"Nuclear winter" to be taken seriously

In two recent articles[1,2] John Maddox has criticized our technical analysis of the long-term worldwide consequences of nuclear war[3]. Our findings on what we have called nuclear winter evolved from, and were partly calibrated by, 12 years of related research on martian dust storms, the climatic consequences of volcanic explosions on Earth and the possible collision of an asteroid or cometary nucleus with the Earth at the time of the Cretaceous/Tertiary extinctions.

Given the importance and sensitivity of the subject, we took extraordinary measures to have our calculations reviewed by a large number of experts in atmospheric physics and chemistry, at a meeting specially convened for this purpose in April 1983, and by other means — well before the submission of the paper for publication. Our article refers to some 95 published scientific papers and reports in which further technical details can be found. We have discussed the technical issues with a large number of recognized experts in a variety of fields (which is not to say that all these experts endorse all of our conclusions). Most of the details of our mathematical models are already published, and a detailed 145-page description of our data sources and methodology has been widely distributed. A much more extensive discussion is in preparation.

Some apparently believe that unless nuclear winter is demonstrated beyond a shadow of a doubt, it is dangerous to discuss it in the scientific literature and in public forums. But this is not a subject amenable to experimental verification — at least not more than once. We hold, to the contrary, that open and informed debate on this issue is the only responsible approach, given the gravity of the potential climatic catastrophe we believe we have uncovered.

Whether Maddox has carefully read the work he is criticizing is open to some doubt. He seems to have missed the point that the major cause of nuclear winter is sooty smoke from fires rather than silicate dust raised from the surface. We clearly state that the greatest climatic effect is from smoke generated in the burning of cities. Nevertheless, in his first article and much of his second, Maddox surprisingly holds that the main climatic perturbation we derive is from dust. That this is not merely a careless use of words is made clear by his repeated comparison of our predictions of the effects of the nuclear war with the frosts produced by volcanic eruptions, exemplified by the work of LaMarche and Hirschboeck[4], who find good agreement between the timing of some volcanic eruptions and temperature declines. Our previous work[5,6] shows that major volcanic explosions in the past few centuries may have produced optical depth perturbations of at most a few tenths, and global temperature declines of at most one degree centigrade. These values are less than even the dust in a nuclear war would probably produce, to say nothing of the soot.

Maddox writes "The chief reason for this huge discrepancy between the consequences of a nuclear war and a volcanic eruption stems from the assumptions made about the quantities of dust carried into the stratosphere by the different events." But our calculations unambiguously show that if there were no dust whatever injected into the atmosphere, and no aerosols of any composition injected into the stratosphere, severe climatic perturbations might nevertheless ensue from upper tropospheric soot. By his statements, Maddox also seems to be unaware that climatic effects of volcanic explosions are caused principally by sulphuric acid aerosols, not by silicate dust. Likewise, our results differ from those of the 1975 National Academy of Sciences report, largely because that study did not consider the injection of soot into the atmosphere from massive fires. But it is also true that the 1975 report did not carefully quantify the dust injection.

Maddox evidently believes that we have not justified or even stated the properties of the dust we employed in our calculation. In fact, we used direct measurements of the particle size distribution of dust produced by nuclear explosions, data on the properties of soils and observations of windblown dust. The log-normal/power law distribution function adopted, the complex refractive index of the dust, and other relevant parameters are explicitly stated.

Nuclear explosions are not volcanic explosions, and the differences between the two kinds of events far outweigh what they have in common. The relative importance of smoke in dominating the attenuation of sunlight in the immediate post-nuclear war environment was first noted by Crutzen and Birks[7]. Our calculations show that smoke is much more effective than dust per unit mass of aerosol, both for optical and for climatic perturbations, because of the greater absorptivity and finer particle size of the former.

Maddox criticizes Covey *et al.*[8] for applying their "new model" to nuclear winter before its applicability has been demonstrated in more conventional calculations of the general circulation. The 'new model' is in fact the Community Climate Model of the National Center for Atmospheric Research, which has been in general use for some years. In our *Science* paper[3] we explicitly noted that the great heat capacity of the oceans will have an ameliorating effect on the post-nuclear war temperature perturbations even in continental interiors, and made a rough estimate of the magnitude of this amelioration. As Covey *et al.* make clear, their three-dimensional general circulation model, as well as one, two and three-dimensional models at Lawrence Livermore National Laboratory and at the Computing Center of the USSR Academy of Sciences, are all in good agreement, in hemispheric average, with our published results. Moreover, Covey *et al.* show that the effects could be significantly worse than we had stated, in that "quick freezes" could occur in a matter of days in areas far distant from the combatant nations, as well as showing that calculated perturbations to the global circulation could bring dust and soot rapidly to the tropics and into the Southern Hemisphere, as we had originally proposed.

There are many points about nuclear winter that require further work, both theoretical and experimental. We have never suggested that the last word has been said on this subject[9]. We have, however, run some 50 different cases, to study uncertainties and sensitivities, in many of which, including those we consider most likely, the climatic effects are very severe.

There are contingencies, which we consider unlikely, that could mitigate the severity — for example, if the smoke is everywhere efficiently scavenged from fire plumes, or if there is persistent mesoscale patchiness in the aerosol cover. But there are also circumstances, some of them very likely, in which the effects could be considerably worse. For example, the smoke from fires set by one nuclear explosion over a city can be propelled rapidly by a later explosion to very high altitudes, where it is immune to scavenging mechanisms. By creating channels for bomblight and by drying, a first nuclear explosions over forests can enhance the burning produced by a subsequent nuclear explosion. We have also entirely ignored the effects of tactical nuclear weapons; there are now almost 35,000 of them in the world inventories, many with yields greater than those of the bombs that destroyed Hiroshima and Nagasaki.

