Lawrence Livermore National Laboratory

September 4, 1984

Dr. Herbert Friedman
National Research Council
Commission on Physical Sciences,
Mathematics, and Resources
2101 Constitution Avenue
Washington, DC 20418

Dear Dr. Friedman:

My apologies for the delay in responding concerning the review of the report "The Atmospheric Effects of Nuclear Explosions;" I was on travel in Europe for most of August.

Although I have enclosed a substantial list of specific comments and suggestions, mostly on newly prepared material, the vast majority are simply intended to further tighten and complete the arguments rather than being critical of the thrust of the arguments. My congratulations to the committee on this draft--it is greatly improved and should be issued. The majority of my previous comments were completely addressed. The report much more carefully and completely presents what is known and what the uncertainties are, laying an excellent framework for the further research that is needed.

The only general comment that I would make is that the potential interactions between the various individual influences (smoke, dust, and chemistry) seem to be underplayed. While little is known about what these interactions may be (e.g., smoke or dust leading to stratospheric perturbations which influence the ozone distribution, recovery time, etc.; or the ozone reduction in the stratosphere changing its stability in a way that might allow even greater upward spreading of the smoke), it would seem that the need to investigate such synergisms should receive some emphasis.

Again, my compliments to George Carrier, the committee, and Larry McCray.

Sincerely,

Mike MacCracken

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MCM/nb
P.S. You may identify these comments as mine. Responses to each comment are not expected.

SPECIFIC COMMENTS AND SUGGESTIONS

Page 1-3, line 4: What is Moran (1982) reference?

Page 1-5, line 17: The range of 2 to 6% seems much too narrow. Some intense fires give well less than 1%, some synthetic materials give substantially more. I would suggest perhaps 1-8% or at least indicate what level of confidence (e.g., 50%, 90%) to apply to the 2-6% range. Alternatively, just indicate that 4% is your choice and probably accurate within factor of 2 to 4.

Page 1-6, line 5: (1) suggest adding "smoke from the ensemble of fires," (2) change "at the beginning" to "after initial mixing" to allow for residual mixing during first day after injection. (3) clearly indicate here whether you mean uniformly in mass or in density.

Page 1-6, lines 15-17: You should mention that this finding assumes an unperturbed atmosphere (or at least stratosphere), which is unlikely given the smoke injection.

Page 1-6, line 23: Change "heat" to "sunlight." Net downward longwave radiation below the smoke must increase, even if the smoke has no optical depth in the infrared.

Page 1-7, line 11: You might alter to say, early (local) and intermediate (mesoscale) scavenging.

Page 1-10, line 8: True, but there are some theoretical limits to "greater than." For example it is very hard to get hemispheric average temperatures (e.g., in a 1-D model) below about 235 K.

Page 1-10, line 21: You might add "event and the relative role of solar radiation in controlling that season's climate."

Page 1-12, lines 26-27: Your initial baseline <u>hemispheric</u> (versus mid-latitude) loading with an extinction optical depth of 3.5 will not reduce the light level at the surface by 99% everywhere or at all times of day, given scattering and the diurnal cycle.

Page 1-13, line 19: You might change "that" to "many of which."

Page 1-14, lines 1-4: You should point out that this analysis (i.e., focusing only on the stratospheric contribution) assumes an unperturbed atmosphere. A larger fraction of the dust could be important if tropospheric stabilization occurs and to treat particle coagulation in the troposphere.

Page 1-15, lines 2-3: The quantitative results assume an unperturbed atmosphere (as well as being 1-D calculations) and a qualification should be added that the situation will likely be different in a perturbed atmosphere.

Page 1-16, line 25: Add: "((a) through (e)) <u>and more</u>."

Page 2-2, line 5: "aerosols <u>and their evolution in time</u>."

Page 2-2, line 6: "precipitation <u>and cloud cover</u>."

Page 2-2, line 8: 1-D models can't do meteorology (as indicated in item (c)), just temperature, but they can do sensitivity studies regarding (e).

Page 2-2, line 11: 2-D models can do all that 1-D models do (so they can do item (c) and sensitivity studies regarding (e)) and some can look at item (f). The words "neglect of" should be changed to "do not explicitly represent" since the such models do attempt to represent the effects of the eddies, but, especially for this problem, their parameterizations are highly suspect.

