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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Low sensitivity of cloud condensation nuclei to changes in the sea-air flux of dimethyl-sulphide" *by* M. T. Woodhouse et al.

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Woodhouse et al present a very interesting study with a global offline aerosol model and come to the main conclusion that the role of DMS in climate regulation is very weak. This is a very important, and some would say provocative conclusion. In general I think that the conclusion is well supported and that limitations and uncertainties of the study are well explained. I only have minor comments and suggest publication after these have been taken into consideration.

Introduction

The authors ignore to mention that DMS is also oxidised by BrO, Cl and O_3 (in cloud water) which can be very relevant as even concluded in a study by one of the co-

authors, Boucher et al., ACP, 2003. I think this should be added in a few model runs or at least discussed in some detail as neglecting these reactions introduces further uncertainties as the reaction products are different for these pathways and often don't lead to the formation of new particles but rather to the growth of existing particles (see e.g., von Glasow, Env. Chem., 2007). It is important to point out that H_2SO_4 is the only sulphur species that is involved in the formation of new particles.

Methods

p. 3722, l. 15 - 20: The use of offline oxidants for DMS and SO_2 obviously comes with the limitation that any reduction in the concentration of these oxidants by DMS and SO_2 is ignored and hence the oxidation is potentially overestimated. I would expect a comment on this. Is this a problem for this study (esp. for NO_3) and what regions are especially prone to this?

p. 3723, l. 14 - 21: As all scenarios are described in Table 1 and not only CLIM 2 and CLIM3 I would start this section with something like: "All scenarios are listed in Table 1, here we give more details for each of these scenarios."

p. 3725, l. 5 - 10: I think the "global warming scenario" should be described in more detail at this point. What is the relevant temperature increase and how are the DMS fields linked to the rise in atmospheric CO_2?

Results and discussion

p. 3728, l. 8 - 12: I assume it is unavoidable to use different years for the meteorology driving the model and the years of which you have DMS data from these locations but it would be good to briefly discuss the limitations of this comparison.

p. 3732, l. 3 - 15: Doesn't the lack of presence of sub-mircon sea salt particles in your model limit the extent to which you can draw conclusions? If I remember correctly, previous results from your group have shown some interesting non-linearities plus the absolute number of CCN would be affected as well. Please discuss whether/how the

lack of small sea salt particles limits the applicability of your model study to the overall question of this paper. Also: could you please show a figure that shows the sensitivities that you discuss in this paragraph?

Figures

Figure 2 and 3: I think these figures could be merged.

Fig 4, caption: What do you mean by "land is not included in the meaning."?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3717, 2010.