Lawrence Livermore National Laboratory

December 21, 1983

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

Dear Dr. Friedman:

Thank you for providing me the opportunity to review this very important report on "The Effects on the Atmosphere of a Nuclear Exchange." The aspects of this issue raised during the past two years concerning particularly the effects of smoke emissions deserve careful and measured review and explanation for the public, the government and the scientific community. I believe the report must be more a very initial, highly qualified assessment than a snapshot of the present understanding of the problem pretending that there is quantitative validity in the results. The length of the report itself seems to imply more certainty than is justified.

As a member of a project team investigating these questions for almost a year, I recognize the difficulty of balancing the importance of discussing the potentially very severe derivative consequences of a nuclear exchange with the normal degree of caution that scientists must take when early knowledge of a subject is limited and uncertainties remain large. My most important criticism of the present draft of this report, particularly the early chapters, is the failure to describe consistently the uncertainties and underlying assumptions regarding our present knowledge of the derivative effects of a nuclear exchange. Three particular examples are offered to support this criticism:

1. The emissions from fires. A though uncertainties concerning fires are described in some detail in the latter chapters of the report, the early chapters seem to automatically equate 6500 Mt and 200 Tg of smoke with virtually no indication of uncertainties. Just to mention a few influences that could reduce emissions and that did not get discussed elsewhere: double targeting to assure destruction of key facilities, targets that are partially surrounded by water, damping of fires by meteorology, topographic sheltering of sub-areas of the target, etc. Many processes, e.g., close-in scavenging, are barely treated although they could be very important. Emissions could also be greater, of course. Why then is no measure of uncertainty attached to the nominal figure that is cited?

2. Smoke optical properties. A critical aspect of the smoke-effects argument is the question of smoke properties. The range of uncertainty here, especially given the lack of observational evidence, is quite large, particularly if one considers the aging of the smoke in the atmosphere. Roessler et al., 1983 (Applied Optics, 22, 3648-3651), for example, find variations of about a factor of 10 in the absorption index when soot and dust mix; others are finding that smoke may scatter more than absorb. The uncertainty introduced into effects estimates by the assumptions of optical properties has not been included.

3. Climatic effects. We have so far only the very crudest indications of what will occur, with all model studies so far affected in probably large, but unknown, ways by their assumptions, simplifications, and tuning to the present climate. Turco et al., for example, do not allow an increase in (water) cloud amount during the cooling, do not assess early time IR effects of the smoke when optical depths may be 20 or more, do not assess IR effects of the simultaneously lofted gaseous species, diffuse particles despite lower atmospheric stability, assume uniform particle density in the vertical (which appears to rest implicitly on an assumption that there is a tendency to relatively intense fires, despite evidence cited from Hiroshima and Nagasaki), do not treat seasonal variations, and provide no verification that the quantitative estimates of their model, although useful in providing a basis for further work, can be believed for such a perturbation. Potential three-dimensional effects, e.g., baroclinic interactions, are just beginning to be investigated and must be treated. Uncertainties are extremely large, and unknown. Particularly troublesome is the failure to emphasize the nonlinear characteristics of the problem. At the "lower" end of the spectrum of assumptions, the fires and dust might not dramatically alter the climate; at the upper end they may sustain the effect for a very long time (through reduced scavenging). Only in the middle range have the various sets of results been shown to be similar, and this should be stated.

In addition to the report's weakness in describing the uncertainties, the breadth of the report is limited by the consideration of only one nuclear exchange scenario. While the baseline scenario is not physically impossible, there are a wide range of other possible scenarios involving different target sets, duration of the exchange, total yield, number of explosions, etc. The estimates of climatic consequences may indeed be relatively insensitive to such variations if a very large number of cities are targeted, but the present draft did not seem to adequately convey that the consequences of all possible scenarios are not alike and that the contention that all nuclear wars are global suicide (as seems implied on page 2-1, lines 17-22) is not scientifically substantiated. In particular, exploding about 50% of the total yield in the arsenal in essentially one day does not represent only a moderate nuclear war. Given system reliability, inventory structure and availability, and military procedures, it is very hard to conceive of a more severe conflict. It is also not completely clear how the report can estimate the potential effects of the 6500 Mt scenario, when the various quantitative studies are not based on this assumption.

