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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatic consequences of regional nuclear conflicts" *by* A. Robock et al.

A. Robock et al.

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Reply to comments on "Climatic consequences of regional nuclear conflicts" by A. Robock et al.

Authors of this reply: **Alan Robock, Luke Oman, Georgiy L. Stenchikov, Owen B. Toon, Charles Bardeen, and Richard P. Turco**

Comments are repeated in italics.

Anonymous Referee #2
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Comments: The major point that distinguishes this work from previous published papers on the subject (and completely governs the result) is the concept that black carbon absorption will lift the aerosols into the upper stratosphere, where there will be a long residence time. To prove this, the authors use an 'off-the-shelf' soot model in GISS *model E, a soot model which is derived primarily from sources like diesel exhaust. The soot model has a mean effective radius of 0.1 microns. The critical question is: how do the results depend upon the assumption of black carbon size, since 100 Hiroshima-class nuclear weapons undoubtedly would throw up a very wide size range of particles. For the particles used here - spherical particles - the radiative properties do depend upon size. The size will also impact the radiative impact if it actually does get into the upper stratosphere. In addition, with such extreme forcing, there is likely to be an internal mixture of particles whose radiative characteristics could vary widely. In fact, the radiative properties of smoke from forest fires varies widely by itself. So from the theoretical standpoint, there are many uncertainties concerning whether this mechanism would really be operative following such a nuclear engagement. From the observational side, there is no evidence that I am aware of indicating that the solar absorption component of ash (as is thrown up in some volcanoes) really does induce convection. The companion article (by Toon et al.) doesn't mention any either - referring just to two 'private communications' concerning mesoscale models.*

We agree with the reviewer that additional work is needed on the dependence of the results on the assumptions we made about the aerosol properties. The size distribution may change over time due to coagulation, and the results will depend on the optical properties, which may change over time due to aerosol aging and chemical interactions. Reid (2005) clearly shows that smoke from biomass fires has very small particles, with a mean diameter of 0.1-0.2 μm. Furthermore, the optical properties of non-spherical carbon particles do not change significantly with particle size, as do those of spherical particles (Nelson, 1989). Volcanic ash, however, is not relevant, as this term refers to large particles put up by volcanic eruptions that fall out of the atmosphere very rapidly, and do not persist long enough to have climatic effects. Toon et al. (2006) in the companion paper address this issue in more detail.

Could it happen? Perhaps, but using a model's convection scheme is certainly not a very strong reed to lean on, especially when considering the extreme static stability *of the stratosphere and the very uncertain radiative forcing that is employed. This paper would have to be labeled very speculative, and for a subject this important, could only be published if the authors did one of two things: (1) Conduct radiative-convective model experiments varying the aerosol properties within range of what their imagination can come up with, and report how the results varied; or (2) Emphasize the uncertainties to a much greater degree than is currently done (where they are basically argued away). Approach (1) would be preferable, and would more clearly round out this study with the sort of careful assessment of uncertainty that it deserves.*

The request to conduct high-resolution model simulations is reasonable, but that work would be beyond the scope of this paper. However, our team has carried out two such studies, which were reported at the Fall 2006 American Geophysical Union meeting, one by Eric Jensen, and the other by Georgiy Stenchikov, Eric Fromm, and Alan Robock. In both cases, very high resolution simulations of smoke lofting support the results in our low-resolution global model simulations. Their results will be reported in other journal articles soon.

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The scenario in this paper seemed to me especially far-fetched-and I think this distracts from some interesting scientific issues that arose in the new calculations. So, as a former researcher on "nuclear winter," some comments:

1. Abstract: Assuming a regional nuclear war that involves some combination of one or more nations simultaneously exploding 100 Hiroshima size weapons as airbursts over cities just seems to me really far-fetched-even in this day and age of mentally unstable terrorists. And implying that this is somehow more plausible as it is a small fraction of the total global yield of all weapons seems to me a real stretch-such a war that did not involve the US, NATO, Russia, or China would basically involve a large fraction of the inventories of the smaller nuclear powers-for any of those parties, it would be a huge

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undertaking, if possible at all.

We make no claims about targeting strategies or war-fighting scenarios of any countries. We merely choose one possible scenario and evaluate its climatic response. This scenario is within range of Israel, Pakistan and India now. We have added further discussion of these issues to Toon et al. (2007), including references to how such a war could be started. However, a large range of scenarios should be studied, and we intend to do that in future work. In addition, these experiments should be repeated with different climate models (we have additionally done them with them with the WACCM model with similar results), to see how much difference this makes in the results.