Page 2-3, line 24: Add "tests <u>in low and high latitudes</u>."

Page 2-3, line 26: Add "debris <u>and thus avoided early rainout</u>."

Page 2-4, line 2: Add "troposphere <u>and in mid-latitudes</u>."

Page 2-4, line 6: The Erice proceedings is now published, so the Knox (1983) reference should read: "Global scale deposition of radioactivity from a large scale exchange," International Seminar on Nuclear War, 3rd Session: The Technical Basis for Peace, held at the "Ettore Majorana" Centre for Scientific Culture, Erice, August 19-24, 1983.

Page 2-4, lines 12-17: Also might mention need to consider various soil types, wetness, etc.

Page 2-5, lines 1-3: Also need to consider ozone issue in perturbed atmosphere.

Page 3-2, line 23: Do you really need to mention Vietnam? Is it a key ally?

Page 3-5, line 24: Add "the <u>present</u> estimation."

Page 4-1, line 22: Given you cite a factor of 3 to 4 uncertainty in the submicron fraction, shouldn't the range 10 to 24 be larger?

Page 4-5, line 12: Figure 4.3 goes back at least to Peterson (1970), <u>Health Physics</u>, <u>18</u>, 357-378.

Page 5-4, lines 11-13: But such fires are also much cooler since not as large, hence likely emit more smoke.

Page 5-4, lines 20-21: Suggest deleting "all parts of the world" as unnecessarily argumentative.

Page 5-5, lines 9-11: But small particles do coagulate to larger ones, that are more effectively scavenged.

Page 5-7, line 9: I was under the impression that Eric Kraus' report of smoke color was not "black." You might say "dark" instead.

Page 5-8, line 7: Add "humidity, moisture, firebreaks and topography."

Page 5-21, line 8: The population of 750,000,000 people being in 1100 cities with populations greater than 100,000 in just the Warsaw Pact and NATO countries seems high. The combined total populations of USSR, Europe and US/Canada is about 1,000,000,000. I thought urban fraction (towns > 2500 people) was about 2/3; a better reference than to Turco's report would be helpful. (More references for the many statements on this page would be helpful).

Page 5-22, lines 8-16: While these substance may burn and emit particles, aren't most of these essentially non-absorbing, which would make them relatively unimportant, especially compared to oil and natural gas emissions which might be quite important. It would be helpful throughout to rank importance of sources based on the optical absorption of the smoke they produce (this would play down forest and grassland fires, and again emphasize importance of urban fuel loadings, particularly of non-wood organics).

Page 5-44, lines 7-8: "essentially all" should be reworded, as it is essentially always true in terms of particle number and thus only meaningful when referring to particle mass fractions.

Page 5-44, lines 16-21. A factor of 2 variation in extinction coefficient would be very important. Indeed, early coagulation does not seem to alter the size distribution to bring the extinction coefficient down substantially, but does prepare the aerosol for later coagulation that will do so.

Page 5-62, line 13: Just as solar radiation could increase plume height by heating during the day, strong infrared cooling at night (when some fires will certainly occur) could lead to lower altitudes, or even plume collapse.

Page 5-63, line 26: Uniformly by mass usually means gm/gm; uniformly by density usually refers to gm/cm^3. The wording here could be improved.

Page 5-64, line 17: Isn't scavenging below 4 km mainly of the larger particles, leaving the smaller particles? Hence the air is "cleaner" rather than "clean."

Page 5-66, line 20: How can one have 63 ± 53%? Does this mean 10-100%?

Page 5-77, line 10: "no" should be "not."

Page 5-81, line 5: What is the basis for saying that tropospheric average smoke optical depths of about 1 (implying absorption optical depth of about 0.3-0.5) would lead to a major perturbation (e.g., a nuclear winter). Cess has run $\tau = 1.5$ in the OSU/GCM at Livermore and found greatly reduced effects in July, and the effect would be even less at other times of the year.

Page 5-81, lines 12-13: The effects can hardly be more severe than Turco et al. found (based on theoretical limitations to temperature change) whereas they can be much less with just a few factors of 2 (e.g., half the smoke and half as dark, etc.). The phrasing might be altered to say that there are as many (or at least as many) uncertain factors that could maintain (or sustain) the findings of severe cooling as there are that could moderate it somewhat.

