

Letters

Nuclear winter

After reading Barbara Levi and Tony Rothman's "Nuclear winter: A matter of degrees" (September, page 58), I wondered what ever happened to the old-fashioned practice of incorporating information obtained by experiment or by observation into technical analyses, especially the conclusions developed therefrom. In the article it appeared that only calculations based on theoretical models whose validity is unquestioned but not justified were used. Although there have been no 5000-Mt (total) nuclear explosions, some attention could well have been paid to some major volcanic events and earthquakes as well as fires (and firestorms) that have occurred. For fire effects, these include the devastating Chicago fire and the Minnesota forest fire of the last century as well as the World War II fires of Hamburg, Dresden, Tokyo, Coventry and even London; in the explosion category, one might consider Krakatoa and the thoroughly studied Mount St. Helens event (estimated to have been equivalent to some 50 Mt of buried explosive) as well as the Tambora incident. And of course *The Effects of Nuclear Weapons* by the Department of Defense gives experimental information for much smaller nuclear explosions.

From the discussion of fire-produced soot, one might conclude that fires initiated by nuclear explosions differ markedly from those caused otherwise, as by firebombing or even chemically; could this mean that the Tokyo fires of World War II differed qualitatively from those of Hiroshima and Nagasaki? Further, I have difficulty accepting the apparent assumptions that all fire soot is instantaneously injected into the atmosphere, apparently meaning the burn time is zero, and that the fuel available to a "nuclear fire" is essentially independent of any concurrent explosive effects in distributing that fuel. Then it is apparently assumed that 5000 Mt would be exploded instantaneously but it is not clear whether only one or two points are involved or if some 1000 points (probably 5 Mt each) are considered. For a variety of technical and nontechnical reasons, neither

assumption seems realistic; even the notion of 5000 Mt exploding as a single event in time is questionable.

Overall, it is obvious that the conclusions developed in any analysis of this type depend heavily upon the assumptions made, and in the article there is little critical evaluation of the validity even of those assumptions stated outright. I recognize that such information may be available in the references and that the article may even be considered a summary of the conclusions reached by the authors of those references. However, considering the absence of experimental information and the apparent uncritical acceptance of unjustified theoretical models, the article adds little useful information on the subject even though it may display the authors' calculating abilities. In fact, it may come very close to fitting the computer analyst's definition of GIGO (garbage in, garbage out).

HUGH F. HENRY
Greencastle, Indiana

11/85

LEVI REPLIES: I certainly agree with Hugh Henry that technical analyses should be based on experimental data. In fact, my coauthor, Tony Rothman, and I tried to stress in our article that the severity of climatic effects after a nuclear war is highly uncertain because so few relevant hard facts exist. Certainly people studying nuclear winter have looked at the historical evidence of large volcanoes and intense urban or forest fires but these events were not recorded instrumentally. Researchers have heavily consulted the pages of *The Effects of Nuclear Weapons* to estimate, for example, the distance from a nuclear detonation at which a fire might ignite on a day of given visibility, but it cannot tell us how much combustible material might be there to burn, nor what the visibility will be on the day of an attack. Most available quantitative data come from lab experiments under controlled conditions that may be very different from those that would exist in an actual war. The climate models used to simulate the climatic effects indeed make very simplifying assumptions about the distribution of smoke in time and place, as Henry points out. I hope that work will continue to make these programs more capable, and hence more realistic.

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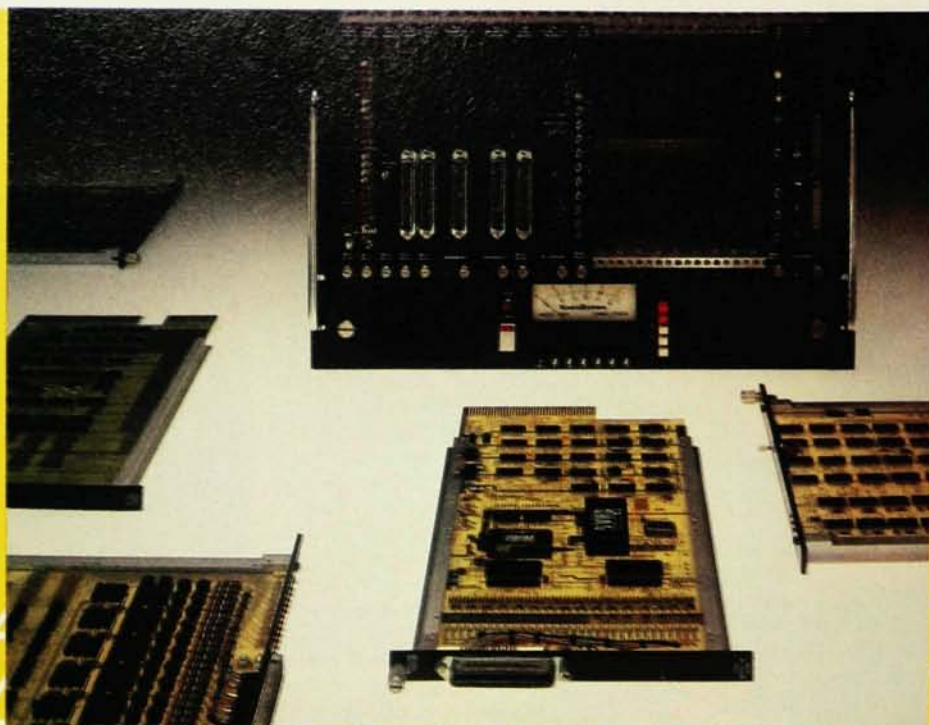
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letters