2. Page 11818, lines 20-21: This set of references seems a bit strange-why not, after the Turco et al. reference, citing Pittock et al., and Harwell et al.-both 1985 and very major works.

Good idea. We do this in the revision. Actually they are Pittock et al. (1986) and Harwell and Hutchinson (1986).

3. Page 11818, line 24: It seems to me you really need a reference to justify saying that the nuclear winter (really "nuclear autumn" and "nuclear drought"-though you seem to want to avoid references to the NCAR, LLNL, and LANL simulations that did the 3-D calculations well) were an "important factor" in causing the end of the arms race. The Soviet Union went through an economic collapse-it was simply not a viable state (some even argue that it was the "Star Wars" challenge that busted the Soviet bank). Climate change was never the threat that scared off the US and USSR-in the scenarios considered, the direct and economic and cultural consequences were so large that the climate factor was not really the issue-the whole idea of that threat was that it would affect the other non-combatant countries.

It is not fair and not accurate to accuse us of wanting "to avoid references to the NCAR, LLNL, and LANL simulations that did the 3- D calculations well." In the first place, you cannot know our desires and motives. Second, the paper does include references to Covey et al. (1984), Ledley and Thompson (1986), and Schneider and Thompson (1988) from NCAR and Malone et al. (1986) from LANL. However as requested by this reviewer, we include Ghan et al. (1988) LLNL in the revision.

As to the role of nuclear winter in the end of the arms race, the speculation of the reviewer is no more valid than our suggestion. Robock (1989) did make a case for this role, including wording from the INF agreement. The Soviet Union did not end until 5 years after nuclear warhead numbers began to drop steeply, and the end of the Soviet Union did not alter the slope of the decline. Robock et al. (2007) have recently redone "nuclear winter" calculations using a modern model, and "nuclear fall" was apparently an artifact of the limited modeling capabilities in the 1980s. These references are added.

4. Page 11819, line 6: As I read the papers, to get to this 1-5 Tg, several worst case assumptions are made-this seems clearly a very worst kind of case (and this does not seem to be said very clearly). The worst case assumptions I am referring to are generally:
a. The targeting is all on the largest urban areas and all explosions are airbursts (so require some sort of reasonably sophisticated delivery system),
b. All of the explosions go off essentially simultaneously (or at least in the same window to get smoke aloft),
c. Fuel loads are quite high and essentially all fuel burns in the intense phase (seeming to neglect that in what are the most likely volatile regions, most of the buildings are not made up of wood or other burnable fuel),
d. The soot emission factor is quite high,
e. There is little scavenging of the smoke plume,
f. All smoke reaches essentially the upper troposphere, and
g. The smoke is injected over a relatively small area (though I understand a separate case was done for spreading in the latitude band)-and is it really the case that the model can simulate such a strong point injection (that was clearly a problem with the

early NCAR model with its use of spherical harmonics).

a.-f. are really related to the Toon et al. (2006) paper. To summarize:
a. Toon et al. did consider targeting urban areas, but this does not require a sophisticated delivery system. All one needs is a pressure switch, or a light aircraft.
b. We do assume that the weapons would be released in the same general time period. Essentially we assume the same sort of characteristics that the U.S and Russia envisioned in mutually assured destruction.
c. Fuel loads are not quite high. They are the same loads assumed in the 1980s by a variety of researchers including those supported by DOD. They are, in fact, much smaller than some researchers suggested for urban cores. Toon et al briefly discuss building construction, but it would be useful to directly survey third world cities for fuel loads. We did however, assign a fuel load per person, since no data are available for most of the countries investigated.
d. The smoke emission factor is not high. It is the same emission factor used by researchers in the 1980s including those supported by DOD.
e. We used the same precipitation removal efficiency as recommended by Turco et al. (1990). Subsequent data quoted in Toon et al (2006) support these values, though more data would be highly desirable.
f. We did make this assumption. Toon et al. reviewed the plume rise data, and the tops of the smoke plumes should certainly rise to these levels. However, the vertical distribution of the smoke is not well known, and new work on this issue would be valuable.
g. Yes, it was no problem for the model to handle this. It is a grid-point model, so no spectral issues apply.