This plea for much more qualified treatment of the problem is not intended to make the potential problem seem less important, but rather to indicate that the range of possibilities, as delimited by our knowledge of the physics of the problem, is still very large. It would seem to me that, given the limited state of knowledge, the main point that the NAS should be making in this report is that, with plausible assumptions, the smoke issue could be quite serious and must be carefully studied. The thrust of the present introductory chapters (and the committee scope) could be made more in-line with this recommendation by replacing the word "would" (e.g., page 1-1, line 26: The total amount of smoke . . . would be; page 1-2, line 25: would be some tens of degrees . . .; page 1-3, lines 3-5: no reason . . . removal . . . would be more rapid; page 2-2, line 7: smoke would consist) with phrases indicating the uncertainties involved. At the least, every declarative sentence should be referenced to available articles or sections of the report. Another possibility in this regard might be to combine the "Summary" and "Conclusions" sections, deleting the didactic character of the present summary. The present phrasing ascribe too much credibility to our level of understanding. As Jonathan Katz' exception properly notes, the certainty attributed to present results is premature.

With respect to the three particular areas you recommend be examined carefully:

- Scope: As indicated above, I think there is too little qualification included with the numbers.

- Southern Hemisphere Effects: There is too little known to say much of anything on this subject. All multi-dimensional models that hint at such effects have very important short-comings. Rapid transfer may be possible, but at present such suggestions are only educated guesses.

- Particulate Removal: I am not sure that this subject can yet be quantitatively evaluated, although that did not seem to prevent estimates of other equally unknown aspects of the problem. This may be the issue that ultimately is most important in assessing the potential impact on human activities (because it determines the length of the climatic modification and it should be discussed in terms of what reasonable sensitivities in this process imply about the duration and intensity of the effect).

The experience gained since the 1975 NAS Report on this subject indicates that it will take a long time to resolve uncertainties (we are only now moving from one- and two-dimensional models of ozone chemistry). The smoke problem is both more important and more complex, having even greater uncertainties. It seems doubtful to me that quantitative estimates will stand the test of time, and therefore, the focus of the report should be on the potential seriousness of the issue and the work needed to understand it better.

A listing of specific comments, over and above those just given, is attached.

Sincerely yours,

Michael C MacCracken

Michael C. MacCracken
Deputy Division Leader
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MCM/fw

Attachment

SPECIFIC COMMENTS

Concerning Overall Draft

1. For such an important subject, there is an extraordinary reliance on talks presented at meetings, references not yet published, reviewed, or, in some cases, even available in draft form, much less references that have stood the test of time and reworking by independent investigators. Drawing definitive conclusions at this point seems very premature.

2. There should be a clear indication where every number came from and the various assumptions that went in to formulating it. The simple multiplications that have been done should be clearly stated.

Page 00801-7

line 7: The first charge to the committee seems impossible to meet at this time, despite the committee's efforts. There has simply not been enough adequate work to provide ". . . a quantitative description of the more important of the changes that would characterize that modification [emphasis added]." Even with the caveat about current knowledge, "could" would be more appropriate than "would."

line 23: What is "long-term"? Months, years, decades, etc.?

Page 00801-8,

lines 6-7: The statements concerning "prior preparedness" are highly scenario and location dependent. General statements seem premature.

lines 8-9: I would suggest "may expose" and "may visit." Again, such threats are scenario and location dependent.

lines 14-15: This is one of the only places in the first few chapters where uncertainties and difficulties are mentioned.

Page 00801-9

line 12: 6500 Mt involving 25,000 explosions may be a "moderate" fraction of the inventory, but it is much more than a moderate nuclear exchange. This line should clearly state that this is essentially a full exchange of deliverable warheads (given readiness, failure rates, availability of delivery systems, defenses, etc.) involving no restraint in target choices. To say it is not "extreme in any respect" seems incredible.

lines 16-17: I do not consider all parameter choices as not being extreme. Assuming zero overlap of explosions might well be viewed as choosing a parameter at an extreme value. 1500 Mt on urban areas is a higher percentage of yield than Turco et al. choose in their comparable scenarios, and in terms of total yield seems quite high.

line 24: What "data" were not available for the previous study? It sounds as if we have done atmospheric testing since 1975, which is, of course, not true.

Page 00471-1-1

line 7: This whole page and the rest of the summary is filled with assumptions, yet only on line 7 is this indicated. Each place where the word "would" appears an assumption has been made based on much more detailed analyses later on. We are not, however, told here that these are assumptions. The summary needs rewriting without the word "would".

