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

M. MacCracken

mmaccrac@comcast.net

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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 research 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 undertaking, if possible at all.

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.

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.

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,

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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).

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

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).

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

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climate. So, perhaps this lofting, while lengthening the effect (which seems plausible) also reduced the intensity of the precipitation reduction.

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.

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.

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.

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

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.

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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.

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.

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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11817, 2006.