LETTERS

"Nuclear Winter" Studies

The article "Soviets offer little help" by R. Jeffrey Smith (News and Comment, 6 July, p. 31) contains remarks by Richard Turco and Starley Thompson that are critical of work being carried on at the Computing Center of the U.S.S.R. Academy of Sciences, where we have used a three-dimensional climate model to simulate the climatic consequences of nuclear war.

The results of these simulations were first published in English in October 1983 (*1*) and were first presented publicly at the 3rd International Seminar on Nuclear War in Erice, Sicily, in August 1983. They were also presented at numerous international conferences, including "The World After Nuclear War" conference in Washington, D.C., on 31 October and 1 November 1983; at hearings held by Senator Edward F. Kennedy (D–Mass.) and Senator Mark O. Hatfield (R–Ore.) on 8 December 1983; at the special working group session of the Papal Academy of Sciences in Vatican City at the end of January 1984, and at the Climate Commission of the National Academy of Sciences session in Washington, D.C., on 2 March 1984.

I believe it is useful to emphasize the following points about the criticism of our work.

1) The model we are using now is a product of several years of Soviet-American cooperation. Many leading American scientists know the detailed history of our climate model (similar in origin to models in use at several major U.S. institutions), which includes calculation of both the atmospheric circulation and the thermodynamics of the upper ocean.

2) The purpose of our work was to test the worldwide effect of massive atmospheric pollution resulting from postwar fires and the induced large reduction of surface temperature. This was first predicted by Paul Crutzen (West Germany) and John Birks (United States) (*2*).

3) Our goal was to use a relatively simple climate model to simulate the major geographical distribution of the perturbations in the normal state of temperature, solar flux, wind fields, and so forth resulting from a nuclear conflict. We obtained a three-dimensional transient view of a "nuclear winter" simulation using a computer ten times faster but with less memory than an IBM Personal Computer.

4) Our results compared favorably with those of other scientists, including the group working with Carl Sagan. We have also prepared a joint paper (*3*) with several scientists at the National Center for Atmospheric Research (NCAR) in Boulder, Colorado (Starley Thompson is one coauthor). This report shows quite good agreement with the much more detailed and computationally expensive NCAR model.

The details of our research, ranging from the origin of the basic model to the presentation of our results, have been presented at many national and international conferences. Numerous leading scientists from the U.S.S.R., the United States, and other countries participated in those meetings (including Turco and Thompson) and offered no substantial criticism.

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1. V. V. Aleksandrov and G. L. Stenchikov, *The Proceedings on Applied Mathematics* (Computing Center of the U.S.S.R. Academy of Sciences, Moscow, 1983); *U.S.S.R. Comput. Math. Math. Phys.* **24**, 140 (1984).
2. P. Crutzen and J. Birks, *Ambio* **11**, 115 (1982).
3. S. L. Thompson, V. V. Aleksandrov, G. L. Stenchikov, S. H. Schneider, C. Covey, R. M. Chervin, *ibid.*, in press.

It is regrettable that my statement regarding Vladimir Aleksandrov's "nuclear winter" modeling (*1*) appeared in such a harsh light and with no supplemental explanation. An explanation is in order.

The atmospheric component of Aleksandrov's computer simulation model was originally developed in the United States in the early 1970's. He has since enhanced and modified his model to, among other things, allow it to run using the limited computational resources available at the Computing Center of the Academy of Sciences. Aleksandrov's model does have a number of defects (weaknesses would be a better word)—all numerical models do. We freely admit in our reports the weaknesses and questionable assumptions in our own nuclear winter simulations (*2, 3*).

One of the simplifying assumptions made by Aleksandrov was to impose a stepwise decrease in smoke amount over time. This assumption resulted in an instantaneous removal of a large amount of smoke from the model atmosphere when smoke-free conditions were allowed to return. The unrealistically rapid removal of the smoke would create a strong surface warming as the previously heated atmosphere transferred heat to the surface. As noted by Aleksandrov and Stenchikov (*1*), "The Northern Hemisphere is warmed up extremely, because of the above described reheat after the switch off of the black out." I was referring to this post-smoke warming when I said that one of Aleksandrov's "major conclusions is apparently incorrect."

Aleksandrov's most important conclusion, strong initial cooling of land under the hypothesized smoke cloud, agrees qualitatively with our studies. Any known model deficiencies have little effect on this fundamental result. Indeed, Aleksandrov, I, and our co-workers have coauthored a paper (*3*) comparing our model results, which, despite the substantial differences in the models, have many similarities.

Aleksandrov should be congratulated for taking the lead in Soviet studies of climatic effects of nuclear war. He and his co-workers were pioneers in three-dimensional simulations of nuclear winter effects, but there is much work left to be done. I see an opportunity for the Soviet research effort on nuclear winter to build on Aleksandrov's pioneering contribution. The resulting communication and scientific cooperation would serve to enhance our incipient understanding of a complex and potentially catastrophic phenomenon that would not respect national borders.