Clearly more relevant data are needed before any prediction of nuclear winter rests on a sound foundation. We hoped our article would help highlight those parameters that are key determinants of the climatic effects so that research efforts might be directed to narrowing the most critical uncertainties.

BARBARA G. LEVI
Princeton University
Princeton, New Jersey

6/86

I wish to compliment Barbara G. Levi and Tony Rothman on avoiding, for the most part, the following perversion of the discussion on nuclear winter in their article. It has been all too common in the recent spate of press treatments of the possible final-solution consequences of nuclear war to stand the discussion on its head. One reads that the conclusions are (not surprisingly) not certain. One is invited to take some comfort in this. Surely in a sane world the burden of proof is on the proponents of bearable nuclear war to prove that the cataclysmic nuclear winter will *not* occur, rather than the other way around.

W. C. MEECHAM
University of California
Los Angeles

10/85

'Von Kármániana'

William Sears's article about Theodore von Kármán (January, page 34) will certainly interest many of his old friends—but I am sorry to see that Frank J. Malina's name was spelled incorrectly in the illustration on page 38. Historians of engineering—and in particular of the Jet Propulsion Laboratory—may like to know that Marjorie Malina has now donated her late husband's library to the Arthur Clarke Centre for Modern Technologies at the University of Moratuwa, Sri Lanka, and it is now in the process of being cataloged.

May I add my own two favorite bits of von Kármániana to the record? Both occurred at IAF congresses. At Zurich in 1953, von Kármán was lecturing fluently in English, German and French when his attention was distracted by a handsome lady in the front row. The lecture went on and on and the audience was getting restive when the object of his concern, who happened to be my wife, whispered to me, "Do you think if I leave now he will stop talking?"

At another IAF lecture, a hapless American engineer, straining for superlatives, stated that "not only is the

curve exponential—but the rate of increase is *also* exponential!" At this point von Kármán interjected mildly, "I always thought that was the definition of an exponential function."

ARTHUR C. CLARKE
University of Moratuwa
Sri Lanka

1/86

Memories of Pauli

The recent article by Victor Weisskopf about Wolfgang Pauli (December, page 36) reminded me of a story that a certain Northwestern University professor (who will remain nameless, but who was a graduate student at Princeton while Pauli was there) used to tell in his physical-chemistry classes.

As Weisskopf noted in his article, Pauli had a young wife. She liked to dance. After a party that ended with the then graduate student dancing with Pauli's wife, she told him, "Wolfgang will drive you home." Upon reaching the car it was discovered that Mrs. Pauli had forgotten her boots, and Pauli asked the student to retrieve them. After returning with the boots he found the Paulis in the car and ready to go. When he attempted to enter the car on Mrs. Pauli's side, Pauli said, "No, over here please." When he reached the other side of the car, Pauli opened the door and took the boots. He then drove off without the student.

The point of this story is to help us remember the Pauli exclusion principle.

W. B. GLEASON
Chaska, Minnesota

1/86

High-school physics

The description of the high-school physics teacher's world in "The physics classroom revisited: Have we learned our lesson?" (November, page 46) is absolutely correct.

As one with 15 years' experience, usually as the only physics teacher in the school, I can attest to both the isolation and the diverse demands of the job. Not only are we expected to be up to date in our personal knowledge of physics content and teaching methods and materials, but we are also supposed to make physics "nonthreatening" to the general student body, increasing enrollment and providing opportunities for academic success while at the same time challenging the "super scientists" and "whiz kids" who pop up now and then.

The possibility of collaborating with physicists in higher education and industry in developing a more appropriate high-school physics program is exciting. We high-school teachers need professional-growth opportunities such

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