

First of all we would like to thank the editor and both referees for their positive judgment and valuable comments on the manuscript. We revised our manuscript according to the suggestions. The individual points are addressed in the point-to-point reply below.

Reply to review 1:

R1: 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.

AC: Thanks a lot for the clarification. We changed our text accordingly.

R1: 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.

AC: We used the wavelength-dependent refractive index (real and imaginary part) provided by OPAC and calculated the required optical properties (long- and shortwave extinction, long- and shortwave single scattering albedo, long- and shortwave asymmetry factor) for the 100nm and 200nm-diameter particles using Mie theory. In Exp5_200 the 200nm size was used for both, radiation and sedimentation. We changed our text to clarify these points.

R1: 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.

AC: We apologize for this mistake. We did not consider the orography for the calculation of the total soot burden and, therefore, overestimated the total soot mass from Mills et al. Fig. 1 is now correct.

R1: 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)."

AC: We agree with the referee that our text was confusing. We rephrased the sentence as suggested.

R1: 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 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.

AC: We agree that our comparison to the Robock et al is limited due to the fact that we don't have ensemble simulations. To clarify this point we added the following sentences: "However, the comparison of regional changes is limited and has to be handled with care, since for SOCOL only one realization of each scenario run is available while Robock et al. (2007) performed an ensemble of

several model runs for each case. For a profound analysis of systematic differences between SOCOL and the model of Robock et al. (2007) an ensemble of SOCOL runs would be necessary.

R1: 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.

AC: *To further highlight this problem we included to following sentence: "This is a further example pointing to the limitations of having only one realization of each scenario." Figures 8 and 9 have been removed.*

R1: 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.

AC: *We changed and hopefully improved our color scale.*

R1: On page 12107, lines 15-17, your sentence is too long. Split it into two sentences.

AC: *As suggested we split the sentence into two parts.*

R1: 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.

AC: *We revised our manuscript according to the referee's suggestions, i.e. "the latitude band around 20°" is now "the latitude band between 20°N and 20°S"; commas on line 17 are removed; "these latitudes" is now "at latitudes above 60°"*

R1: 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".

AC: *To clarify this point we included the following sentence: "Nevertheless, soot aerosol is much more efficient in depleting ozone due to its stronger heating effect." We corrected the affiliation for M. Mills, thanks a lot for this hint.*

R1: Typographical issues

AC: *All typos corrected*

Reply to review 2:

R2: I have reviewed the paper by Stenke et al. "Climate and chemistry effects of a regional scale nuclear conflict". Generally this is a very well written paper that describes an independent study of the potential climate and ozone perturbations that might result from a nuclear conflict using a modest number of low yield weapons. The calculations are explained well, and the conclusions drawn are carefully made and defended. There are a few places where additional information might be helpful to others interested in this problem. For example, the authors might include a graph of the latitude and time dependent zonally averaged soot optical depth so that others could compare numbers on this important quantity. For the ozone loss calculations it would be useful to describe the optical calculations. For example, was multiple scattering by the soot particles included directly in the photorate calculations?

AC: *As suggested by the referee we added a plot showing zonally averaged soot optical depth for our 5 Tg case. This might be indeed helpful for other modeling groups interested in this topic.*

Furthermore, we added some information about the photolysis scheme in the SOCOL model to Section 3.1: “Precalculated photolysis rates as functions of the ozone and oxygen amount, including effects of the solar irradiance variability, are used in a look-up-table approach and interpolated to the actual ozone profile at every chemical time step (Rozanov et al., 1999). The effects of clouds on photolysis rates are parameterized according to Chang et al. (1987), the effects of aerosol particles, however, are not considered.”

R2: On minor comments On page 12093, line 5-12. The 6.6 Tg of lofted soot in Toon et al. (2007) included the 20% removal due to initial rain out. Therefore they thought 6.6 Tg is the amount lofted to the upper troposphere. Mills et al. did not “assume” anything about the amount of soot rained out in the first 10 days. Rather their model computed a rainout rate based on the abundance of rain. Robock et al. (2007a) assumed soot would be hydrophobic for part of the first day, while Mills et al did not assume the soot was ever hydrophobic. This is likely one reason that Mills et al have slightly less soot in the stratosphere, than does Robock et al after the first few days.