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My comments on the scientific work of Vladimir Aleksandrov and his colleagues seemed particularly harsh in Smith's news item, taken as they were out of the context of a broader critical discussion of Soviet research on "nuclear winter." Aleksandrov's original calculations (*1*) were actually performed in a timely manner and represented a useful preliminary three-dimensional simulation of global nuclear winter effects (*2*), for which he is to be applauded. My criticisms, however, centered on two specific points: (i) that these preliminary calculations contained flaws not properly acknowledged or promptly corrected;

and (ii) that Aleksandrov's simulations have represented the major Soviet contribution to the SCOPE (Scientific Committee on Problems of the Environment) international study on the atmospheric effects of nuclear war. Aleksandrov's model suffers from problems of low horizontal resolution with only two vertical levels, inadequate treatment of smoke and dust radiative properties and time-dependence, deficient portrayal of normal climatology, and anomalous forecasts of nuclear winter effects. These problems have attracted considerable attention from, and are familiar to, scientists working in the field (*3*). Nevertheless, Aleksandrov's calculations were the first of their kind and thus deserve special recognition.

Other Soviet responses to the nuclear winter issue have been more disappointing. On 1 November 1983, at the Washington Conference on the Consequences of Nuclear War, Soviet academicians, via a direct "Moscow Link," stated that they had solved the nuclear winter problem independently and had arrived at essentially the same results as their Western colleagues (*4*). Nevertheless, during the subsequent 8-month period, no substantive physical data, and little evidence of objective scientific analyses, were forthcoming. It would be artificial, in my opinion, not to be skeptical under such circumstances.

The Soviet scientists I have met this past year are amicable, technically competent, and apparently concerned about the prospect of a nuclear disaster. I hope that cooperative research will go on, but with the clear understanding that criticism, as a crucial element of scientific inquiry, will continue.

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3. R. D. Cess, unpublished manuscript.
4. P. R. Ehrlich, C. Sagan, D. Kennedy, W. O. Roberts, *The Cold and the Dark* (Norton, New York, 1984), pp. 131–153.

Care of High-Risk Infants

We write to comment on Constance Holden's briefing "Baby Doe compromise imminent" (News and Comment, 20 July, p. 294). There are three organizations in pediatrics whose constituencies are responsible for the vast majority of care of high-risk infants. These are the American Pediatric Society, the Society for Pediatric Research, and the Association of Medical School Pediatric Department Chairmen. Despite our considerable interest in and involvement with the care of high-risk infants, none of our societies was involved in the crafting of the proposed amendment to the Child Abuse Prevention and Treatment Act. Thus the amendment did not result from "intensive consultations with interested parties," as Holden states. Nor does the statement "satisfy everyone while affirming prevailing medical and ethical practices." In fact, our three societies are steadfastly opposed to the proposed amendment, just as we were to regulation on the same subject issued by the Secretary of the Department of Health and Human Services.

We firmly believe that the sensitive and highly complex issues concerning the care of high-risk infants (whether or not they are handicapped) must be decided on a case by case basis in discussions between the responsible physician(s) and the parents, with consultation, whenever appropriate, from the institution's ethical review board. In this regard we warmly support the recommendations of the President's Commission on the Study of Ethical Problems in Medicine and Biomedical and Behavioral Research that were issued in March 1983.

Finally, there is no mention in Holden's article that, in addition to the dissent of our organizations and the American Medical Association, the Association of American Medical Colleges also dissents. That organization represents the nation's medical schools and their faculties as well as the associated teaching hospitals. Thus, the proposed amendment is very far from being a consensus statement.

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Haitians and AIDS

Gallo *et al.* (*1*) report that HTLV-III is the most likely candidate virus for the acquired immune deficiency syndrome (AIDS). The test for HTLV-III antibodies will help define more accurately the populations at risk of developing AIDS. In the Research News article about the work by Gallo *et al.* (4 May, p. 475), Jean L. Marx projects that "there may be an enormous demand for the test." She notes that among the 20 million homosexual males in the United States an unknown number are promiscuous and therefore at high risk of developing AIDS. However, she also includes half a million Haitians, virtually all Haitians living in the United States, in the group of people who will need to be tested for HTLV-III. We are not aware of data in the literature that show an association of HTLV-III and healthy Haitians, or even Haitian patients with AIDS. In our most recent experience in Haiti, we have found accepted risk factors in 67 percent of our patients with AIDS (*2*), indicating that not all Haitians are at risk for AIDS, as Marx implies, but rather a selected subgroup.

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2. J. W. Pape *et al.*, *Clin. Res.* **32**, 379A (1984).

Erratum: The name of the spokesperson for the Union of Concerned Scientists quoted in Eliot Marshall's briefing "The secret recipe of GE's reactor safety study" (News and Comment, 20 July, p. 294) was misspelled. Her correct name is Susan Niemczyk.

Erratum: In table 1 of the report "Rock avalanches caused by earthquakes: Source characteristics" by David K. Keefer (23 Mar., p. 1288), data for location 20 (Buller River Canyon, New Zealand) was incorrect. Under the column headings "Intense fracturing," "Planes of weakness dipping out of slope," and "Previous slides†," the entries should have been "Yes" rather than "No." In addition, the following citation should have been included in reference 13: M. R. Johnston, *Trans. N.Z. Inst. Eng.* **1**, 239 (1974).

Erratum: In the report "High-resolution chromosome sorting and DNA spot-blot analysis assign McArdle's syndrome to chromosome 11" by Roger V. Lebo *et al.* (6 July, p. 57), errors occurred in the legends for figures 1 and 2. In the next-to-last line of the legend for figure 1, "Lief bucket" should have been "Leif bucket." In the fifth line of the legend for figure 2, "ϵ-globin" should have been "α-globin."