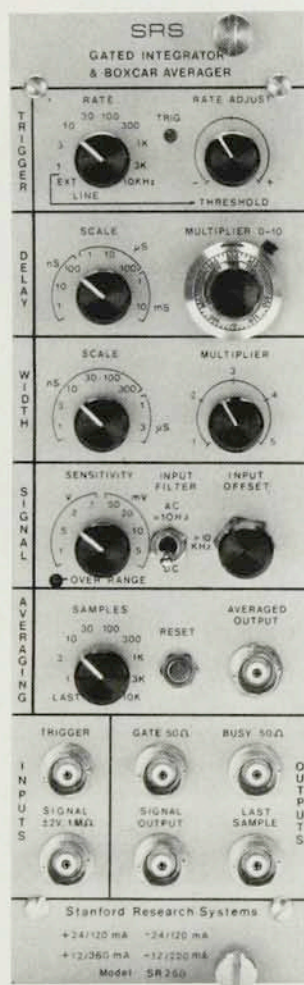


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letters

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Peer review

Let's not assume as an axiom that "peer review" is a system totally beyond question. I don't know of anyone who has never received a stupid, incompetent "peer" review. The idea of peer review is good; in practice, however, we sometimes get arrogant reviews, malicious reviews, hostile reviews, incompetent reviews and reviews by people who haven't even read through the proposed work.

There is a reason to be concerned about the morale of scientists whose well-thought-out projects are denied funding. What nonsense it is to maintain that the peer-review system is always an impartial adjudicator of scientific merit. Are reviewers indeed impartial? Generally they are people who feel competitive, at the very least. So some university bypassed the process. Big deal. They had to convince people, too, and I haven't seen much evidence that scientists as a whole are less prejudiced and more able to judge usefulness of other people's work than Congressmen; nor is there evidence that reviewers are necessarily experts on the proposed work—particularly if it is new and original or even possibly a breakthrough. In fact, in the latter case the reviewer will generally be hostile. Look at George Green, Oliver Heaviside, Joseph Fourier, or a thousand others.

Peer review should require that reviewers be selected from a group of candidates who are competent in the author's area. Peer review should allow answers to irrelevant questions or statements such as "I don't see how this could work" before a final decision is made. If the goal is to provide efficient allocation of resources for the good of the country, a better system is needed, not blind support. Richard Bellman, a member of both the National Academy of Sciences and the National Academy of Engineering and a winner of numerous awards, had a file drawer of incompetent reviews with which he had intended to paper his study, but then didn't bother.

When I give my time to carefully review a proposal (without compensation), I'm sometimes upset more by some of the work I see funded than by the very few that may have missed review, although that too is undesirable.

I suppose I'll get a scathing response from a well-funded reviewer, entrenched with friends in an agency, who can happily continue his decades-old work and deny funding with a five-minute glance at a proposal he has not

understood. However, if the reviewers are so smart, why do they hide behind complete anonymity? Is scientific truth a matter of opinion? Shouldn't scientists be able to agree with discussion? If not, the system doesn't matter, and it might as well be political and avoid pretense of solemn, scientific, impartial judgment. G. ADOMIAN
12/84 Athens, Georgia

Nuclear winter

In February 1984, PHYSICS TODAY published a Search and Discovery story (page 17) summarizing the studies of nuclear winter as conducted by several groups. Since then I have not seen further comment in PHYSICS TODAY; but from the Pentagon there is a report that a panel of scientists have confirmed the findings.

A matter of such importance is an area in which angels fear to tread, and I voice my misgivings with the greatest hesitation. However, they are:

► In general, man's efforts are puny compared with Nature's. In 1883, Krakatau exploded with great violence, and the dust thrown up into the stratosphere colored sunsets for months. What the experts are saying is that, in terms of stratospheric dust, Krakatau is dwarfed by gigaton nuclear explosions.

► This argument may be dismissed as too vague, although I for one find it disturbing. The chief point is that there is an experimental observable, namely, colored sunsets. In 1945 two cities were destroyed by 20-kiloton nuclear bombs. Now, the 100-megaton bomb, referred to in the article of a year ago, represented the effects of one thousand 100-kt bombs on individual cities; the effects of single bombs are therefore presumably additive. Thus in 1945 we should have seen a dust burden of, say, 10^{-3} of that of the nuclear winter. But I for one recollect nothing at all in the way of colored sunsets. I submit that the discrepancy between no visible effects and total blackout for a month must represent a difference of considerably more than three orders of magnitude. I submit also that in the 1953-68 period of above-ground nuclear testing, there must have been an occasional month when the total megatonnage exploded must have been 100; and the same argument applies.

If you can lighten my nonnuclear darkness on this discrepancy, I shall be grateful.

W. D. ALLEN
Oxon, UK

1/85
THE AUTHOR COMMENTS: W. D. Allen notes some apparent conflicts between the nuclear winter predictions and historical experiences with volcanoes

and nuclear explosions. These discrepancies are rather easily explained. Many studies have tried to compare the quantities of dust that might be generated by a large-scale nuclear war, involving perhaps 5000–10 000 Mt of ground-burst explosions, with those produced in the eruption of large volcanoes such as Krakatau. They have generally concluded that the quantities are roughly equivalent. The climatic impact of nuclear war, as predicted by the nuclear winter studies, is greatly different from that of Krakatau, however, because the nuclear war produces soot as well as dust. It is the soot that causes the sharp plummeting of temperatures. (That effect is apparent in the figure that accompanied my story. Compare the curve labeled "baseline, dust only" to that labeled "baseline, 5000 Mt.")

Dust is generated by nuclear weapons only when the warheads hit on or explode close to the ground. The two bombs dropped on Japan in World War II were airburst, so they did not generate any dust. Those explosions did ignite fires, and hence there was smoke, but not enough to obscure the sunlight over a wide area for many days.

The size of nuclear war that might bring about a nuclear winter would depend on the nature of the attack, but in any case is highly uncertain. The 100-Mt attack scenario described in the nuclear winter studies should not be used as an indication of the threshold. That scenario is specifically designed to maximize smoke production by targeting only those areas of cities with the highest density of combustible materials. It requires targeting the centers of nearly 1000 cities, or more than all the cities in the US, Soviet Union and Europe with populations greater than 100 000 (certainly not a "threshold" attack in terms of human casualties). Furthermore, the particular calculation in the original nuclear winter study used several parameters in that scenario that were doubled compared to the baseline. As a consequence, this 100-Mt scenario yields nearly 60% as much smoke as the baseline scenario.

BARBARA G. LEVI

Center for Energy & Environmental Studies,
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Correction

June, page 27—The last line of the middle column of text in Gerold Yonas's article on SDI was lost in the course of production. The sentence that concludes the paragraph should have read: Unless the world is made aware of Soviet actions in ballistic-missile defense, the panel states, "the US will probably be blamed for initiating 'another round in the arms race.'" □

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