Nuclear winter and carbon dioxide

The National Academy of Sciences has at last reported on the nuclear winter. It clarifies the scientific uncertainties that affect other important climatic models.

THIS week's report on nuclear winter by the US National Academy of Sciences may be late (by more than a year), but is nonetheless welcome. The document, prepared by a committee under Dr Georges Carrier of Harvard University, was a key reference in the paper that remains the most coherent account of what the nuclear winter would be like, that of Turco *et al.* (*Science* **222**, 1283–1292; 1983). Much of the delay has come about because of the diligence of referees of earlier drafts.

The nuclear winter is the phenomenon by which, in the aftermath of nuclear war, the upper atmosphere will be filled with particles of smoke from fires burning on the surface of the Earth. After a few days, the smoke will be distributed uniformly. If there is enough of it, the Sun will be obscured, the lower atmosphere cooled and a temperature inversion formed that may be stable for weeks or even months. Given persistence, and enough obscuration, surface temperatures would be so low for so long that most living things would die.

During the past year, the concept of nuclear winter has been widely canvassed. Apart from successors to the paper by Turco *et al.*, there have been public meetings, popular articles (of which the first seems to have been that by Carl Sagan of Cornell University in the supermarket magazine *Parade* for November 1983), and several speeches by politicians. Under the auspices of the International Council of Scientific Unions, the Scientific Committee on Problems of the Environment is well launched on a formal study of the problem that is due to be published next year.

It is in no sense surprising that the issue has been controversial. Most issues concerning nuclear war have this effect. Earlier reports on the consequences of nuclear war by the National Academy of Sciences, most recently in 1978, were deficient in their treatment of the consequences of atmospheric soot, a circumstance first pointed out by P.J. Crutzen and J.W. Birks (*Ambio* **11**, 114–125; 1983). I have run into trouble with Turco *et al.* for suggesting, on grounds disputed, that their original article had been "hyped", as publishers say (see *Nature* **307**, 107; **308**, 11 & **311**, 307; 1984), but I had not known then that the launching meeting had been supported by a grant of more than $50,000 from the Boston-based Kendall Foundation to a public relations firm. Edward Teller made a similar case more convincingly (see *Nature* **310**, 621; 1984), if at greater length.

The National Academy's new report is a major contribution to this evolving discussion. (A full report of it will appear in *Nature* next week.) Inevitably, its effect is to accept that nuclear winter could be a consequence of a major nuclear exchange but to emphasize more clearly than has been customary the uncertainties in detailed calculations of what a nuclear winter would be like or how long it would last. Nobody will be surprised. In any attempt to predict the behaviour of a system as complicated as the atmosphere, it is natural that the starkness of first approximations should be relieved when refinements are introduced.

Those who think otherwise could do worse than glance at almost any issue of the purple version of the *Journal of Geophysical Research*. The October issue, for example, contains an account of one of the most ambitious attempts to calculate the climatic consequences of increased carbon dioxide in the atmosphere (Washington, W.M. and Meehl, G.A. *J. geophys. Res.* **89**, 9475-9503; 1979). The authors have used the versatile community climate model they have helped to develop at the National Atmospheric Research Center (NCAR).

The calculation is both an illustration of how competent the climate modellers have become and of how far they have to go. The authors have set out to calculate the properties of two atmospheres, one with the present concentration of carbon dioxide, one with twice as much. The surface of the Earth is given a "realistic geography", which means that the continents are accurately placed where they should be, so that the variation of albedo over the surface of the Earth is not very different from reality. But the oceans are represented only by a slab of water 50 metres thick, which allows for seasonal heat exchange but not for global heat transport, a simplification which is sensible enough when the objective, as here, is simply to calculate the equilibrium condition of two atmospheres containing different amounts of carbon dioxide.

This version of the model is persuasive. It reproduces well enough the seasonal variation of the atmosphere as it is. Stratospheric temperatures come out well, for example, as do the general patterns of surface temperature and even wind velocity. Precipitation is less well calculated, sea-ice cover is over-estimated (perhaps because of the neglect of global oceanic transport), as are summer continental temperatures (because of the modellers' endless problem of allowing accurately for clouds).

The prediction of what the climate would be like if the carbon dioxide were twice as abundant as at present is in the circumstances all the more telling. Briefly, the greatest differences of temperature are at the poles, but also at mid-latitudes, while precipitation is everywhere increased. Inevitably, to the modellers, calculation has been seriously constrained by the speed with which even NCAR's computing resources can handle climatic fluctuations from one season to another. And this, in a sense, is merely an equilibrium calculation. Rapidly changing systems should be inherently more difficult to calculate accurately, although it may then be possible to omit some slowly-changing components of the climatic model.

Schneider *et al.* have shown (*Nature* **308**, 21; 1984) that a different version of the community climate model can indeed be used to simulate nuclear winter; they decided at the outset to take the properties of the atmospheric soot layer as given. But that, of course, is where much of the argument about nuclear winter centres. Will the layer of soot be uniform and, if not, will it be stable? How long, in any case, will it last? To ask these questions is not to suggest that the nuclear winter is a kind of hoax but rather that the nagging difficulties in the original account persist — the National Academy's report clarifies but does not answer them.

For what it is worth, the same conclusion seems to apply even to the more fully studied problem of carbon dioxide. The same issue of the same journal gives an account of accurate measurements carried out in the past decade at three Canadian stations of the atmospheric concentration of carbon dioxide. The detail is absorbing, but C.S. Wong *et al.* (*J. geophys. Res.* **89**, 9527; 1984) also confirm what others have recently suggested, that the rate of increase of carbon dioxide (at 1.4 parts per million per year) is between a third and a half of the rate measured in earlier decades. The reasons for this change are entirely unknown, but presumably involve interactions with either the sea or the biosphere. Washington and Meehl's work is no doubt still valid, but it will be longer before it comes into its own. **John Maddox**