5. Page 11820, lines 13-14: This new finding of stratospheric lofting is interesting. I don't recall what the results were from the various NCAR, LLNL, and LANL simulations-I would have thought there would have been some lofting. My guess is the main prob-

lem was that the run times of the models were pretty short back then-did anyone ever run for years?

In Table 1 of a subsequent paper (Robock et al., 2007), we compared previous nuclear winter climate model simulations. They only were able to run 20-40 days, with atmosphere-only models with tops of only 20-32 km. The new modeling capability today allows us to get these new results.

6. Page 11821, lines 4-15: I am a bit perplexed by the comparisons here. First, the comparison should be for the tropopause and not for the surface as they are connected-the CO2 influence on surface temperature could be as large as it is even if there were no change in surface flux. I am surprised that the large change in forcing did not actually cause a larger change in surface temperature-I guess what is happening is that heat is being pulled out of the oceans to limit the cooling (reinforcing the problem with the original TTAPS model simulation that had no surface heat capacity-so no ocean heat).

Radiative forcing from stratospheric aerosols is very similar at the surface and the tropopause (Stenchikov et al., 1998). The large forcing did cause a large climate response, as discussed in the rest of the paper. Criticism of the original TTAPS paper is not relevant here.

7. Page 11822, lines 17-20: I am quite surprised that the effect on the hydrologic cycle is not larger. What the Ghan et al. paper made clear (and this really should have been cited in discussing the change in precipitation) was that all it took was a limited amount of smoke in the upper troposphere to shut off convection and cause a "nuclear drought" where precipitation amounts dropped really dramatically. It appears that the lofting of the smoke into the upper stratosphere thus was more like turning down solar radiation (and so one kept a troposphere and stratosphere as at present) and all that happened was a rather small reduction in precipitation, and not a nuclear drought, which was the really problematic climate change in the "nuclear autumn" type of response of the *climate. So, perhaps this lofting, while lengthening the effect (which seems plausible) also reduced the intensity of the precipitation reduction.*

As noted above, we will include a reference Ghan et al., (1988) in the revision. This interpretation of the results is reasonable, and we will include it in the revision. There may indeed have been a short-term response as found by Ghan et al. (1988), but the location of the smoke clearly determines its effects on the hydrological cycle. Moreover the reviewer is referring to effects after a full scale war with smoke emissions that are more than 10 times larger than we consider. The climate forcings for a regional war are significant, but not as great as those after a full scale war. Robock et al. (2007) indeed found a reduction of more than 40% in global average precipitation for a full-scale nuclear winter simulation.

8. Page 11823, line 15: The papers seem inconsistent here. The Toon et al. paper basically does not worry about radioactivity for air blasts, and the Robock et al. scenario, to get enough fuel burning, is assuming air blasts, but then here it says "radioactivity" and is presumably getting a lot of the indicated casualties from this.

This line merely refers to the Toon et al., paper which includes radioactivity. For instance, if only airbursts were used and some were in regions with rainfall, there would likely be significant radioactivity deposited on the ground. We do not think is it necessary to change this sentence.

9. Page 11824, line 7: It would have been helpful here to go through the assumptions made here-they are, in my view, as indicated earlier, chosen in ways that make this all quite improbable, even given we are talking about an improbable type of event.

We do not have the ability to judge how improbable this scenario is. It is possible. We hope that once world leaders learn of the results here, it will indeed become very improbable. The probability of a nuclear conflict is not low, and, in fact, recent statements by leaders such as Henry Kissinger, George Shultz, Sidney Drell, and Mikhail Gorbachev all express concern about nuclear proliferation. The purpose of the paper

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is not to propose a probability distribution of possible nuclear war scenarios. Rather it is to evaluate the climatic response to one such possible scenario. In addition, as mentioned above, many other such scenarios need to be evaluated. We have evaluated, or at least listed, the uncertainties related to our climate model. Toon et al. (2006) evaluated those due to the scenario. There are several other works in progress with other models that address issues of spatial resolution, and model characteristics. We cannot quote from these at this time, but so far they support our conclusions. We encourage others to repeat our work, and to improve upon it.

