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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi-model simulations of aerosol and ozone radiative forcing for the period 1990–2015" *by* Gunnar Myhre et al.

Anonymous Referee #3

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In their manuscript "Multi-model simulations of aerosol and ozone radiative forcing for the period 1990-2015", the authors present an updated multi-model estimate of aerosol and ozone radiative forcing for the period 1990 to 2015.

Using an updated emission dataset, aerosol forcing is found to be stronger positive in the new estimate as compared to IPCC AR5 assessment report, which is attributed to a stronger decline of SO2 emissions and stronger BC emissions in the updated inventory. The manuscript is well written and the results are valid. Yet, given the differences in the used emissions and the selection of models, the results are not particularly surprising, which left me a bit confused about the overall scientific objectives of this work. Before recommending this work for publication, I would encourage the authors to emphasize the objectives of their work as well as to deepen the analysis of the process chain leading to the simulated forcing changes.

General issues

- As a whole, I was missing a more in-depth analysis and discussion of how much of the differences to prior scenarios are simply a reflection of emission changes (in terms of magnitude and location) or due to an arbitrary selection of models (which also have changed from their state at AR5).
- As emission changes seem to drive most of the simulated changes, the assumptions leading to these differences should be discussed in some detail. At present, the readers are simply referred to Stohl et al. (2015) but given the importance of these changes it should be possible to repeat the key points here.
- You fit a linear trend to emissions changes, how good (or bad) is this assumption? Are all local emissions changes at least monotonic?
- The use of "forcing" is at times a bit ambiguous. Readers are used to forcing with respect to the pre-industrial period, however you seem to use it for the period of interest. Maybe this could be made more clear?

Specific issues

- *Page 5: "In all of multi-model analyses, differences are not simply proportional to burden because radiative forcing is calculated with different assumptions of optical properties and the radiative transfer calculations."* Forcing depends on more factors than just burden, optical properties and radiative transfer, as has been quantified in recent AeroCom experiments. Please expand on this.

- *Page 5: "BC is generally a more efficient absorber over regions of South and East Asia (increasing emissions) than over Europe and US"* This is ambiguous. Pure BC is likely to be of similar absorption efficiency. Do you mean BC has different coatings (or coating thickness) or do you mean it has higher forcing efficiency (due to surface albedo differences or changes in cloud cover)?
- Table 1: The caption needs to unambiguously describe the table. Acronyms like SOA vs. POM are not clear here.
- Table 2: It is entirely unclear how the comparison was done. Have models been sub-sampled at measurement sites / times to minimize sampling errors?
- Figure 1: The caption is not clear: this should be specific that this is a linear fit etc. . .

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