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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Regional and global climate response to anthropogenic SO_2 emissions from China in three climate models" *by* M. Kasoar et al.

Anonymous Referee #1

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Review of "Regional and global climate response to anthropogenic SO2 emissions from China in three climate models" by M. Kasoar et al.

The authors present coupled atmosphere-ocean simulations with three models to study how a removal of sulfure dioxide emission from China would impact local and global climate. Given the localized nature of aerosols and expected future reduction in sulfate aerosol, this sort of study is certainly interesting, also because the use of three models has the potential to distinguish robust and non-robust responses. As such, the study is in general suited for publication in ACP. Yet, I feel the analysis needs to go further to increase the paper's value, and a discussion about whether studies like these are indeed useful or not seems warranted at the end of the paper. What I mean by this

should become clear in my comments below.

1. The advantage of a model intercomparison study is that it allows for a clean juxtaposition of models. Yet, fundamental model diagnostics differ between the models, and I find that this very much complicates the comparison and limits the ability to draw firm conclusions beyond the statement that the models differ. I find the lack of clear-sky shortwave fluxes for CESM most striking - clearly this is a standard diagnostic, and I know that CESM has this diagnostic implemented. So why is it not available for the runs provided here? Having the clear-sky shortwave diagnostic would greatly aid the discussion of cloud effects in Sect. 4.2. Similarly, why is AOD diagnosed differently across the models, which seems to inhibit firm conclusions about AOD differences and aerosol radiative efficiencies. And finally, why is there no measure of internal variability available for CESM? I understand that this has to do with the lack of ensemble control runs (available for HadGem) or one long control run (as for GISS), but why have such runs not been performed. Aren't the authors in charge of the simulations presented here? I think the paper could be much stronger if the above limitations were addressed and the model setup and experiments were designed such as to eliminate them.

2. As a result of the above I am wondering what I am supposed to take away from the current paper, apart from the statement that there is large model uncertainty. The authors attempt to trace the uncertainty to different sources, including aerosol chemistry (Sect. 4.1), cloud-radiative effects and aerosol-cloud interactions (Sect. 4.2), aerosol-radiative interactions (Sect. 4.3) and climate sensitivity (Sect. 4.4). None of these seems to be the sole smoking gun, though. While I appreciate that there maybe is no single factor that explains most of the uncertainty, what kind of experiments would be needed to better understand the individual contributions of the above four factors? I think a discussion of this question is needed in the conclusion section.

3. Pattern of global temperature response: I am wondering to what extent the temperature patterns between the three models in Fig. 2 are more similar than acknowledged by the authors. What I mean is that GISS, while having no global-mean response,

seems to show cooling in northern hemisphere regions in which CESM and Hadgem show relatively less warming (e.g., over the North Atlantic and Iran). Maybe the temperature patterns between the models look similar when the global-mean temperature change is removed? That would be interesting and point to robustness in the remote dynamical response.

4. Reflecting on point 1, why is AOD diagnosed differently across the models? What is the motivation for this, and how to differences in the AOD diagnostics affect the results?

5. At the end of section 4.1.1, I think a statement similar to the one on page 21, lines 23-25 would be helpful to wrap up this fairly complicated subsection, which simply seems to say that comparison to observations of current AOD doesn't help to constrain the model response.

6. Sect 4.2, lines 19, "what we would expect from a simple amplification of the radiative response due to indirect effects": Clear-sky shortwave changes will always be larger than all-sky shortwave changes because clouds mask some of the aerosol. So how can a comparison between clear-sky and all-sky changes inform about aerosol-cloud interactions (i.e., indirect effects)?

7. Sect. 4.4: The idea to use global climate sensitivities derived for a uniform forcing to explain the local response to a highly localized forcings seems flawed to me to begin with, and indeed the authors find that global climate sensitivity does not help to understand the model differences. I suggest to condense this section into one or two sentences in the conclusion section.

8. Instead, I would like to encourage the authors to expand their analysis of the changes in shortwave fluxes. The diagnostic approximate shortwave model of Donohoe and Battisti, J. Climate 2011 (Atmospheric and Surface Contributions to Planetary Albedo) would be a very valuable tool to understand the contribution of atmospheric and surface reflectivity to the changes in surface flux. One can further use the model for clear-sky and all-sky fluxes separately in order to distinguish aerosol effects (from

the clear-sky use of the model) from cloud effects (when all-sky fluxes are used). I believe such an analysis has the potential to give much more insight and to grealy improve the paper.

Minor comments:

1. Information about the shortwave radiative transfer schemes is missing in the model descriptions.

2. page 8, line 1: the East China box should be drawn in one of the figures for easier reference.

3. caption figure 1: focuses –> focus

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