Page 00471-1-2

line 1: What multiplication of factors gives 18 Tg. This deserves a clear explanation somewhere in the report.

line 5: 18 Tg gives 10% reduction in what (direct, direct plus diffuse, net, downward) over what area (30-70, hemisphere, globe)? A comparison with El Chichon would be helpful.

lines 7-10: The question of early-time "ozone holes" is not covered at all by NAS; the uncertainties of a one-dimensional model deserve attention.

lines 16-17: I believe the "one part in one thousand" applies to direct solar beam assuming no scattering. Little direct solar radiation gets through thick clouds (probably 1 part in 10^9). Scattered radiation may be important depending on optical properties of smoke -- or it may not.

line 20: Insert "significant amount of smoke".

line 25: Temperature change is not even as certain as mentioned here.

Page 00471-1-3

line 3: "no reason" should read "it is unlikely that." The issue has received little attention.

Page 00591-2-1

line 7: Again the word "would." Try "could" or even "may be able to."

line 13: You state uncertainties are "large," but don't explain how they may indeed alter the conclusions and should preclude definitive statements.

lines 17-22: Two points:

a. The statement implies that a much more thorough examination has been made than is actually the case. Present studies have only scratched the surface of what must be done.

b. It is not the atmospheric conditions themselves that are such a threat to the surviving population; it is rather the effect of the atmopsheric changes on our ability to gather and grow food that is the threat. It may well be that this threat could be greater and more immediate to those outside mid-latitudes where life is much closer to subsistence levels than in mid-latitudes where stockpiles are greater. Given the earlier disclaimer about the lack of expertise on biological issues represented on the panel, it is somewhat surprising to see the conclusion phrased in this way.

Page 00591-2-2

line 7: The composition of smoke is only roughly known. This phrasing implies much too much certainly.

lines 9-11: This is all based on best estimates, not modeling.

lines 14-15: Earlier the report said 999 parts in a thousand would be removed, now only 99.

lines 17-20: Phrasing implies even this small (?) amount of 200 Tg is of concern and actual level could be much higher. The amount could also be much lower.

Page 00591-2-3

lines 10-12: Climatic effects are also sensitive to many additional factors (amount, time of year, optical properties, etc.).

line 26: Is light direct beam or direct plus diffuse?

Page 00591-2-4

line 1: Where does 2°C come from? How is this related to 10% change?

lines 5-7: If by upper bound you mean useful upper bound, I think "relatively easy" is an improper description (perhaps I should first ask relative to what?). In doing simple calculations, one must make many, many simplifying, and possibly incorrect, assumptions that make later quantitative use of the numbers very dangerous.

line 18: There should be a sentence on "ozone holes."

line 24: "over normal at mid-latitudes."

Page 00591-2-5

lines 6-7: "would be likely" is much too positive. NCAR results might be interpreted to say that "some preliminary calculations indicate that it may be possible for smoke clouds to drift . . ."

line 17: 1°C is too high, few tenths of a degree is more likely.

Page 00591-2-6

line 2: replace "several" by "many."

line 21: "usual" is a gross understatement. The problems in doing such calculations accurately are now very large. Try "much more than the usual."

Page 00601-3-1

lines 16-26: Given all these uncertainties, why are results phrased with such certainty? Add: (g) The initial transport and mixing of soot on the mesoscale and its feedback on dynamics and precipitation.

Page 00601-3-2

line 6-8: Three-dimensional GCM cannot treat the initial distribution of the soot and its mixing and transport on the regional scale.

Page 00601-3-3

line 17: The significance of the increase is not in the global average dose (even though current estimates are a factor of 10 greater than NAS (1975)), but in the much greater probability that there will be rain-induced hot spots that are much higher than the global, or hemispheric average.

Page 00521-5-1

line 3: All we've had to this point is an analysis in brief. Uncertainties should be addressed sooner.

lines 13-15: What does this mean? How can one spread anything over just the land masses?

line 20-24: Where does 18 Tg come from? From numbers given so far 0.03 Tg/Mt * 1500 Mt = 45 Tg.

Page 00521-5-2

lines 1-7: Range of 2000-6000 Tg seems too small, given variations in fuel loading, fire spread, condition of fuel, etc., etc.

line 25 through line 3 (next page): "It seems clear" usually means you can't justify the statement. You need to say "uniform" in what (density, mixing ratio, etc.). The assumption of uniformity in soot density (gm/cm^3) from 1 to 8 km would seem to imply, given that air volume expands as pressure decreases, that one must have a much greater number of high rising (very hot) fires than low rising fires (assuming all fires inject the same density of material at the surface). I infer this because high rising fires ultimately inject material at lower soot density (due to expansion) than low rising fires. Add to this the observational evidence that hot (high-rising) fires usually burn more efficiently than low-rising fires and one has real trouble achieving the uniform density assumption. This may not matter much, but it is likely a poor assumption.

