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.**

O. B. Toon et al.

We appreciate the time that the reviewers spent considering our manuscript. Several of these issues will be treated in detail in our revised paper. Reviewer comments in brackets {} {: 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.} One needs to recognize that

no research has been conducted on the topic of regional nuclear wars. The world is continuing to proliferate nuclear weapons with no thought about the consequences of their use. We invite the world science community to investigate and expand upon our work to consider alternative scenarios. We did not, in fact, consider a generic nuclear war, but a unique scenario, an attack on urban centers, in each of a number of specific places. We considered attacking each of 50 specific urban sites in each of a number of countries. For each country we listed the fatalities for an attack on the most densely populated area, and the second most densely populated area. For a number of specific megacites we listed the fatalities for a number of weapons. In particular we showed the distribution of attack locations for the Tokyo megacity complex. We assumed a specific weapon yield likely to exist in the arsenals of new nuclear states, and used a population database for each target. We also summed these for each country. There is little or no dependence on meteorology for the fatalities, or the smoke. The radiation can depend on winds or rain as noted. Hence our example is quite concrete, we chose real places, we used real population data sets and specific types of weapons and specific ways to use them (airbursts or ground bursts). The results are not generic, however, we did compute them for many places.

{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 calculation.} Each section of the paper has a specific error discussion. We already have error bars on the numbers of weapons that presently exist in various countries, and on the fuel loadings. We have now noted in the revised text the dependence on yield (linear), and on numbers of weapons used (log of number). This can be used to estimate the uncertainty based on the scenario assumed. Unfortunately, there are some issues, such as rainout, for which the data do not exist to provide a good error bar. As we point out, in many cases the error is dominated by the scenario. This is simply an irreducible uncertainty. Some wars would be much worse than we assumed because they would involve more weapons with higher yield. We considered less than 0.1% of the yield on the planet, and less than 1% of the numbers of warheads. Some

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might be not as bad because fewer weapons were used. As mentioned we added to the paper the uncertainties due to number and yield. However, all this misses the point. We find that a regional war could be much worse than estimated by scaling from previous studies based on full scale nuclear wars. This result is surprising, and it means that people who think they know the effects of a regional war, without doing an analysis, have likely considerably underestimated the effects. The irreducible aspect of this problem is not unlike the situation with the greenhouse effect. Groups such as the IPCC make a range of guesses about future emissions scenarios. Given these guesses, they conduct climate models. However, future emissions could vary widely depending on unpredictable decisions that future generations may make. The IPCC predictions of climate change are interesting because they tell us the range of possible futures we could expect, not because they predict the precise future that will occur within some error bar. Likewise we hope the future will never bring a nuclear conflict. Hence no fatalities will occur. However, the surprising results we find are that the fatalities from a regional nuclear conflict could be much worse than one would think by scaling from previous work on full-scale conflicts, and that the entire world could be impacted by the smoke.

{In addition, there are various discussions throughout the paper that are more philo-sophical 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 appropri-ate for this journal.} We don't believe we have any political discussion, this is not a policy paper, and would not fit in policy journals. We do briefly discuss reasons that a conflict might occur, and lead to attacks on cities. Many reviewers, such as Mike Mac-Cracken question whether there would be a city attack. Hence we need to discuss this sort of issue, to explain why our scenario makes sense. We have now expanded this discussion and referred to policy authors who have examined the reasons that nuclear

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conflicts might occur.

{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.)} What we meant was that if you estimate fatalities, or smoke, by scaling from the results of studies of full scale nuclear conflicts you will conclude that smoke and fatalities are 100 time less than we find for our total yield. We have changed the wording in the final manuscript to be clearer. We have used these statements because many people think they can guess the effects of a regional war. For example before we did this study we did not expect significant smoke emissions because we knew that in a full war with thousands of megaton used perhaps 150 Tg of smoke would be created. For example as quoted in the paper Small estimated 37 Tg of smoke would be produced from using 4000 Mt in a full scale attack on the U.S. We predict 1.2Tg would be generated from the use of 0.75Mt. Our smoke production /Mt is 130 times greater than Small's, an unexpected result. Our new sentence reads; "We find that low yield weapons, which new nuclear powers are likely to construct, can produce 100 times as many fatalities and 100 times as much smoke from fires per kt yield as previously estimated in analyses for full scale nuclear wars using high-yield weapons."

{ 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.} Presumably the reviewer here is referring to our sentence in the abstract saying that the climate and ozone responses are significant. It is difficult for many people to understand what is implied by a global average temperature decline of 1-2C. Robock et al attempt to address this by pointing out the changes in growing season. Loosing a month from the growing season is indeed significant. Work reported by Mills and Toon on global ozone loss at the Fall AGU meeting, and soon to be submitted to a journal, shows an ozone loss near 30% on a global average, and much higher values at mid-latitudes of both hemispheres. These are indeed very large. An additional point is that the direct

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fatalities will be felt in the region of the conflict, while the climate and ozone changes will be felt worldwide. Hence to the bulk of the global population, it is climate and ozone changes that are of direct concern. There might also be other issues, such as loss of vital trade, but that depends significantly on the place that is attacked. We did not attempt to address these issues, even though for a full-scale war they would be of paramount concern. Hence we stand by our original assertion that significant climate and ozone losses could occur.

{ P11747, lines 12-13: in fact, Robock et al. primarily explored the consequences of 100 nuclear weapons, not one.} Our wording here was ambiguous. We changed it to read "Here we discuss the effects of the use of a single nuclear weapon by a state or terrorist. We then provide the first comprehensive quantitative study of the consequences of a nuclear conflict involving multiple weapons between the emerging smaller nuclear states. Robock et al. (2006) explore the climate changes that might occur due to the smoke emissions from such a conflict.

{Line 14: The results of this study show that the potential effects of_} We modified the sentence as suggested "The results of this study show that the potential effects of nuclear explosionsĚ.

{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.} We state that "Because of the smoke released in fires ignited by detonations, there is a possibility that 50 to 100 15-kt weapons used against city centers would produce global climate disturbances unprecedented in recorded human history (Robock

et al., 2006)." In fact Robock et all show that starting from today's elevated greenhouse climate a regional war emitting 5Tg of smoke to the upper atmosphere, could lead to the coldest decade in the past 1000 years (the extent of our detailed climate history knowledge). Hence we believe that our sentence is accurate. We removed the 50 number since Robock et al consider 100 weapons.

{ 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 somebody from the Defense Department.]} We have mainly reviewed the literature. The principal sources of data on weapons for larger states, such as the U.S. are the many papers written by Norris et al. in the Bulletin of the Atomic Scientist. For the states with smaller numbers of weapons, such as India, we used the Pu and HEU estimates from Albright et al, as updated at their web site. These estimates are clearly very difficult to make, and there is little way to verify them. We preferred these because Albright et al. are very clear about how they obtained them. The weapons estimates obtained are pretty close to those from Norris et al. We have added a quote to a paper by Lavoy and Smith (2003). Lavoy is at the Naval Postgraduate school. These are also similar to the numbers we obtain. Unfortunately it is not clear if all these numbers are fundamentally based on Albright's work. We added the following to the text: "In Table 1 we estimate that India has between 65 and 110 weapons, with 85 being most likely. We also estimate that Pakistan has 44-62 weapons with 52 being most likely. Our estimate for India is identical to that of Albright et al., (1997) for India in 2004, because we followed Albright et al.'s technique and used his numbers for Pu and HEU. Norris and Kristensen (2005d) estimate India had 40-50 assembled nuclear weapons in 2005. Norris and Kristensen (2006) state that "independent experts" estimate India has 60-105 nuclear warheads, of which 50-60 may be assembled. Norris and Kristensen (2006) state "experts" believe Pakistan has 55-90 weapons with 40-50 assembled. Lavoy and Smith (2003) extrapolated older Pu and HEU estimates from Albright et al (1997) using reactor production rates to determine that India in 2002 might have 40-120 devices, with a

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medium guess of 70, and Pakistan 35-95 with a medium guess of 60."

{ 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.} We added a reference to medical services small nuclear conflicts: "Modern medical treatment might prove effective for at least some of the types injuries that could not be treated during WWII. However, the availability of medical treatment will depend on the numbers of casualties, the damage to the medical infrastructure, and the ability of outside medial experts to enter the combatant countries (Leaning, 1986)."

{ P11755, end of second paragraph: how did the conclusions for Pakistan and India compare to those given here}? We added: "Several previous researchers have investigated casualties in individual cities; for example McKinzie et al. (2001) use an approach similar to ours for Pakistan and India. While a precise comparison is not feasible, they estimate 2.9 million fatalities and 7.7 million casualties if 10 cities in Pakistan and India were attacked with Hiroshima sized weapons. In our scenario we do not attack individual cities, but population concentrations. For our ten highest density regions in India and Pakistan we find 4.3 million fatalities and 8.9 million casualties."

{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. } We added the following analysis:"There are numerous uncertainties in computing fatalities and casualties. We believe that the uncertainties in our analysis, such as using the casualty probability curves from Hiroshima, and using the LandScan population database, are relatively small. For instance, in Table 4 the average ratio of fatalities in the air and ground bursts is 1.85 for a factor of 2 change in ?2. Since the area within a given damage contour is proportional to ?2, one would expect the ratio to be 2 if the population density were uniform over the area. The ratio of 1.85, slightly less than 2, indicates that the population is concentrated in the center of the area so that slightly less damage occurs as the zone of destruction expands. As noted in Table 3 two estimates of the Hiroshima fatalities give ?2 values whose ratio is 1.3. Hence, had we

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chosen the casualty curve of Oughterson and Warren (1956) instead of Ishikawa and Swain (1981) we would have calculated fatalities that were about 30% lower than in Table 4. In order to investigate the sensitivity of our results to the population data base we also investigated fatalities in India, Japan, China, U.S. and Pakistan using the population data base from Columbia University (CIESIN, 2004). This population database is on a coarser grid than LandScan, with a typical area per grid cell near 20 km2 at the latitude of Pakistan. Hence, the population density does not vary over the typical scale of the nuclear damage area. The data we used were for 1995 from CIESIN, while LANDSCAN updates data to remain as current as possible. In this case we found that with the CIESIN database fatalities were 20% greater for the U.S. but only 23% as large for China than with LANDSCAN. As discussed in the next section we also investigated the fatalities from attacks on the 50 most populated regions for several countries. In this case the two databases yield results within 10% for India and Japan. For the U.S. the CIESIN database yields 30% higher fatalities for the U.S., but only 40% as high fatalities for China and about 50% for Pakistan. We believe that the CIESIN database is not on a fine enough scale to be reliable for these calculations. As we discussed above a difference of about 10% is likely due to not being able to integrate the fatality curves over the population distribution. We expect, however, that the larger differences for China and Pakistan reflect the large increase in urban population that has occurred between the 1995 CIESIN database and the 2003 LandScan database. To summarize, the differences between LandScan and CIESEN are not random, but rather due to lower spatial resolution and older data in the CIESIN database, which is why we chose to use the LANDSCAN data. Likewise we chose the Ishikawa and Swain (1981) fatality curves because they were done at a later time when more data were available. The greatest uncertainty in our fatality estimates, as noted in the introduction, is likely to lie in the targeting of the weapons, and in the types of weapons used. Table 4 shows that ground bursts are likely to produce about 75% of the fatalities as air bursts, but, as discussed later, also leave behind lingering radiation. We previously discussed the 33% fewer fatalities that occur if one targets the 10 largest cities in India and Pakistan

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rather than the ten most densely populated regions. Based on our discussions of the linear relation of damage area to ?2, and to the fact that the damage area varies linearly with yield, it follows that the fatalities will be roughly proportional to yield. The yield could vary by a factor of 10 or more from our assumed 15 kt, depending on the sophistication of the weapons maker. There are numerous other uncertainties related to the use of the weaponsĚ"

{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.} We added: "In this section of the paper we chose examples based on attacks on the most densely populated areas in numerous countries listed in Table 4."

{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.} Of course high yield weapons would be directed at cities. This paragraph has been moved to later in the text where it hopefully will not be so confusing.

Second paragraph: This is all speculative philosophy - perhaps this is not the proper journal for it.} We replaced this paragraph with a reference to military analysts. We also point out that military targets would likely be attacked, but we are ignoring those casualties. "In our computations we have assumed that the densest population centers in each country are targeted. There are many circumstances that could trigger a regional-scale nuclear conflict, and many scenarios for the conduct of the ensuing war. For instance, analysts (Lavoy and Smith, 2003) suggest Pakistan and India could get into a nuclear war because a conventional conflict threatened to overwhelm the strategic conventional forces or command and control structure of either side. Alternatively

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they might launch a nuclear war to preempt a nuclear attack, real or imagined, by their adversary. Iran and Israel, and numerous other countries, might exchange nuclear weapons for similar reasons in the future. In our analysis we assumed that the densest population centers in each country-usually in megacities-are attacked. Such an urban attack might be conducted to inflict maximum damage. It is likely that military targets would also be attacked. We have not attempted to locate specific military targets, and ignored casualties related to such targets, but note that many military targets are in cities. Hence the "small" wars assumed here are similar in principle, if not in scale, to the strategies for all out nuclear warfare and war fighting embraced by the superpowers in the mid-20th century in the context of "mutually assured destruction".

{ 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.} You are correct. For example India seems intent on building an arsenal that is comparable with those of China, Britain and France, as we state. However, it is difficult to build higher yield weapons, because the designs are more complex. We don't want to speculate about the goals and capabilities of countries, so we have not added anything here.

{ 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?} That is basically the point. We have modified the paragraph to read: "The fatalities in Table 6 for a 50-weapon attack on the United States are comparable to those previously estimated for a limited or counterforce attack involving 3000 weapons and 1300 Mt (OTA, 1979; Daugherty et al., 1986). Scaled against total weapon yield, the fatalities per kiloton are 100 times greater in the small weapon scenario, even when full

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scale urban targeting has been considered in past scenarios (Harwell, 1984). The high fatality rate of low yield weapons is not due to any non-linear phenomena. While the use of thousands of high-yield weapons would certainly lead to more casualties than might occur in a small attack or exchange with low-yield weapons, the number of casualties is not reduced in proportion to the total yield because of inefficient use of the huge arsenals of high yield weapons. For example, even today Russia and the U.S. maintain much larger arsenals than are needed to strike all significant military targets as well as every moderate to large city in the world. Many weapons are aimed at the same target, or aimed at missile silos or submarines in unpopulated regions. For high-yield weapons in the Mt range, much of the area inside the destruction zone would be sparsely populated- even in large cities- as the population density decreases rapidly toward the perimeter. Therefore, based on the present results, relatively small numbers of low yield weapons targeted at densely populated urban centers may lead to similar casualties as in a full-scale counterforce war."

{ I 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.) } The reason we emphasize this point is that many people think they can guess the results of a regional war. If you start with one city and work your way up you can guess correctly, if you know what happens in one city correctly. However, if you take previous estimates for full scale wars, and try to scale them down, you will be off by a factor of 100. For instance we never expected a significant amount of smoke to be generated by 100 weapons, based on our previous work where tens of thousands were used.

{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.} We changed the sentence to read: "We take the wind to blow directly toward the east, as prevailing westerlies although in reality the winds could blow in any direction, and in

some countries we consider prevail toward the west."

{Where does equation 2 come from? Did it work for Chernobyl?} Eq. 2 comes from fitting the data of Glasstone and Dolan in Fig. 7a. which is based on nuclear weapons test. Chernobyl was quite different from a typical test because it was not a nuclear explosion. We added two sentences. "Given the complexity of the fallout problem, and sensitivity to parameters such as wind speed, rainfall and dust particle size that cannot be determined in advance, we utilize the simplified fallout model documented by Glasstone and Dolan (1977) to compute areas subject to given exposure levels downwind of a surface contact burst. This model was based upon experience from nuclear weapons tests." And "Figure 7a shows the potential maximum 48-hour whole-body dose as a function of downwind distance from a 15-kt burst obtain from the Glasstone and Dolan (1977) model."

{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?} Nagasaki and Hiroshima were airbursts, so there was little radioactivity deposited on the ground. There were large numbers of nuclear weapons tests, so there is a pretty good understanding of the fallout distribution. One can't predict the details in advance, but the general patterns of radiation deposition are pretty certain. It is less clear how people respond to the radiation, and what their exposures will be.

{ 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.} Following the Hiroshima explosion there was "Black Rain", which contained some radioactivity. However, the main factor generating rainfall was the fire storm. As discussed later the fires generate

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more than 1000 times the energy of the nuclear explosion, so they can induce pyroconvection. It takes several hours for the firestorm to develop, and by that time the initial radioactive cloud will have blown away from the urban area. Therefore, for airbursts pre-existing rainfall matters. We added "As discussed later the mass fires likely to occur after a nuclear explosion are capable of generating pyroconvection and possible associated rainfall. However, it takes several hours after the explosion for these clouds to develop, by which time the nuclear debris cloud will have blown away. Hence preexisting natural rainfall in the area of the explosion is the source of concern for short-term radioactivity from an airburst."

{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. } We removed the paragraph.

{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.} We believe that each term in this section is discussed and error ranges estimated, usually be referring to the studies of Small and Turco which themselves were based on many studies.

{P11776, second paragraph: note to be consistent with the discussion on pp. 11764-11765. R should be set to 1.0 (no rainout).} We addressed this issue just above. The pyro-convection is relevant to the removal of smoke since it is initiated by the fires. However it is not relevant to radioactive debris that is in the fireball, and likely blown away from the immediate locality by the time the fires storms start.

{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 immediate, longer-term rainout (P11777, line 6) is

not the issue.} I think we have confused the reviewer by having too many thoughts in one paragraph. We divided this into two to better distinguish pryoconvection and normal wet removal. "An issue that was widely discussed in previous work is the extent of smoke rainout in fire-driven convective columns (Pittock et al., 1989; Turco et al., 1990). Here, we adopt a baseline value for the rainout parameter, R (the fraction of the smoke emission not removed), of 0.8, following Turco et al. (1990). This relatively high value for R implies inefficient removal of smoke in the pyrocumulus systems driven by an urban fire. Observations of such convection associated with forest fires are consistent with smoke particle over-seeding of capping cumulus clouds, which severely inhibits induced precipitation. As a consequence, essentially no smoke removal is observed in pyrocumulus plumes that stabilize below about 5 km (Andreae et al., 2004). According to Andreae et al. (2001) in natural fires the ratio of injected smoke aerosol larger than 0.1 μm to enhanced carbon monoxide concentrations is in the range 5-20 cm3/ppb near the fires. Jost et al. (2004) found ratios ˜7 in smoke plumes deep within the stratosphere over Florida that had originated a few days earlier in Canadian fires, implying that the smoke particles had not been significantly depleted during injection into the stratosphere (or subsequent transport over thousands of kilometers in the stratosphere). Such evidence is consistent with the choice of R = 0.8 for smoke removal in pyroconvection. Smoke might also be removed by rainfall after the smoke has been dispersed and no longer is concentrated enough to inhibit precipitation. Evidence suggests that this wet removal is very efficient. For example, observations at altitudes near 10 km of smoke plumes processed by deep tropical convective systems remote from originating fires have yielded smoke to CO ratios of about 1 cm3/ppb (Andreae et al., 2001), suggesting extensive rainout of the aged accumulation mode smoke aerosol. This efficient removal in the troposphere is avoided in the studies of Robock et al. (2006) because solar heating quickly drives the smoke into the stratosphere where rainfall does not occur."

{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

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for black carbon (although that is referred to).} I am not sure which study the reviewer means. I added specific comments that will directly refer the reader back to previous sections: "Using Eq. 5 we calculated the elemental carbon emitted in each target area in a number of different countries. In each case for Eq. 5 we used LandScan for P, assumed A was 13km2, assumed Mf was 1.1x10ˆ7 g fuel/person, that R was 0.8, that C and Q were each unity, and that the sum of FS is 0.016 g soot/g fuel."

{ 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).} Actually we only listed things that would decrease ozone, we don't see any that would increase it. We have found additional research showing that soot is not destroyed by ozone except at a very small rate. We will stand by our statement that substantial ozone loss will occur. We have other work in progress on this topic as well. We change the paragraph to read: "Soot, like other stratospheric particles, may also catalyze chemical reactions involving key species such as HCl, leading to accelerated ozone loss. At one time it was thought that carbonaceous aerosol may be consumed by reactions with ozone (Stephens et al., 1989) and other oxidants, reducing the lifetime of soot at stratospheric altitudes. However recent data shows that the reaction probability for such loss of soot is about 10ˆ-11, so it is not an important process on times scales of several years (Kamm et al., 2004). A full simulation of stratospheric chemistry, along with additional laboratory studies, would be needed to evaluate the importance of these processes. It should be noted that rate constants for a number of potentially important reactions are lacking. Nevertheless all the known reactions suggest that the stratospheric ozone would be lost, not gained, so that substantial stratospheric ozone depletion is a likely outcome of the scenarios studied here."

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