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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 #3

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The authors present a three-model comparison of the consequences of removing anthropogenic SO2 emissions from China. They compare the radiative and climate responses, and use ground and satellite observations to try to evaluate model performance. The study is an interesting and relevant addition to the literature on the diversity of climate model responses to comparable perturbations. While some aspects of the analysis, diagnostics and presentation could have been clearer, the main results of the study are still clear: For an aerosol perturbation that is weak but realistic (i.e. not scaled, as is done in most multi-model intercomparisons), the diversity in model response is very large indeed. The paper should be published in ACP after some additions and clarifications have been made.

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Discussion paper

Major comments:

- While the perturbation applied is well specified, the model output and diagnostics retrieved seems to vary a lot. I realize it's hard to do anything about this once the simulations are done, but for later studies I would encourage the authors to use a wider output protocol. E.g. clear-sky vs all-sky fluxes should be possible to diagnose for all these climate models, and for sulphate perturbations their difference can be very instructive due to differences in treatment of the indirect effect.

- Page 8, line 23++: For a sudy such as this one, a good diagnostic of TOA RF is very useful. It can be extracted from relatively short and inexpensive fSST runs, as was done here for HadGEM3. I would encourage the authors to add this also for the two other models, and to take the results into their intercomparison discussions.

- Page 11, line 30-31: The GISS-E2 model has had some problems with its nitrate implementation, and e.g. pulled these results from AeroCom Phase II. Is this issue resolved for the simulations presented here? (I assume so, but still ask since nitrate here seems to be one of the drivers of intermodel differences.)

- Page 13, line 1-10: This section is very interesting, but briefly presented. I would suggest expanding it somewhat, perhaps adding some comparison plots? This would make the study even more useful for future model work.

- Page 15, line 12-30: This section discusses wet deposition results vs observations, and link good performance to a realistic SO4 distribution. However, isn't this also very dependent on the representation of precipitation? The China/Asia region has a lot of variability both in actual and modeled precipitation, and until it's shown that these compare to a reasonable degree I would be cautious about the above interpretation of wet deposition.

- Page 17, line 17-19: It's hard to assess if e.g. "a 3-fold larger clear-sky SW change" is significant without some indication of the internal variability. Since the results in this paper are mostly from 150-year integrations, I would encourage the authors to add more information on the year-to-year variability (i.e. just the standard deviation of the result across the integration, or similar) throughout the manuscript.

- Table 1: The numbers listed here seem to have an unrealistically high precision (e.g. -0.034810. . .) Please give a reasonable number of significant digits, and also include some indication of the internal variability in each model (see previous comment).

Minor comments:

- Abstract (p2): "…and reinforces that caution must be applied when interpreting the results of single-model studies." I believe the results of this paper show that we should be cautions also in interpreting multi-model studies. They are usually just ensembles of opportunity, with little or no observational constraint beyond what is already taken into the model parametrizations. Hence their average values are not necessarily closer to reality, but instead just indicative of the present model diversity.

- Page 3, line 31-32: The Phase II AeroCom study (Myhre et al. 2013, ACP) which you cite later probably belongs in this company.

- Section 2.1: The description of HadGEM3-GA4 is very long compared to the two other models. Could the descriptions be clarified and made more uniform? Perhaps through a table of the most relevant model parameters/physical processes included?

- Page 18, line 1-2: The SO4 forcing is not very sensitive to the vertical distribution, compared e.g. to absorbing species. See e.g. Samset and Myhre, GRL 2011, doi:10.1029/2011GL049697.

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