Page 00521-5-3

line 17: Add at what time 40% reduction occurs.

line 19: about two years.

lines 20-24: J. Birks presented a paper at the December AGU agreeing that there were significant chemical consequences from smoke injection.

Page 00521-5-4

lines 9-11: Specify whether or not these are Tg C.

line 14: Specify whether or not this is 6.8 Tg N or Tg NO_2. Fires are thought to emit NO not NO_2.

lines 21-23: What is the basis for the statement of O_3 concentration increases at the surface? In calculations that combine smoke and gaseous emissions, surface O_3 concentrations are generally decreased. Under some assumptions a 60% increase is possible, but it's certainly not more likely than a decrease.

Page 00521-5-5

Line 15: The reluctance to quantify a potentially important moderating mechanism (given the willingness to quantify everything else) does give one pause. The next few lines indicate a tendency to dismiss moderating influences by claiming conservative choices elsewhere. Such bias should be avoided -- make your best choices everywhere if indeed you are going to make a choice rather than give a range.

line 23: The phrase "very preliminary" should be featured much earlier!

Page 00521-5-6

line 4: 90% of what? (direct and/or diffuse).

line 20: "land masses" or "continental interiors." Don't carelessly interchange.

Page 531-6-1

lines 16-17: Time scale insensitivity only applicable if time for exchange less than removal time for smoke in normal atmosphere.

Page 531-6-2

lines 2-8: I believe assuming one-half of the inventory explodes on target is very high. Our studies show that it is about the maximum possible to use. Was any account taken of available delivery systems for bombs, theater weapons, etc.?

Page 00531-6-3

lines 12-15: I believe adding detail could significantly alter the effect of smoke. For example, smoke from near-ocean targets might be subject to faster removal.

Page 531-6-5

line 13-14: Placing ≤ 1500 Mt groundburst on siloes seems exceedingly low. Many people assume a warhead is also airburst over the siloes.

lines 19-23: This statement is only true for the current, crude models. With more realistic models, we may find that more detailed knowledge of target locations is important.

Page 531-6-7

Misprint .05 to .15. I would have thought more of the larger warheads would be surface burst against hardened targets.

Page 541-7-1

lines 21-22: The explanation here is too brief, because it requires checking many later bits of information to reproduce the numbers. A clearer explanation would be helpful. The estimate of 12-30 Tg is also much less than Turco et al., indicating again how important various assumptions are.

Page 00541-7-12

lines 19-26: Is 1% of the total mass less than 1 μm or less than 10 μm? The two sentences seem contradictory.

Page 541-7-18

lines 1-4: These lines are very confusing. The atmosphere won't be "greatly modified" when the injection takes place. The last phrase of section makes no sense to me.

Page 541-7-20

line 4: Ramaswamy of NCAR quotes NAS as using r_m = .08 μm and gets quite severe climatic consequences. Is he greatly mistaken?

Page 00551-8-2

line 23: The area ignited is not linear in yield. It depends on the atmospheric transmission. In addition, the ignition fluence depends weakly on bomb yield (see Glasstone and Dolan).

Page 00551-8-5

line 12: Why assume little overlap? Is there any basis for such an assumption?

Page 00551-8-6

lines 2-5: The pictures painted here appear too clearcut. Isn't it also possible that winds might blow out many of the fires in this region, and that mass fire conditions would develop more slowly from those areas remaining ignited?

Page 00551-8-7

lines 5-6: Even though you indicate Hiroshima was among least intense fires, everywhere else this observation seems to be generally ignored.

lines 11-12: This remark seems to negate the picture given on page 00551-8-6.

lines 13-15: How does a crushed concrete city really burn?

line 24: Why is there doubt? Didn't Martin actually do both pieces of work?

Page 00551-8-11

What is fraction of cities hit by earthquakes where major fires start?

Page 00551-8-13

line 17: Do you mean ≥ 20 cal/cm^2?

Page 00551-8-13 and 14

line 26 to line 7: Did the several fires cover all of the flattened 1600 km^2? It appears that area burned is < 160 km^2/Mt, and maybe much less.

Page 00551-8-14

line 20: Why is aspect ratio a scaleable number?