Page 5-1-3, line 1: How about "directional shifts in the wind."

Page 5-2-4, line 25: What does "15 min" mean? Is it 15 minutes worth of global scale evaporation? Based on 1 m/yr of rainfall (and evaporation), I get closer to 1 hr for 40000 Tg.

Page 5-2-7, line 18: We have also simulated smoke movement in a 2-D model and, given that you cite Haberle as a private communication, you can cite MacCracken that way (1984, NRC/CRC Meeting). We also have some preliminary indication of it in our model that couples the OSU/GCM to the new 3-D version of the GRANTOUR trace species transport model, again not yet written up, but presented at Erice in August 1984.

Page 5-2-7, line 23: The kinetic energy of winds can also be returned via adiabatic compression (descent), which I suspect is more important.

Page 5-2-10: My impression was that the average atmospheric water vapor burden was 2.5 g/cm^2 (e.g., see Sellers' book), which gives almost twice the Table's number of 7.1 x 10^6 Tg on a global basis. Somewhere your numbers are off, probably a result of there being both larger area and higher mixing ratios in the tropics. I would suggest clarifying the discrepancy, because your global water vapor burden is much too low (for example, for average precipitation rate of 1 m/yr, it gives a water vapor lifetime of 5 days, which is a factor of 2 too short). Once you correct this factor of 2, a few corrections in the text are needed.

Page 6-13, line 26: The Luther (1983) paper is now available in the Erice proceedings. The reference is: "Nuclear war: short-term chemical and radiative effects of stratospheric injections," International Seminar on Nuclear War, 3rd Session: The Technical Basis for Peace, held at the "Ettore Majorana" Centre for Scientific Culture, Erice, August 19-24, 1983.

Page 6-14: Luther's study also indicated that it would be difficult to get a very large ozone hole with explosions in the 1-Mt range. You might also note here that this large explosion excursion is, as you said earlier, not particularly likely.

Page 6-23, lines 15-18, lines 15-18: Is the additional attenuation significant? It is not obvious that a slight vertical redistribution of absorption would be significant. Remember that the soot (about 1 g/m^2) would be mixed vertically (by wind-induced motions) through about 100 m depth.

Page 6-26, line 1: What does "significant local contamination" mean? How would effects compare to radionuclide hazard, etc.? A statement such as is made without some contextual framework is not very useful.

Page 7-1, line 15: Spectral techniques are really only used to represent horizontal transport; other processes (e.g., radiation, convection, etc.) are done on a grid. Thus spectral techniques are only a method of solving equations for a (carefully located) grid of points.

Page 7-2, lines 1-4: I (MacCracken) reported initial calculations of this type at Erice in August 1984. We've not yet adequately validated the scavenging algorithm, but we are moving material around in such away that the moving smoke then affects the radiation which affects the dynamics, which controls the precipitation fields that scavenge the smoke. Two particle sizes were treated, which were scavenged differently, but did not transform from small to large particles. The reference is: MacCracken, M. C. and J. J. Walton, "The Effects of Interactive Transport and Scavenging of Smoke on the Calculated Temperature Change Resulting from Large Amounts of Smoke," presented at the 4th Session of the International Seminar on Nuclear War, Erice, Sicily, 19-24 August, 1984.

Page 7-2, lines 6-8: The OSU GCM of Gates includes diurnal variations, and Cess, Potter and Gates reported the results of smoke calculations using the model with and without diurnal variation at the Erice (1984) meeting. Reference: Cess, R. P., G. L. Potter, and W. L. Gates, "Climatic Impact of a Nuclear Exchange: Sensitivity Studies Using a General Circulation Model," presented at the 4th Session of the International Seminar on Nuclear War, Erice, Sicily, 19-24 August, 1984.

Page 7-2, line 11: "cannot now be included <u>in detail</u> in these models." We (and others) are trying to parameterize these processes.

Page 7-3, line 21: Actually "by the radiation perturbations induced by the particulate clouds." (Except for cloud microphysical modifications).

Page 7-4, line 19: Much of the North American smoke will form plumes over the North Atlantic Ocean.

