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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Atmospheric effects and societal consequences of regional scale nuclear conflicts and acts of individual nuclear terrorism" *by* O. B. Toon et al.

Anonymous Referee #4

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General Comments: Assessing the impact of a 'generic' nuclear war - or of a random terrorist strike - is practically impossible, given the range of conditions under which it might arise (even more so factoring in the uncertain meteorological conditions). This paper attempts to do it anyway, and the result is numerous ad hoc assumptions which are unlikely to be realized in any particular situation. The numbers would have been much more plausible if the authors had considered a unique example in a unique location - they could still have presented the technique which, given that the results themselves are not really believable, is all that this paper really offers. The most 'unscientific' aspect is the failure to quantify the error bars on the calculations or myriad assumptions; I believe they would highlight the impossibility of such a generic calcu-

lation. In addition, there are various discussions throughout the paper that are more philosophical in nature concerning societies, politics and strategies which seem out of place for this journal. A foreign policy journal might be a better recipient of this aspect of the work, or perhaps even for the paper as a whole. The best part of the work concerns the dust emissions. Concentrating on that, and using output from the Robock et al. simulation discussed in the companion paper, would make this article more appropriate for this journal.

Specific Comments:

Abstract: lines 10-11: is the meaning here that high-yield weapons wouldn't be targeted at cities? Hard to believe they would have less effect than low-yield ones if they were. (Also see later comment.) Lines 4-5 from bottom: the climate effects listed - cooling of 1-2°C, lasting for about a decade, are really of negligible impact compared with the direct effects.

P11747, lines 12-13: in fact, Robock et al. primarily explored the consequences of 100 nuclear weapons, not one. Line 14: The results of this study show that the potential effects of~~Ł~~

P11748, lines 2-4: The tone of this sentence exaggerates the importance of the climate change. The Robock et al study itself says the effects are not as drastic as had previously been imagined, and cooling of 1-2°C in a decade while perhaps unprecedented in human history, would be nothing as drastic as the direct effects discussed in the early part of this paragraph. In addition, the cooling itself would perhaps act to offset global warming of a similar magnitude - radiative impacts of this order are being suggested as a geoengineering approach for just that reason (e.g., Crutzen's papers). Discussion of the climate change impact should be minimized to keep things in proportion.

P11750: Note that the US is now talking about developing a next-generation nuclear weapon; whether Congress will go along now is another matter. [This whole section should really be reviewed by an expert in nuclear weapon inventories, preferably some-

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body from the Defense Department.]

P11754, end of first paragraph: However, there have been advances in medical treatment that might prove effective for at least some types of injuries here.

P11755, end of second paragraph: how did the conclusions for Pakistan and India compare to those given here?

P11756, lines 1-3: what does relatively small mean? 10%? 50%? And how is this estimate to be derived? An attempt, at least, should be made to quantify the uncertainties. End of first paragraph: The words 'in each country' towards the end of this paragraph are ambiguous. The paper is being somewhat circumspect about defining what countries are being considered here. Are these the countries that were mentioned previously (e.g., Iran/Israel, India/Pakistan) or is it all developed countries? The Robock et al paper confined itself to tropical/subtropical countries - is that the case here as well? Clearly 'all' countries could not have been assessed - there are hundreds of them.

P11758, first paragraph: There is no a priori reason why in a full-scale nuclear war, high yield weapons would not be aimed at large population centers, in addition to military facilities. I don't find this paragraph at all convincing. Second paragraph: This is all speculative philosophy - perhaps this is not the proper journal for it.

P11759, first paragraph: Considering that this is all speculation, it could equally well be speculated that in lieu of building numerous small-yield weapons, countries are equally likely to try to build higher yield ones, to scare their opponents with shear magnitude. By the time a country has the capability to build 50 weapons, the speculation here is that it is unlikely it would have not passed the stage of aiming for higher yield ones.

P11760, second paragraph: why are the high yield weapons used against less populated facilities than the low yield ones? Is it that there are so many high yield ones in the countries that have them, that they can afford to hit these other areas as well? If

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so, then perhaps yield/kiloton is less, but that seems like a trivial point to emphasize - the total with the high yield, including both high and low density population centers, would still be much larger. I'm not sure that anybody cares how 'efficient' the death toll is! (The comparison with WWII is more rational.)

P11762, beginning of second full paragraph: a wind blowing to the east would not necessarily be an appropriate assumption for the subtropical countries discussed earlier. Where does equation 2 come from? Did it work for Chernobyl?

P11763, second paragraph: of course, all of this is truly unforeseeable, and hence quantification consists of building one assumption upon another; this negates any possibility of deriving uncertainty estimates. This is true for the third paragraph as well. Would any of this have worked for Nagasaki or Hiroshima?

P11764-11765: the Robock et al paper imagines that fires will drive convection putting black carbon into the upper troposphere; at least initially, were this to occur, one might imagine that the convection would be associated with at least some rainfall, and perhaps a large magnitude if the blasts were in the tropics/subtropics where moisture is plentiful. Rainout would seem a likely assumption to include.

P11766-11767: while much of this is undoubtedly true, one wonders what this is doing in a scientific paper; again the question of whether this is the proper journal for it arises. Perhaps a foreign policy/think tank journal would be more appropriate.

P11770-11771: numerous assumptions continue to pile up here without any attempt at error bars, although the range of results quoted on P11773 is a start in this direction.

P11776, second paragraph: note to be consistent with the discussion on pp. 11764-11765. R should be set to 1.0 (no rainout). In addition, with respect to the comment on lines 17-18: the assumption in the Robock et al paper (and later in this one) is that the convection will reach the upper troposphere - even in the Andreaa et al (2004) study that would likely result in significant precipitation. And since the fires would be

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immediate, longer-term rainout (P11777, line 6) is not the issue.

->P 11783: the smoke discussion is undoubtedly the best part of this paper. What is surprising is that it does not make more use of the modeling study done previously for black carbon (although that is referred to). P11784: the conclusion at the end of the second paragraph concerning ozone seems too strong. The paragraphs in this section indicate reasons for both stratospheric ozone increase and decreases; and given that the rate constants are not well known, any strong comment would seem out of place. (BTW, lifting air into the stratosphere from the troposphere, to the extent that it results in greater strat/trop exchange, would increase tropospheric ozone).

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11745, 2006.