Page 00551-8-15

lines 3-5: Presumably the nature of the fire (i.e., its intensity) determines the plume height. The injection height of firestorms and mass fires may differ and the resulting atmospheric effects will also differ.

Page 00551-8-16

lines 23-25: Is this true or an assumption? It would have been helpful to look at typical targets. Airfields, for example, are <u>not</u> in city centers.

Page 00551-8-17

lines 5-8: This remark is based on a large number of improbable events all occurring at once.

lines 9-10: Oil and gas production fields are unlikely targets.

Page 00551-8-18

line 15: "involved" or "susceptible to burning"?

line 26: There are also many photographs of light-colored plumes.

Page 00551-8-19

lines 9-11: These qualifications are not reflected in opening sections.

Page 00551-8-20

lines 6-7: It should be pointed out that the smoke emission rates and optical characteristics from a mixture of fuels cannot be characterized as a linear sum of the rates and properties of the individual fuel elements.

lines 18-20: Is there really any thought that a mass fire, which requires oxygen to continue its burn, could emit more smoke than one that only smolders? All the evidence from foresters points to the opposite conclusion.

Page 00551-8-21

line 3: The smoke emission rate of 5-10% seems high. Factors vary from 0.25% to 6.3% in forest fires. Considering that the major urban fuel is dry wood, with emission rates at about 2%, these numbers seem high.

lines 4-5: Treatment of early-time scavenging is quite inadequate, yet it could possibly wash away the effect, particularly in summer when atmosphere is moist and barely stable.

Page 00551-8-22

lines 1-5: These factors will probably change for mixtures burning at once.

lines 5-6: This statement does not follow from the arguments made thus far. For example, forest fire smoke properties have not yet been discussed. In addition, the fraction of fuel that is fossil, fiber and plastics has not been discussed or the emission characteristics of mixtures.

line 15: The mean radius and mode radius are two different parameters. In the expression given r_m is the mean geometric radius.

Page 00551-8-23

lines 18-20: Was any consideration of these larger particle sizes given in the calculations? Turco et al. use 0.1 μm as a mean geometric radius. The optical depth is reduced by a factor of 5 to 30 if r_m = 0.5 μm instead of 0.1 μm (see attached figure).

Page 00551-8-24

lines 8-11: One cannot infer this from the evidence given. The dust taken up with the fireball is accounted for elsewhere. The argument made here is not clear.

lines 16-18: You should also point out that this effect substantially diminishes the climate impact. This is not pointed out anywhere in this report.

Page 00551-8-26

line 20: What is the imaginary index?

lines 22-26: One must be very careful in estimating optical properties of the smoke. One cannot just look at the combustion products of different fuels separately and add them up because when a mixture of substances burns it may give off a different set of products. It thus is probably very dangerous to talk in terms of equivalent elemental carbon and to do linear combinations. Also, since the contamination of carbon particles can change the optical properties depending on how this occurs, one cannot just add up carbon amounts.

lines 24-26: Recall earlier section where this hypothetical composition is given as the definitive composition.

Page 00551-8-27

lines 8-11: Use of "mode radius" here is incorrect. According to the formulas presented earlier, r_m is a geometric mean radius. A factor of 2 uncertainty in the specific extinction is a large uncertainty in the light attenuation and climate effect. This should be made clear.

line 13: "mode" radii is used for r_m again? Or is it really meant to be mode here?

Page 00551-8-28

lines 15-18: What basis is there for assuming $\leq$ 20% is scavenged? Turco et al. cite a reference (Chen and Orville, 1977) for their assumption of 20% scavenging that does not support their conclusion! In fact, their abstract says, "Results of this numerical study are not encouraging for the direct formation of cloud lines by the spread of carbon black dust in the tropical atmosphere, unless the atmosphere is much more humid than normal. No conclusions concerning mesoscale effects of the solar heating and indirect formation of cloud lines are possible within the framework of this cloud-scale model." Apparently, no calculation of mesoscale scavenging are available.

Page 00551-8-29

lines 5-6: The range for backing fires is 0.25 to 1.1% according to Tangreu, McMahon and Ryan, "Contents and Effects of Forest Fire Smoke," Southeastern Forest Fire Experiment Station, USDA Forest Service, Macon, GA. The central value of 4% used by the committee therefore seems high.

Page 00551-8-30

line 8: Why isn't published work of Crutzen referenced rather than unpublished work of Turco?