10. Page 11827, line 6: It seems to me, that as I noted earlier, it was not the climate response that made in suicidal for a major power to initiate a nuclear war-the direct effects were more than enough to deter the major powers (the one additional point that really became clearer, I thought, were the likely consequences of total economic collapse. In earlier times, people were less dependent on the global (and even national) economic system, able to grow crops, get water from wells, rely on their own septic tanks, depend on a refrigerator instead of an icebox, etc.-but in the modern world, this is not the case-we are all dependent on each other and the overall societal system. Again, climate change would have then been on top of this, but people can only die once. The important aspect of the climate effect was to make clearer to non-combatant trouble-making nations that they too would suffer, from the economic and climatic effects.

For a full scale nuclear war the climate issues may be of second order (unless of course you are in the part of the world not directly attacked). For a regional conflict the issues we identify means that everyone in the world is likely to be affected, not just those in the restricted region that is directly attacked. This turns the tables from the days of nuclear winter. In that time some people took the position, as you do, that you had little concern beyond the people directly attacked, or directly affected by economic collapse. However, in the case of regional wars, most of the world will not be attacked, nor affected significantly by economic collapse. Instead the climate issues will be the major ones threatening them.

11. References: That Pittock et al. is not even referenced is quite an omission, as is ignoring Ghan et al. on the precipitation issue.

We will include a reference to Pittock et al. (1986). We thank the reviewer for reminded us of these papers of his.

12. On Figure 1, it would really be more appropriate to plot part A as an equal area plot-which would show more effectively how the lower latitudes clear pretty quickly as the smoke is pushed toward the poles.

There are different ways to do this. The reviewers suggestion is equally valid, but we prefer a lat-lon grid with equal spacing of the axes, a very common method of plotting global results.

13. On Figure 2: It is really misleading to say that the change for the 5 Tg case should be compared to 1.5 W/m2 for the two times CO2 case. Suggesting there is a factor of 10 difference is just wrong-one should be comparing changes at the tropopause. In addition, the CO2 forcing lasts indefinitely, while the smoke induced forcing clear. If you want to do a comparison at the surface for 2 times CO2, it should be to the forcing after the climate is changed, and at that point, the surface forcing change is like 16 W/m2, if I recall an early Hansen paper correctly (yes, this includes feedbacks, and one should do this for the smoke case as well, which would have less than normal water vapor feedback as this is a cooling.

The comparison we are making is to the instantaneous forcing at the time of the smoke injection. The forcing is clearly much larger than CO_2 forcing, and it produces a large negative climate response. We do not suggest that the global-average forcing is 10 times that from CO_2, nor that the effects would be 10 times that of a doubling of CO_2. The reviewer is correct about needing to include the timing and response of the forcings. But what the caption says is correct.

14. On Figure 4: So, the smoke strengthens the stratospheric inversion, but seems to *let the troposphere then operate. With smoke in the upper troposphere, this was not the case-the troposphere basically shut down.*

Yes, but it is not reasonable to expect smoke to linger in the upper troposphere for more than a few days. The current results produce a more reasonable simulation of this than those in the past. As noted in item 7 above, we will revise the paper to compare to the previous simulations of the hydrological cycle. However, once again the reviewer is comparing the regional war case with a full scale war in which more than an order of magnitude more smoke is present. The greater smoke impacts the climate more significantly.

15. On Figure 5: Again, this should be an equal area map. To test your Mercator bias, can you name a country about the same size as the Greenland ice sheet? Doing this gives a quite different perspective on whether the Greenland Ice Sheet is likely to be around for a long time or not.

We do not use a Mercator projection, which would make Greenland much larger than shown here. We use a lat-lon grid with equal spacing of the axes, a very common method of plotting global results. Much of the world population lives at midlatitudes. If we condense the midlatitudes on the graph it will be more difficult to read the changes. We are not interested here in conservation properties, which we agree are more easily understood on other axes.

Again, I felt that the scenario was so unrealistic as to damage the important findings in the study-that even a few nuclear explosions can cause tremendous death and destruction and, quite possibly, land contamination. On the other hand, my conclusion would be that it takes quite an impressive, and quite unlikely, war to have a significant climatic effect.

We thank the reviewer for this opinion. As mentioned before the scenario we propose is possible, and we have attempted to use the best choices currently possible given what is known. We invite others to repeat our work and add to it. As discussed above there are many knowledgeable policy people (Henry Kissinger, George Schultz, Michael Gorbachev,etc) who have recently expressed great concern about the potential for nuclear conflict in the changing world.

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