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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing and climate response to projected 21st century aerosol decreases" *by* D. M. Westervelt et al.

Anonymous Referee #2

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I thank Daniel Westervelt and his co-authors for their first response to my review. Here, I would like to clarify my major points further, and point out that a few additional simulations would strengthen the paper significantly.

1. I acknowledge that the authors mention specific features of their model, such as the strong effective radiative forcing, but the reader is given little support in assessing which of those features affect the main conclusions, and to what extent. I agree that Takemura (2012) and Bellouin et al. (2011) also do not explore the impact of their model idiosyncrasies, but that is a flaw in those studies, and we now need to raise the bar. An important conclusion of Golaz et al. (2011) is that modellers have the freedom to choose the strength of aerosol radiative

forcing – specifically its indirect component – with little impact on present-day climatology. Surely, as long as such a level of freedom exists, an analysis of climate projections must be accompanied by an exploration of different possible aerosol radiative forcing strengths, in addition to exploring different emission scenarios. For example, had the authors used a CM3 version from Golaz et al. (2011) where aerosol radiative forcing is weaker, would they still find that 30-40% of 2100 warming in East Asia is from aerosol decreases?

2. I welcome the authors' efforts to give more importance to analysing and discussing their simulations in a novel way, especially with respect to Levy et al. (2013). However, my experience is that such a rewrite goes beyond major revisions. The more regional and quantitative results need to be seen in light of the comments made in point 1, so multi-model comparisons seem the way to go, and Shindell et al., doi:10.5194/acp-13-2939-2013, 2013 already looked at regional differences. To link different energy use trajectories with different regional climate responses, one would need to carefully assess complex combinations of carbon- and sulphur-rich sources, and switching off emissions from selected sectors is probably an easier option than comparing RCPs which include simultaneous changes. As for correlating aerosol changes and climate response, the authors would need to demonstrate that this method can yield useful results in spite of our knowledge that radiative forcing and climate response are not co-located. Perhaps by doing the analysis zonally?

To strengthen the paper on both counts, I would recommend to either

- run CM3 in configurations where aerosol radiative forcing is weaker (Golaz et al. (2011) seems to suggest that configurations where it is stronger do not exist);
- or run CM3 with aerosol-precipitation interactions switched off.

Doing so for a few ensemble members (and a single RCP, since they are so similar) would give a great and novel view of climate response to aerosol radiative forcing, including the extent to which unconstrained aerosol interactions matter to regional climate projections. It would also ensure that when aerosol forcing changes in future versions of the GFDL climate model, the present paper remains useful. Finally, it would firmly raise the bar for future modelling studies.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9293, 2015.