Page 00551-8-31

lines 1-3: Can't background soot concentration actually be closer to 1 $\mu g/m^3$? Seems to be required if absorption optical depth is to be about 0.01.

lines 7-19: Why no references to Arctic soot, LBL group (Novakov), etc.?

lines 14-19: This argument is not consistent with the possible inducement of "black rain" argued for earlier.

Page 00551-8-32

lines 11-12: Using one reference to conclude something is well-established always troubles me.

lines 14-16: Plausibility should be demonstrated by analysis.

line 19: This section deserves more discussion of ranges, importance of assumptions, etc.

Page 00551-8-33

lines 7-14: This whole forest argument needs more explanation. Doesn't one really attack cleared targets surrounded by forests (e.g., airfields, etc.). Why should one believe a random correlation?

line 15: 50% may be moderate in summer, but not moderate for the annual coverage, which is what I thought the baseline case was.

line 25: Horray. "assumed" instead of "would be."

Page 00551-8-35

lines 14-16: Earlier you indicated Hiroshima was a less intense fire than Dresden, so why use European city fires to determine injection altitude? This doesn't seem "moderate" to me.

line 19: Less than what?

Page 00551-8-36

lines 2-3: It would be helpful to explain the importance of this arbitrary assumption. If all smoke were injected very low, one could get warming.

line 22: If 75% of urbanized area is not extreme, given variety of urban shapes, topographic sheltering, etc., I would be very surprised.

Page 00551-8-37

line 3: A range derived in this way is not an uncertainty. Uncertainties should be indicated. Note also that the range of climate effects from this range of emissions is even larger!

Page 00551-8-38

lines 12-14: But urban fires will depend on meteorology at time of explosion.

Page 00551-8-39 to 40

. The ranges quoted for (3), (4), (6), (7), and (9) are probably too small.

Page 00551-8-54

line 1: How is the word "Actually" justified? Do authors have new data?

lines 11-12: What fraction of material burned?

line 23: Many accounts from 1849 and 1868?

Page 00551-8-57

Table 8.1: It would have been helpful to have table with ranges. Alternatively, you might add uncertainties here rather than appear so definitive.

Table 8.7: Fuel consumption is kg/m^2. The smoke imaginary refractive index is much less than that used by Turco et al. and others. It would thus seem necessary to explain effect of this before using those calculations to estimate climatic effects.

Page 561-9-3

lines 13-16: I thought Kondratyev and Nikilsky had observations of NO_2 following nuclear tests in the 1960s.

Page 00561-9-4

line 26: A better explanation is needed. Total mass of gases, total mass of fuel, carbon mass? The next page uses another reference for percentage.

Page 00561-9-16 to 17

lies 26-3: This statement is incorrect if smoke and gaseous emissions are treated simultaneously. Even when emissions are extended for 2 months (the original Ambio assumption), surface O_3 concentrations are only increased by 60%.

Page 00561-9-18

lines 19-21: If fire plumes are rising high to carry smoke up, won't toxic chemicals go with smoke? If toxics and smoke stay low, then climate problem is greatly reduced. (See also p. 00561-9-19, lines 21-23).

Page 00561-9-20

lines 10-14: What about possible differences in optical properties?

Page 00571-10-2

lines 1-3: It is likely that the one- and two-dimensional models are more tuned than the three-dimensional models, particularly regarding transport algorithms. Thus, I doubt dynamical effects could be characterized in less than three-dimensional models.

line 11: No global models even parametrically try to represent detailed sub-grid scale effects.

line 14: GCM tuning in the vertical may be a problem, but one-dimensional models do it too.

Page 00571-10-5

line 3: To say that the one-dimensional results have been "fully exercised" and "completely analyzed" is wrong. A brief draft publication for Science or an unavailable and unreviewed draft for Rev. Geophys. Space Phys. does not answer all questions!

line 5: It would be interesting to see what Turco's one-dimensional model predicts for the effects of El Chichon using the same assumptions (i.e., mid-continental land only) or what it predicts for seasonal variations in hemispheric average temperature, etc. We should not believe that a one-dimensional model can quantitatively predict climatic sensitivity for large perturbations?

lines 14-15: The one-dimensional model doesn't do transport of particulate matter either. Neither of the three-dimensional runs do radiation well. All runs to date are very preliminary and inadequate to draw quantitative conclusions.

Page 00571-10-6

lines 2-3: This section contains virtually no discussion of shortcomings of models, especially the one-dimensional model. Assuming instantaneous hemispheric spread, for example, as Turco does, is likely very important.

line 17: "available" has not been a universally applied criteria.

lines 17-18: MacCracken (1983) attempts a better calculation than Turco, although still making the same very poor assumption of no change in scavenging or transport. His one-dimensional vertical tropospheric columns move around in a two-dimensional (latitude-longitude) meteorological environment; those results are very different than Turco's assumption of instantaneous hemispheric spread.

lines 21-24: Turco et al. assumed a mid-tropospheric rainout lifetime of 20 days in their April 1983 report. Although the rainout lifetime is not quoted in the more recent report sent to Science, the optical depth curves look similar, leading me to conclude the rainout lifetime was 20 days.

Page 00571-10-7

line 13: Dust would be moved to surface by vertical mixing and then deposited.

Page 00571-10-8

line 3: Here we have an indication of uncertainty ("order of magnitude") that should be emphasized earlier.

Page 00571-10-9

line 6: You need to emphasize that ocean buffering is not treated, as well as the other uncertainties inherent in the one-dimensional approximation. Turco et al. also use an imaginary index of refraction quite different than that in the NAS baseline.

Page 00571-10-10

lines 3-4: Turco uses a lapse rate at altitude of -6.5°C/km. At those altitudes the dry adiabatic lapse rate would be more appropriate.

lines 19-21: One only gets a tendency to isothermal conditions if the smoke cloud has a high IR opacity. Otherwise, simple calculations clearly show the atmosphere will be cooler than the Earth's surface.

line 26: Strongly absorbing in solar or IR? Please be precise.

00571-10-13

lines 10-12: This may be true in one-dimensional calculations when calculating maximum cooling, but does not apply in two- and three-dimensional calculations and is not correct when considering duration and extent of effect.

lines 16-18: My impression is that Turco injected > 100 Tg dust whereas NAS injects < 20 Tg. How can one then apply Turco's findings to the NAS scenario without at least scaling?

line 26 to line 2 (next page): Dust effect may be significant, but it is only about one-tenth the smoke effect. The two should be distinguished.

Page 00571-10-15

line 8: The caveats in this section should come earlier and be more closely associated with the apparent findings, or they will be lost in the transition from this report to the public.

line 16: The reference to Turco seems very strange to quote on this point, given that in their one-dimensional model results they do not account for ocean buffering, except by rather arbitrary percentages.

Page 00571-10-17

lines 13-14: Such ventilation doesn't occur in California's Central Valley -- the fog layer persists for weeks.

Page 00571-10-18

line 9: "sub-stratosphere" might be better term.

Page 00571-10-21

line 6: "cloudy" should be "smoke-laden."

line 8: "fully mixed over" should be "dispersed throughout."

line 23: "very inefficient," except that the strong cooling could create much fog and drizzle, which would lead to efficient removal.

Page 00571-10-24

lines 2-5: Effect occurs in large part because they do not move the smoke around.

Page 00571-10-26

lines 22-23: Only one-dimensional calculations, so far, attempt to account for these effects.

Figure 10.1

Why do lines 1 and 2 cross-over? Why do cases 2 and 4 cross-over?

Page 00571-10-33

It would be nice to have cases of heavy clouds added for comparison. The optical depths for 40 Tg dust seem high. What parameters were assumed in the Mie scattering calculations?

Page 00581-A-2

lines 20-21: Mt. St. Helens certainly had a quite powerful release, and other volcanoes certainly get enough heating to loft material to the stratosphere. I don't understand why volcanoes aren't reasonable analogs.

Page 00581-A-3

line 3: Volcanoes do emit radionuclides.

lines 8-9: I don't understand why the surface burst component of the scenario, which result primarily from explosions that are not widely dispersed, are not quite a good match to a volcano analog.

lines 21-23: Krakatoa certainly didn't cause a 1°C cooling, perhaps 0.5°C. It may, perhaps, as may have El Chichon, perturbed the weather in more important ways.

Page 00581-A-4

lines 1-6: What do one-dimensional models show for volcanoes, using the same assumptions? How does this match observations? Some discussion of verification would be nice.

Page 00581-A-5

lines 22-25: How is this calculated? 10^{15} g is about 100 times El Chichon, so El Chichon is 0.1°C, I presume. Robock gets 0.4°C after about three years. Please explain. Why would the effect just last several months?

Page 00581-A-6

line 14: I thought the NASA group suggested 10^{13} g for El Chichon.

Page 00581-A-16

Aren't the loadings cited comparable to NAS dust loading? Then why aren't volcanoes a good analog?