Page 7-5, lines 18-19: The North Atlantic gap would indeed fill, but likely at that time would be well over Europe. The gaps advect too.

Page 7-6, line 6: The dust injection is irrelevant for how the figure is described, since there was no interaction between the smoke/dust and the windfield in this calculation; the dust did later, however, affect the temperature. You should also mention that this figure moves smoke using tropospheric average winds. At Erice (1984), we (MacCracken and Walton) did a similar scenario with a 150 Tg injection with both control (unperturbed) and interactive winds with 3-D transport and the OSU model. The results, especially at such early times, are quite similar.

Page 7-8, lines 4-7: Marginally correct; if the absorption coefficient is a factor of 2 less, then the temperature decreases are shorter in duration and the vertical distribution of absorption is quite different. The statement is also marginally true only so long as all other factors (e.g. injected mass) are the same. With statements such as this you could individually dismiss many factors of 2 that might accumulate.

Page 7-9, lines 18-21: You need to say that these radiative calculations assume annual and diurnal average daytime radiation at 30° latitude. Doing a diurnal calculation would substantially increase the diurnal average solar radiation reaching the surface, as calculated by Cess.

Page 7-10, lines 5-7: Indeed, and this is why conditions cannot get much worse than TTAPS stated (see comment concerning page 1-10, line 8 and page 5-81, lines 12-13).

Page 7-10, lines 18-20: And at very early times when optical depths are very high (e.g., 20-50).

Page 7-10, line 26: I would disagree that 40 Tg is in the saturation regime; I suspect it is right on the margin. (For us, 15 Tg does nothing, except locally under dense smoke plumes).

Page 7-13, line 10: Unless one knows the jargon, it sounds as if rainout increases with height; you might say that "particle lifetime increases rapidly with altitude."

Page 7-15, lines 18-20: You should state, perhaps in the caption to Figure 7.5, that the total optical depth includes both smoke and dust. Because TTAPS didn't make that clear or Aleksandrov didn't read carefully, he used the total optical depth (dust plus smoke) as the absorption optical depth (making two errors, dust = smoke, and then smoke is totally absorbing). Try to prevent others from doing likewise.

Page 7-16, lines 9-11 and Figure 7.6: It would be better if you plotted temperature change (i.e., start from 0°) rather than temperature, since 286 K is a rather cold starting point for the summer case that NRC is considering. For example, typical mid-latitude continental summer temperatures are 25-30° C rather than the 13° C starting point used by TTAPS. At least, this discrepancy should be mentioned.

Page 7-16, lines 21-22: You might also mention that the optical properties used by LLNL are within plausible bounds, but are not as absorbing as the TTAPS baseline; hence one cause of differences. Another relates to use of a different scavenging algorithm.

Page 7-17, line 15-17: Assuming substantial amounts of smoke extend above 4 km or so.

Page 7-18, line 22: Where was the "threshold" discussed?

Page 7-23, line 13: I believe Aleksandrov assumed the dust was smoke, based on my latest discussions with Stenchkov, but I may be wrong.

Page 7-24, line 3: "not unreasonable, <u>given his very large injection</u>."

Page 7-28: The fogs also provide heat to radiate (originally derived mainly from the oceans) through the condensation process. Thus fogs really can act as a heat source.

Page 7-32, lines 14-15: Given the NRC is emphasizing the summer case, why don't you show (or explain) the NCAR summer results, which are interesting but less dramatic? (They are available, I have a figure from them).

Page 7-33, lines 14-17: It is also evident in our 3-D interactive calculation (MacCracken and Walton, Erice, 1984) and in the MacCracken 2-D calculation presented at the NRC/CRC meeting in March.

Page 7-48, Table 1; you need to add that units are inverse seconds.

Page 8-6, lines 10-13: Loading the stratosphere and the troposphere would lead to quite different effects, depending on whether convection is suppressed, thereby disconnecting the surface and troposphere (as Cess suggests).

Page A-6; line 18: It is MacCracken (1983) and the reference is "Nuclear war: preliminary estimates of the climatic effects of a nuclear exchange," International Seminar on Nuclear War, 3rd Session: The Technical Basis for Peace, held at the "Ettore Majorana" Centre for Scientific Culture, Erice, August 19-24, 1983.