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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Constraints on global aerosol number concentration, $SO_2$ and condensation sink in UKESM1 using ATom measurements" *by* Ananth Ranjithkumar et al.

**Anonymous Referee #1**

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This paper focuses on simultaneously documenting and improving model ability to accurately simulate the vertical distribution of three interlinked atmospheric variables (SO2 mixing ratio, particle number concentration and condensation sink). The authors perform an extensive set of parameter perturbation simulations and compare the results to measurements from the ATom flights, which provide a unique dataset for model validation with a broad geographical coverage. The vertical distribution of particles has been the focus of much research over a long time, with persisting model-measurement discrepancies. This paper focuses on previously less well studied variables and their relationship, hence make an important contribution to the scientific knowledge base. Based on the results from the sensitivity simulations, some suggestions for general



improvements are provided. Overall, the paper is well-written and organized, but it is quite lengthy, and I think an outline of the sections at the beginning would be helpful. In addition, I have some comments, which are mainly minor ones for clarification.

General: - I'm puzzled by the use of model data from a version known to contain an important bug when a corrected baseline simulation is available. I don't see why this cannot be excluded. - Previous literature has pointed to notable differences between the Pacific and Atlantic Ocean in model-measurement comparison, which I am concerned could be hidden here by merging across latitude bands. - It would be helpful with a clearer description of the relationship between SO2 and total particle number concentrations, specifically the role of uncertainties in other aerosol species for the latter.

Specific: Line 89: and by Katich et al. (2018) for AeroCom multi-model data and Lund et al. (2018) for two global models.

Line 100: previous studies have also investigated the role of spatial and temporal sampling specifically for aircraft campaigns, including ATom (Samset et al, Lund et al. 2018). Referring to these could be useful.

Line 120: Could there similarly be a problem with performing only one-at-a-time sensitivity tests in this regard, specifically if they are designed to perturbed parameters known to relate more to one of the three variables one wants to evaluate?

Line 140: is data from all four ATom campaigns lumped together in the comparison with modeled number concentrations? I.e., you're comparing quite different amounts of data points and there would be variations across ATom phases/seasons – does this influence the discussion/interpretation of SO2-particle number relationship?

Line 148: Are these present in addition to the ones above? Please clarify.

Line 163: I'm missing information about emissions used in the model, including natural sources (e.g., DMS, biomass burning). Would be useful to add.

Line 187-188: Is the model run with nudging for all three ATom years? Please clarify.

Line 220: it could be useful with some information about the particle size distribution in the baseline simulation as well.

Line 236: SO3. . .

Line 244: This confused me a bit. What is then shown in Figure 2 – w or wo error? Why do you want to show the erroneous model version at all? I think you need to show (in the appendix perhaps) the difference between the two and add a rationale as to why are you using the default version with the bug at all, and not just the new, correct baseline. If it's only for reference to CMIP6 data, I would suggest moving that to the Appendix all together to avoid it distracting from the current study. (The choice to show both becomes even more puzzling in Figure 4.)

Line 247: would be nice to add these region definitions to Figure 1.

Line 247: Previous studies of other variables/aerosols have shown that there can be significant differences in model-measurement agreement over the Pacific versus Atlantic Ocean in the ATom and HIPPO evaluations. I would encourage the authors to do a similar regional split of their current latitude bands to check if this is the case also for these variables. Might shed some new light and be useful for the community.

Line 260-264: If the bug-fix does not affect SO2, what is the reason for the small differences in the tropics?

Line 343: is it possible to from available literature to provide an assessment of the role of oceanic sources relative to terrestrial, in general? The lack of former here raises the question of what the result of the sensitivity test would be if you did include marine organics or whether that could reduce the bias on its own.

Line 493/494: Would it be possible to somehow distinguish positive and negative biases in Table 2 (and A2), as it's not easy to remember all the details of the results from the first comparison?



Line 532: The two condsink simulations are very pronounced on the bar chart and should be mentioned

Line 565: "can help to reduce" yes, but what is the realism? Given the opposite effect on condensation sink bias, I think this should be rephrased. Maybe say that Ntotal is very sensitive to this assumption or something.

Line 602: It would be great to already at the beginning of section 6 specify that a three-way comparison will be done in section 7. I was left wondering why the combined impacts on parameters were not discussed in the preceding sections.

Line 663: I.e., the NMBF as defined in section 4, or a new measure? Please clarify.

Line 687: I think this should also be mentioned at a much earlier stage

Line 692: it would be good to have a similar split between Northern and Southern hemisphere within the high lats and extratropics here, as for the baseline comparison. (see also my comment to line 244).

Line 713: could you give a couple of examples of such possible structural errors?

Line 746: consistent across latitude bands/regions as well?

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