**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Estimating the influence of the secondary organic aerosols on present climate using ECHAM5-HAM" *by* D. O'Donnell et al.

Anonymous Referee #2

The manuscript presents a model for secondary organic aerosols (SOA) implemented in global climate model ECHAM5-HAM. Since the host model is a climate model rather than a chemical transport model, the SOA model is designed with computational cost-efficiency in mind. Emissions of isoprene, monoterpenes and anthropogenic aromatics are included. Model results are compared to other global models and observations, and some climatic effects are presented.

The manuscript is well written and the presented work provides an important step towards understanding the global aerosol system and it's effect on climate. However, the manuscript can benefit from some clarifications and structural modifications.

Specific comments:

1. The current title emphasises "influence on climate", not comparison to other models/observations or model evaluation. However, the manuscript only includes chapters 3.7 and 5.7 on radiative forcings. Maybe the current title can be rephrased to better match with MS content. You could also go a bit beyond your current end-result (radiative forcings), and sum up your findings with a few sentences of real climate impacts (effects to temperature, precipitation). With the current model setup (nudging) you don't see change in model climate, but maybe some hypotheses can be made.

2. It is not explicitly stated if the calculated indirect effect is due to cloud albedo effect only, or can the changes in CDNC also change cloud lifetime. The latter is assumed, but this could be mentioned in chapter 2.7. While nudging pushes the model towards a prescribed state, it can still allow changes in cloud lifetime to some degree (depending on nudging coefficients). However, the indirect effect here is not necessarily the same as more often used fixed-SST radiative flux perturbation. You should use nudged simulations for comparison to observations, but I would suggest 5-year fixed-SST simulations for the forcing calculations to better take into account changes in cloud properties. Averaging over 5 years might also help reduce the noise in Figs. 11 and 12.

3. Excluding SOA from nucleation mode is mentioned for the first time in chapter 3.5. Model description should be more clear on what tracers are added for the model and why is nucleation mode SOA neglected. This becomes more clear later in the paper, but should be in chapter 2.3.

4. Some material in model description, especially in chapter 2.3, could be left out or placed in appendix.

5. Since it is emphasized that the model is designed with computational cost-efficiency in mind, some estimate of added computational cost should be given (relative to original ECHAM5-HAM). This would give the reader some idea of the usability of the model in long-term climate runs.

6. Figures should be redrawn with a more consistent outlook. Distinct colors (as in Fig.6) are better than interpolated colors (as in Fig.4). The text above figures in Fig. 7 (and 8) is too small to read: phrases like "annual mean accumulation mode number concentration" and "zonal mean" could be put in the figure caption. Figure 15 has no horizontal lines, as in Figs 13-14.

7. Chapter 3.7: I would like to see changes in CDNC presented before the indirect effect, since the change in CDNC leads to indirect effect.

8. Page 2436, last paragraph: oprical –> optical

9. Figure 11: text inside figure says all-sky radiative effect, should be cloudy-sky radiative effect

10. Table 3: missing footnote 1.

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