truncated references

There is now a trend in articles on particle physics to list the references at the end of the paper only as Aardvark *et al.* Naturally this practice originates in the labor involved in citing fully the forty or so authors who are involved in some of the very large collaborations. One suspects that the practice is rapidly spreading to *all* experimental work in particle physics, but that is hard to tell because of the difficulty of recognizing such truncated references. However, I was recently able to verify that the limit of absurdity has indeed been reached: In a recent *Physical Review* article an old paper on which I was the first of *two* authors was referred to as Highland *et al*! All other experimental references, several with only three or four authors, were similarly truncated. On the other hand all theoretical references, one involving four authors, were given in full.

First I must confess to an ego problem—I take a modest enjoyment in seeing my name in print, if only in footnotes. This may be a failing, but I suspect it is a rather widespread one. The practice of truncation is sapping one of the minor satisfactions of physics research. Further, I really like to think of later authors (especially theorists) working through at least once the full list of authors in their references. In so doing they briefly commemorate the individuals who contributed to the piece of work they are using, instead of quickly consigning everyone to the permanent anonymity of *et al*, in which may be buried one, two or fifty people.

Putting aside psychological questions and class conflicts with theorists, there is also an information problem involved. The real experts on a subject may have the very page numbers memorized, but the rest of us tend to recognize articles by the combination of names involved. The first author may be either too well known or completely unknown; in either case, it is usually difficult to deduce much from his name alone. In a longer list different readers doubtless pick up on different names—those of friends or ex-colleagues—as the means by which they remember a piece of work, identify the laboratory where it originated, deduce the technique involved or generation of the experiment, and so on. All this helps one evaluate the weight of the evidence and determine whether the reference is one that should be looked up. This information is almost totally lost in the truncated references.

Do the AIP and APS journals have any editorial standards on this point?

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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: