Atmos. Chem. Phys. Discuss., 13, C3296–C3299, 2013
www.atmos-chem-phys-discuss.net/13/C3296/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climate and chemistry effects of a regional scale nuclear conflict" *by* A. Stenke et al.

Anonymous Referee #1

Received and published: 8 June 2013

This paper presents an important validation of previous "nuclear winter" studies by Robock et al. (2007) and Mills et al. (2008), using a third, entirely different climate model, and a broader range of black carbon emissions resulting from a regional nuclear conflict between India and Pakistan. The authors present a thorough examination of the calculated responses of the climate system, including stratospheric heating, surface cooling, sea ice expansion, declines and other changes in precipitation, severe global ozone depletion, summertime increases in dangerous UV radiation, and wintertime attenuation of sunlight, adding up to dire challenges to the global human population and ecosystems. I recommend this article be published in ACP with a few suggested minor revisions.

Toon et al. (2007) calculated that 80% of the soot emitted would survive initial rainout

to be lifted to the upper troposphere (150–300 mb), not the stratosphere (as stated on p. 12093, line 8). Therefore, Mills et al. (2008) tested inputs of 1 and 5 Tg of soot in the upper troposphere (150–300 mb), and their model calculated a further loss of 20% as the plume of hydrophilic black carbon rose to the stratosphere. The model of Robock et al. (2007), by contrast, assumed that black carbon is initially hydrophobic, with an aging lifetime of several days to become hydrophilic, and hence did not calculate significant rainout as the plume rose to the stratosphere. Lines 11-13 on p. 12093 should be reworded to clarify that the further 20% loss was a calculation, rather than an assumption in Mills et al.

On page 12096, line 11, you state that you use optical properties for soot particles with diameters of either 100nm or 200nm based on OPAC (Hess et al., 1998). The Hess et al. paper and linked OPAC data set give optical properties only for 100nm-diameter particles (50nm radius, rmodV in their Table 1c). Did you really use optical properties for 200nm-diameter particles as well? If so, how did you calculate them? On line 29, you explain the setup for Exp5_200, where 200nm soot particles were used instead of 100nm. You should clarify that this is diameter, and explain whether the 200nm size was used for optical properties, for microphysics (sedimentation), or both.

In both panels of figure 1, you show the total mass for Mills et al. peaking at about 7Tg. This can't be right, since they only injected 5Tg. Compare to their figure 5a. Please correct this.

On page 12100, lines 18-19, you state "the return to normal temperatures is slightly slower in Exp5_100 (not shown)" than in Exp5_200 in Figure 3. This is confusing. Exp5_100 is shown in Figure3, while Exp5_200 is not shown. Which is slower? Perhaps you should rephrase this as "slightly faster in Exp5_200 (not shown)."

On page 12103, lines 5-14, the issue of natural variability in the model is raised. You should caveat your results in light of the fact that you did not do an ensemble of runs for each case, as Robock et al. (2007a) did. This is important in your comparison to

ACPD
13, C3296–C3299, 2013

Robock et al. in Table 3, and in the subsequent paragraph, where you discuss regional temperature changes. An ensemble of runs would inspire greater confidence in your conclusions about regional changes.

On page 12104, line1, the issue of variability in sea ice in the control simulation is raised, again pointing to the limitations of just running one run per case. This should be mentioned. Given the variability in regional ice coverage, the maps in Figures 8 and 9 add little to the ice coverage changes presented in figure 7. Given that you have 16 figures, consider removing figures 8 and 9.

In figure 14, it is difficult to distinguish the 4 shades of blue in the color scale. These cover the most severe levels of ozone depletion, and hence are very interesting. Please improve your color scale.

On page 12107, lines 15-17, your sentence is too long. Split it into two sentences.

On page 12108, line 10, you mention "the latitude band around 20°". Did you mean 20°S-20°N? Or something else? Please clarify. On line 17, commas around "at first sight contradictory" are not necessary and break the flow of the sentence. The sentence ends with "these latitudes." Which latitudes? Please clarify.

On page 12111, lines 1-3, you mention that soot does not provide surfaces for the ozone-depleting heterogenous reactions that happen on sulfates. This is true, but you should make clear that soot is still a much more effective ozone depleter, due to its heating effect. On line 21, the "University of Colorado" affiliation for Mills should be "NCAR".

Typographical issues

Page 12097, line 26: "scenarion" should be "scenario"

Page 12101, line 8: "strato-" should be "stratospheric"

Page 12102, line 4: "1.4°N and 60°N" should be "1.4°N to 60°N"

ACPD

13, C3296–C3299, 2013

Page 12103, line 6: "except" should be "with the exception of"

Figure 10 caption: "percipitation" should be "precipitation"

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 12089, 2013.