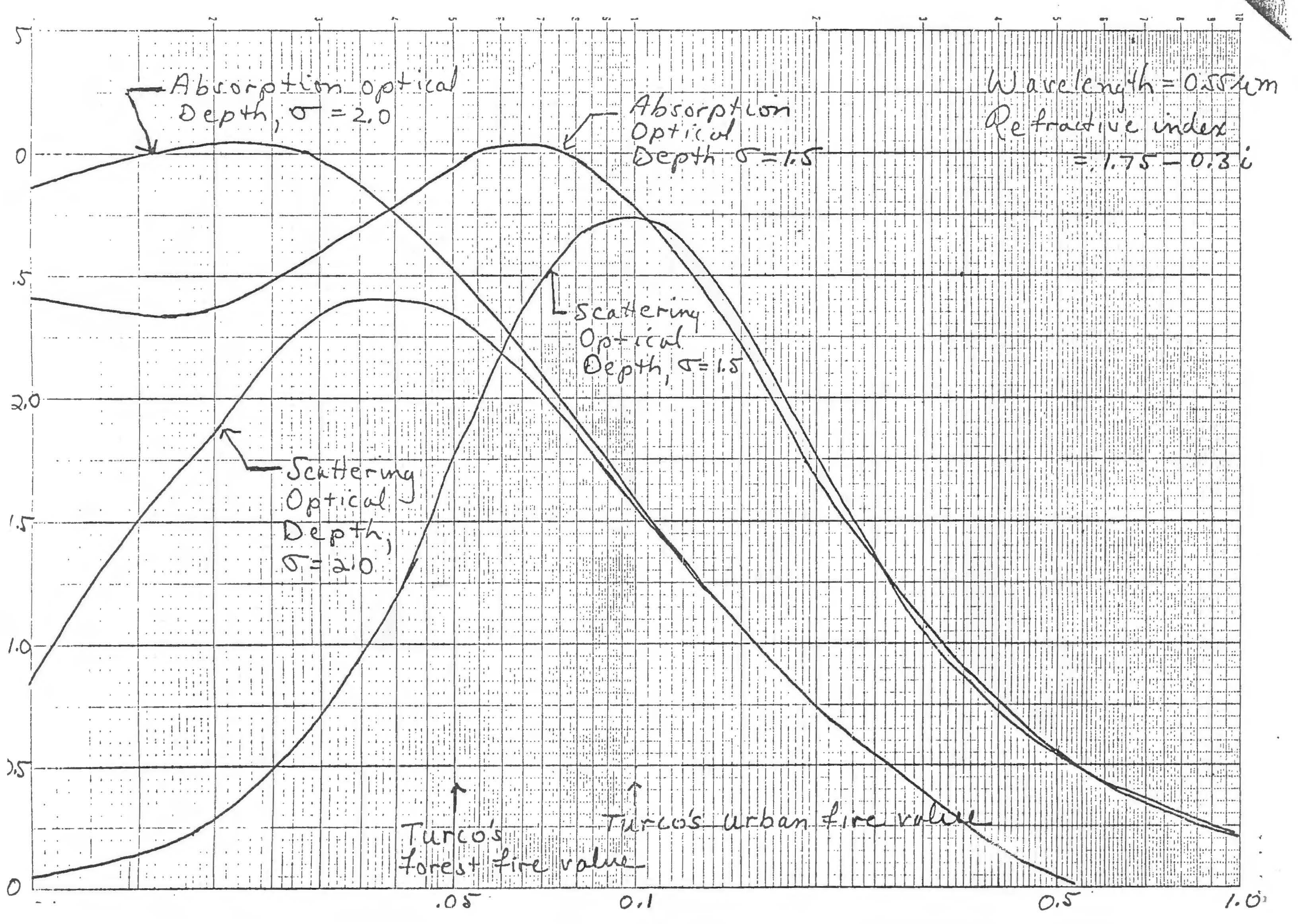
Page 00511-App. B-1

line 9: "repeatedly surprised more than once" somewhat overstates the case.

lines 11-13: I don't think destruction of the entire city was a surprise.

Page 00511-App. B-3

line 8: Why quote just Whitten; there are quite a few others.


Absorption optical Depth, σ = 2.0
Absorption Optical Depth σ = 1.5
Wavelength = 0.55 μm
Refractive index = 1.75 − 0.3 i
Scattering Optical Depth, σ = 1.5
Scattering Optical Depth, σ = 2.0
Turco's forest fire value
Turco's urban fire value
.05
0.1
0.5
1.0
2.0
1.5
1.0
0