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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Understanding the contributions of aerosol properties and parameterization discrepancies to droplet number variability in a Global Climate Model" *by* R. Morales Betancourt and A. Nenes

Anonymous Referee #2

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Review of the "Understanding the contributions of aerosol properties and parameterization discrepancies to droplet number variability in a Global Climate Model" by Morales Betancourt and Nenes

The authors have applied CAM5.1 global atmospheric model to 1) compare changes in cloud droplet number concentrations (CDNC) arising from the use of different cloud droplet (CD) activation schemes, 2) investigate, on parametric basis, sensitivity of CDNC to changes in the key input parameters arising from differing emissions be-

tween pre-industrial (PI) and present day (PD) emissions. Regarding the latter issue, the sensitivities are calculated using adjoint method which the authors have developed previously (Karydis et al., ACP, 2012), allowing for numerically efficient calculation of the derivatives of CDNC in each grid box.

The approach of the manuscript is sound and the results are novel enough to warrant publication in Atmospheric Chemistry and Physics. I have some relatively minor comments that the authors need to consider before publication, though.

General comments

1. The two main topics (listed above) are not very well connected in the manuscript (for example, if I understood correctly, adjoint sensitivities are calculated only for PD emissions, and only one parameterization scheme is used when discussing about changes in CDNC due to changing primary organics emissions between PI and PD emissions). The authors should bring out more clearly the connections between these two topics, and make sure that the reader can keep track of which parameterization and emission schemes are used when presenting the results (text, tables and figures).

2. Equation 4, page 31493. If I understood correctly, the first term in the right-hand side of the equation is calculated from the adjoint using PD emissions. Does this "hide" assumption that the corresponding partial derivates of CDNC stay the same in PI and PD scenarios? In any case, the use of the equation should be justified and discussed more explicitly as this is the key to understanding the authors' results regarding the sources of variability CDNC. In particular, two issues: 1) do the corresponding differentials vary between PI and PD emissions? and 2) if so, how it would impact the results?

3. Section 3.3.1. In the beginning of the section, it is stated that: "Further apportionment of the impacts of aerosol emissions on N_d requires the adjoint of the aerosol module (e.g., Karydis et al., 2012a), which is not yet available for MAM3.". I do not argue about the importance of POA, but it should be put in context of relative impacts of various aerosol (and their precursor) emissions that change between PI and PD sce-

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narios. As this is not possible and given the structure of the section 3 (authors seem to pick one particular issue at the end, leaving the reader a bit baffled about its importance) I would advise to omit section 3.3.1 from the manuscript and use it in a further study probing the issue deeper when the adjoint for MAM3 has been developed.

4. The section 4 could be merged with section 3, as the title of the section states "Quantifying parametric uncertainty with the adjoint approach" while the authors consider a set of parameters related to a single aerosol property (hygroscopicity). Also the choice of +-50% undertainty in kappa should be justified in more rigorously, now the authors refer only to a single closure study.

Minor and technical comments

1. page 31483, line 3. Please clarify the term "statistical emulator".

2. page 31487, first paragraph. Are the results sensitive to the chosen value of alfa_c?

3. page 31495, line 13. Should be "pre industrial"

4. page 31495, line 14. Should there be total derivative of N_d, instead of partial derivative?

5. page 31498, lines 11-15. Here the authors state that "However, the diverse response observed across parameterizations implies that a physically consistent representation of coarse mode aerosol remains a challenge for activation parameterizations, although a recently developed modification of FN addresses this issue altogether (Morales and Nenes, 2013). Has the referred manuscript been published yet? If not, please describe briefly how the issue has been addressed in the referred work.

6. page 31499, line 18. Should here read "parameters" for example, rather than "sectors"?

7. page 31499, lines 22-25. While I admire the authors' work, I would be cautious when using adjectives such as "powerful . . .. information" or "unprecedented" here.

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8. Table 2, third row, first column. Should be “ARG-PD-PIa”. Also, the authors should use consistent acronym for "pre-industrial" as both “PI” and “PIa” are used now.

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