The dramatic restructuring of the Earth's atmosphere by injected aerosols moves the lower atmosphere towards isothermality and the upper atmosphere towards a major thermal inversion, as shown in our *Science* paper. In a fully interactive calculation, this restructuring would significantly prolong the duration of the climatic effects following a nuclear war. The snow/albedo and sea ice/thermal inertia (see ref. 10) feedback effects also act to extend the duration of nuclear winter.

As one measure of the importance of the nuclear winter problem, within the past few months major research efforts, costed at many millions of dollars, have been authorized in the government laboratories of both the United States and the Soviet Union, and still larger programmes are under urgent consideration. Spokesmen for the US Department of Defense have recently testified that they take nuclear winter very seriously[11].

In the closing paragraph of our *Science* paper and elsewhere in the text and notes, we summarize our view of the limitations of this research and express the hope that, because of these new dangers of nuclear war, "the scientific issues raised (here) will be vigorously and critically examined". Many members of the scientific community, ourselves included, are now engaged in such an examination.

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JOHN MADDOX REPLIES — My objective, in the first brief reference to this work and in the second reference, was to remind readers, either of the original works or of *Nature*, that these calculations are uncertain. Plainly I should have made more of the smoke and less of the dust, but the conclusion remains — that the assumptions on which these calculations are based, and the computational techniques themselves, are respectively (but necessarily) so uncertain and imprecise that it would be folly to regard them as more than suggestive, but interesting and even valuable (as I said) on that account.

Uncertainty about the scale on which nuclear war might be waged is probably unavoidable, and I have no complaint at the scenario used; it serves the purpose. The uncertainty about the quantities of smoke generated, the height to which smoke would be carried, the local meteorological effects with which its generation would be attended and the rate at which particles would aggregate and be washed out cannot be processes that could affect the quantity of smoke available for redistribution over the surface of the Earth by one or more orders of magnitude.

Even the climatic modelling is susceptible to sources of error not yet taken account of. The essence of the nuclear winter is a global temperature inversion: how stable is this against lateral variations? The point has not to my knowledge been tested. And there has been no investigation of the likelihood that such an inversion would be formed, by the diffusion of carbon particles over the surface of the Earth, without provoking the vertical movements in the atmosphere whose effect might well be to inhibit the temperature inversion altogether. I acknowledge, of course, that this is a difficult and, for the time being, an intractable problem; the authors have followed other modellers in solving the easy problems first. On such a matter, certain to stir the public imagination, it seems to me improper that the results of calculations should be published even in sober language without a warning to all potential readers of the pitfalls there must be. This is doubly unfortunate when, as on this occasion, a purportedly scientific publication is so fully amplified by popular articles, first in *Parade*, most recently in *Scientific American*.

I do not believe that the authors are politically motivated, except in the most general sense that they believe they may help to save the world. But I think it disingenuous of them to overlook the ways in which their conclusions may be used by politicians in other causes, often of a political character. □

Amino acid transport systems

SIR — We are concerned about the problem of making simple and unambiguous reference to well-characterized amino acid transport systems, and we indicate here how we aim to minimize unnecessary further complexities in our use of abbreviations for such systems.

Where a new transport system appears to be simply a variant of a known system, we plan to recognize this close relation with a terminology such as A1, A2, ..., reserving previously unused Roman letters only for more distinctly different systems. In this way we hope to avoid exaggerating complexity. We hope also to avoid evoking any single amino acid where it may be misleading because the system later turns out not to be approximately specific for that amino acid.

For systems for basic amino acids in general, where the mediator appears to accept these amino acids only in a cationic form, we plan to use the symbol y^+, the positive charge indicating the apparent selectivity for the cationic form. (This symbol omits an initial letter L earlier included in Ly^+ (ref. 1), whereby a single amino acid was too strongly evoked.) Conversely, for systems for dicarboxylic amino acids where these appear to be accepted only in their anionic form, we plan to use the symbol x^-. The plus and minus signs do not merely emphasize the requirement that the amino acid substrates have a positively or a negatively charged side chain; they also allow for the possibility that systems will later be found that specifically recognize and require the uncharged side chain group $-COOH$ or $-NH_2$. We plan then to add a single subscript letter such as A, G, or C (for example, x_A) where it seems desirable to indicate that one of these anionic systems is rather specific for aspartate or glutamate, or includes cystine among its substrates. An important Na^+-independent system for anionic amino acids, x_C^-, does indeed include cystine and glutamate among its substrates. When a transport system x^- and y^+ is Na^+-dependent, we propose to indicate it by making x and y capital letters, X^- and Y^+. Specification as to the cell type or organelle under consideration will generally be needed when referring to a particular system, especially for a system apparently peculiar to one or a few tissues.

The terminology x, y may be seen as part of a scheme x, y, z, in which z stands for 'zwitterionic'. This leaves the as yet unassigned letter z for one or all systems for zwitterionic amino acids. The letter N is preempted for another meaning[2].

We do not necessarily mean to complicate matters by these ideas, by changing nonconforming abbreviations which may be regarded as already well established, even the capital L in the classical system[3], although a lower case l (as in l3, l4, ...) will seem appropriate for new similar Na^+-independent systems. We think it is important that a transport system should be characterized in detail before a code is designated for it. The various designations need even then to be regarded as provisional, as the homogeneity of a given component of transport is itself a provisional conclusion, and two components may turn out to be less similar than was at first supposed.

We invite colleagues who come to participate in identifying such transport systems to join us in a general effort for reasonable simplicity.

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