AC: *Thanks a lot for the clarification. The second referee raised the same comment. We extended our text to clarify the differences in the initial soot removal in black rain assumed in the different model studies.*

R2: On page 12097 line 26. Did you mean “scenario” instead of “scenarion”?

AC: *corrected*

R2: Fig. 1 caption. “dashed” is misspelled.

AC: *corrected*

R2: On page 12098 Line 12 “initial” is misspelled.

AC: *corrected*

R2: On page 12100 line6 “lead” should be “led”, or better “caused”. Line 16 insert “particles” after soot.

AC: *“lead to” -> “caused”; “particles” included*

R2: On pg 12104 line 26 “warmer” than what, “less” than what? Line 6 to end of section. Here you present some data/other models on sea ice responses to volcanic clouds. I was not clear what you concluded from this, or how it applied to your simulations. Can you add some sentences or a paragraph where you interpret what the observations suggest about your calculations?

AC: *“...warmer and showed less sea ice than the other years between 1810 and 1825.”*

We extended this section by a more quantitative comparison:

“The reduced solar insolation after the volcanic eruptions of up to -30 Wm^{-2} (Miller et al., 2012, their Fig. 3) caused an increase in Arctic sea ice volume of about 20%, resulting in a self-sustaining sea-ice/ocean feedback in the North Atlantic and persistent cold summers. For comparison, the decrease in solar insolation in Exp12 peaks at -23 Wm^{-2} , associated with an increase in sea ice coverage of 30 to 40% in northern hemispheric winter.”

Furthermore, we added the following concluding sentences:

“The mentioned modeling studies and observational reports of sea ice responses to volcanic clouds indicate that the persistence of the sea ice response as simulated with the mixed-layer ocean model is

indeed reasonable. However, the comparison with the results of Miller et al. (2012) suggests that the simulated sea ice increase might be overestimated."

R2: Fig. 8/ Fig. 9 are not very informative. All the figures look about the same, and the color bar only shows one color. Perhaps it would be more useful to plot the change in sea ice cover.

AC: *Since the second reviewer raised a similar comment, we decided to remove Fig. 8 and 9.*

R2: Pg 12105-Fig. 11 is rather confusing. From Fig2 we see that the soot burden changes almost linearly in time, so I think this figure is about the response time of the system to the perturbation. However, it could appear to the reader that the response is not linear in soot loading, since for example 2 and 6 Tg yield the same temperature change. I would change the horizontal axis to time instead of soot to be clearer. At least you need to explain the graph to the reader, and explain what the odd dependency on soot loading means.

AC: *Global mean surface temperature and precipitation changes as a function of time are already shown in Fig. 4 and Fig. 10, which also provide information about the response time of the (model) climate system to the perturbation. The idea of Fig. 11 was to identify potential saturation effects with increasing soot loadings (p12105, l 14-17).*

R2: Pg 12106 line 1. The sentence is confusing. Replace the word "it" with words that say what "it" is. Line 4. Did you include the soot in the photorate calculations (Mills et al did not)

AC: *The sentence is now "This change in summer monsoon is interpreted..."*

No, the soot was not included in the calculations of the photolysis rates. We added a short note to the text.

R2: Pg 12110 line 7 "temperature" is misspelled.

AC: *corrected*

Reply to editor comment:

We revised our manuscript according to the editor's suggestions. In particular, we removed the non-scientific adjectives from the abstract and the results section. As suggested we added numbers wherever appropriate.

Furthermore, we carefully revised our conclusions. It wasn't our intention to confuse results from our model simulations with results from previous studies. We fully agree that our model simulations do not provide any information about agricultural effects, even though they seem to be likely. To clarify this issue we revised the conclusions: The paragraph p12119, l3-7 has been removed. The sentence "The climatic consequences..." is now "As discussed by previous studies the climatic consequences...", and we added a citation. Nevertheless, we think that the topic of our study is politically relevant and needs some comments on the consequences of the climatic changes resulting from such a nuclear conflict. Therefore, we decided to keep the final paragraph, but renamed the whole section "Discussion and conclusions".