Is the "nuclear winter" real?

The discussion on the so-called "nuclear winter" is continued here, with further contributions to come in future issues of Nature.

THE article by Covey, Schneider and Thompson¹ breaks new ground in the atmospheric modelling of the possible effects of nuclear war, but despite that article and that of Turco *et al.*² two crucial questions remain: Will there really be a major cooling of the continental land masses (down to some -40°C)? And would such a cooling effect be maintained for months or seasons rather than days or weeks? Here I highlight outstanding uncertainties that need investigation.

Covey *et al.* start their model calculations by assuming a particulate layer in place in the atmosphere as a result of conflagrations of cities and forests following a nuclear exchange. This layer of smoke and soot is assumed to be more or less uniformly distributed over altitudes of 1 to 10 km; thick enough (optical depth = 3.0) to cut off the (shortwave) solar radiation from the Earth's surface but optically thin in the infrared (IR) (so that the surface can cool by radiating longwave radiation into space). In other words, they accept the physical model of Turco *et al.* and, not surprisingly, get a similar answer.

I have the following points to make about the certainty of these effects.

(1) The temperature change of the land surface (assuming small heat capacity) depends on a temporary imbalance between two very small fluxes: the inflow of solar radiation and the outflow of IR radiation, as well as the inflow and outflow of other energy fluxes. One cannot be sure even about the sign of the temperature change until all energy fluxes have been fully specified. Turco *et al.* and Covey *et al.* consider only a severe reduction of visible solar radiation, and explicitly assume that the particle layer would be optically thin in the IR, would not affect IR transmission so would not create a "greenhouse" effect.

(2) But the size and sign of the temperature change depend crucially on the ratio of the optical thicknesses (in the visible and the IR), and thus not only on the total amount but also on the size distribution of the particulates, especially on their detailed optical properties. The particle size distribution would be modified by agglomeration which is most important initially, when the smoke, soot and ash clouds are dense. For the same initial mass, the coalescence of smaller into larger particles will increase optical depth in the IR and decrease it in the visible.

(3) The conflagration would probably generate a variety of complex gaseous combustion products with absorption bands throughout the IR region.

(4) Water droplet clouds (located below the hypothetical particle layer) would trap

and re-emit IR radiation emanating from the land surface below.

(5) About the generation of water vapour there can be no doubt, although the phenomenon was not considered by Turco *et al.*

The combustion of dry mass must generate a corresponding mass of water vapour, since (CH_2) is oxidized to form H_2O . A total dry mass of 2.2×10^{13} kg (see ref. 3) must lead to $\sim 2.5 \times 10^{13}$ kg of water vapour. The amount of water vaporized by the nuclear explosion itself is small: the energy of a 1 Mtonne explosion ($\sim 4 \times 10^{15}$ J), if converted at the rate of 1 per cent, would lead to 1.6×10^7 kg of H_2O . Quite important, however, is the conversion of the energy released in the combustion: $(2.2 \times 10^{13}$ kg of dry mass) $(14 \times 10^6 \text{ J kg}^{-1}) \approx 3 \times 10^{20}$ J. A 25 per cent conversion would generate another 2.5×10^{13} kg of H_2O .

If the fire spread over an area of $\sim 10^6$ km², (ref. 3), it would produce from 2.5 to 5 g cm⁻² of water vapour (2.5 to 5 cm at STP), adding substantially to the normal water content of the atmosphere of about 2.5 g cm⁻². Such a large release of water vapour is possible, depending on the state of the forest, soil moisture, existence of streams and lakes, etc. In forest fires steam is produced copiously, as well as clouds, making it difficult to spot them from IR detectors on weather satellites^{4,5}. Eventually the water vapour will spread over a larger area, making it less effective as an IR absorber.

(6) Another neglected heat source, warming the Earth's surface, is the combustion energy of the very materials that contribute to the smoke and soot. The oxidation of wood releases $14 \times 10^6 \text{ J kg}^{-1}$, and 1 kg m⁻² daily corresponds to nearly 200 W m⁻² — the average amount of solar energy at a cloudless low-latitude location. A smouldering combustion of only 1 ounce of material per square foot per day would generate about 50 W m⁻², many times the minimum solar energy (8 W m^{-2}) calculated by Turco *et al.* for their baseline case. This would be sufficient to keep the surface temperature from falling too low.

Thus surface temperatures are unlikely to fall very low. They could even increase if particle size distribution, water clouds and combustion gases are such as to throttle the loss of heat radiation from the surface. Most probably, temperatures will decrease by a few degrees (rather than tens of degrees).

I now turn to a discussion of the lifetime of particulates in the atmosphere.

(i) The one-dimensional model of Turco *et al.* cannot capture mesoscale effects in the

atmosphere. Nor, unfortunately, can the global three-dimensional models of Covey *et al.*; their resolution is too coarse. Yet mesoscale effects are likely to determine the residence time of the smoke and soot particles. Experience with mesoscale models⁶ argues strongly against the existence of a stable layer in the atmosphere where the solar energy is absorbed. On the contrary, one would expect violent convective activity even if the layer were initially uniform, leading to cumulus-type formation, thunderstorms, rain squalls and accelerated cleansing of the atmosphere.

(ii) Further convective activity (plus rain showers and atmospheric cleansing) comes from the strong temperature gradient at the ocean-land boundary. Again, the three-dimensional model cannot capture these mesoscale effects, although the induced monsoonal circulation is yet another factor which limits large temperature excursions on the continents.

(iii) With the residence time limited by mesoscale effects, it is proper to ask if there is enough time to disperse the particulates uniformly through the global atmosphere, or at least the Northern Hemisphere, as explicitly assumed by Turco *et al.* and Covey *et al.* The latter suggest that the radiative heating of the particles may affect the atmospheric circulation so as to speed up their dispersion. This may well be the case; although in the absence of a fully interactive, self-consistent calculation, one cannot be sure how important the effect is.

Perhaps reasonably uniform dispersion can be achieved before rain-out cleanses the atmosphere. More probably, one will see patchy clouds which thin out rapidly — hardly a cataclysmic nuclear winter.

S. Fred Singer

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3. Crutzen, P.J. & Birks, J.W. *Ambio* **11**, 115-125 (1982).
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5. Matson, M., Schneider, S.R., Aldridge, B. & Satchwell, B. *Fire Detection using the NOAA-series Satellites* NOAA/NESDIS Tech Rep. April 1984.
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COVEY AND CO-WORKERS REPLY — Dr Singer's comments on the plausibility of "nuclear winter" deserve serious consideration, perhaps in more detail than limited space allows.

We emphasize again that the work reported in our article¹ was designed to examine how a three-dimensional climate model would respond to a hypothetical war-generated smoke cloud similar to that

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