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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity of modelled sulfate radiative forcing to DMS concentration and air-sea flux formulation" *by* J.-E. Tesdal et al.

Anonymous Referee #1

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Review of 'Sensitivity of modelled sulfate radiative forcing to DMS concentration and air-sea flux formulation' by Tesdal et al.

The manuscript by Tesdal et al. uses a global atmospheric GCM, which includes a representation of aerosol chemistry and processes, to evaluate the effect of various DMS emission estimates on sulfate burden and the subsequent radiative impact. The study builds incrementally on pre-existing work. The manuscript is well written and structured, and the content is appropriate for ACP. However, before publication I recommend that the authors consider the points below, particularly about evaluating the model's ability to reproduce observed quantities.

Major comments

The suitability of the model for the study is not demonstrated. If we are to have faith in the results of the model's response to different perturbations (as part of the sensitivity study), we need to first know that the model is capable of representing the quantities in question. Has the model ever been evaluated against observations of DMS, SO2, sulphate, CCN, CDN, radiation...? Particularly in regions which are dominated by marine aerosol (Southern Ocean). Does the model simulate the observed seasonal cycles of these quantities adequately?

The model uses a basic (bulk) representation of aerosol which does not fully simulate aerosol microphysics (e.g. competition between condensation and new particle formation, coagulation / interaction between different aerosol species etc). Bellouin et al. (2013, ACP 13: 2027) find stark differences between a bulk and a microphysical aerosol scheme in how they simulate aerosol direct and indirect effects, including the response to a DMS perturbation. Some discussion of the limitations of the aerosol scheme is therefore necessary, and would be particularly useful when connected to an evaluation of model skill vs observations. Calculation of the 'CCN sensitivity' for comparison with the equivalent values calculated in Woodhouse et al., (2010) and others would also be valuable.

I was surprised that greater prominence was not given to the air resistance result and the impact that can potentially have on DMS flux / aerosol. That seems like an important conclusion, and the biggest contribution.

The word 'forcing' is used incorrectly in the manuscript (including in the title). Radiative forcing is the difference in radiation budget between two time periods (e.g. pre-industrial and present day). I think the term 'radiative effect' is what is meant, as the manuscript in question doesn't consider different time periods (except in one paragraph on page 23948: line 25).

It would be very informative to see the spatial responses (change in DMS, sulphate, CDN, radiation... as a map) to the DMS perturbations. Presenting this information would make it much easier to compare and interpret the results against previous work.

Minor comments

Page 23939, line 19: How is the ensemble created? Varying initial conditions?

Page 23939, line 22: 'realizations' = 'ensemble members'?

Page 23939, line 22: It would be useful to state what the spread was between the realizations, for direct comparison to the response from altered DMS emissions.

Page 23944, line 28: Has the statistical significance of the changes been calculated?

Page 23945, line 7: Discussion or presentation of the spatial patterns would be useful in understanding the increase in SO2 but decrease in sulphate. Are any further model diagnostics available to probe this further? It's an interesting outcome and one which it would be useful to understand.

Page 23948, line 20: Vallina et al., 2007 don't go as far as calculating the atmospheric / climate response.

Page 23948, line 20: Wouldn't a low DMS flux result in a lower background aerosol concentration, thus making the system more sensitive to DMS perturbations...?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 23931, 2015.