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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-situ constraints on the vertical distribution of global aerosol" *by* Duncan Watson-Parris et al.

Anonymous Referee #2

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This manuscript uses the GASSP database of airborne measurements to evaluate the aerosol number distribution simulated with the ECHAM-HAM model around the world. It then compares a series of sensitivity experiments which test physical processes controlling the loss of aerosol against these observations. The manuscript is generally clear and the topic is certainly relevant to ACP. In general, the paper needs more detail on necessary background and to substantiate conclusions. Specific comments follow:

1. The model description is incomplete. The manuscript should describe exactly which aerosol sources are simulated (e.g. OA, BC, nitrate, etc.) and previous efforts to evaluate these schemes. For example, the discussion of the ECHAM treatment of SOA on page 28 should have been included in the model description.

2. The sensitivity tests focus on only a subset of physical processes, largely limited

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to loss processes. The authors should acknowledge the role that many other processes play in dictating aerosol number concentrations, including oxidation rates, nucleation, thermodynamic partitioning, and obviously, emissions. It seems equally likely that any/all of these processes could contribute to model bias. The importance of these various processes in the bias could be untangled with an evaluation of speciated mass concentrations. The authors might highlight these complementary approaches (i.e. that number concentration evaluation is perhaps the most relevant metric for climate, but that speciated mass concentrations can provide greater insight into aerosol sources/formation) in both the Introduction (in the paragraph page 2, lines 17-29) and in the Conclusions as a next step to evaluating model fidelity. I note that sulfate mass concentration comparisons are shown in the Appendix – why did the authors not include evaluation of the other species measured by AMS?

3. Page 8, line 11: The statement that "interannual variability in aerosol burden is small" may be true, but the long-term trend is not and given that measurements from GASSP extend almost 2 decades, this statement gives a false impression that variations in emissions are not relevant to these comparisons. This is an inherent weakness of this study (comparisons with only one model year), and in absence of a more detailed analysis of how meteorological and emissions variations over two decades contributes to model-measurement airborne point comparisons, the authors must acknowledge that their comparison is not "apples-to-apples".

4. Similarly, Section 3 explores the temporal sampling aspect of model evaluation, but does not address the fundamental temporal mis-match. It may be more appropriate to evaluate a free-running GCM using a 10 year simulation to capture the role of interannual variation in meteorology and/or emissions, particularly on comparisons with temporally-limited, localized campaigns. Could the authors comment on this?

5. Section 3: Could the authors also comment on why they did not simply average the GASSP measurement to the model spatial resolution (in order to not penalize the model for its inability to reproduce sub-grid variability) rather than using 2 minute aver-

ages?

MINOR COMMENTS

1. Section 2.1: It would be helpful if the authors clearly describe how they will use the measurements in Table 1. It appears that they largely focus on the DMA, OPC, and SMPS measurements, with sulfate mass concentrations from the AMS shown in the Appendix.

2. Figure 1: flight tracks are illegible. Make flight tracks finer so that they don't appear as blotches, and consider different colour scheme that enables differentiation of campaigns in the same region.

3. Page 14, line 6: suggest replace "well" with "best" – biases that exceed 50% are not suggestive of a very good simulation. Similarly, the authors should temper their language on page 27, line 6.

4. Page 14, line 9: should this be 10nm? There is no 20 nm cut-off presented in Figure 4

5. Page 15, line 18: "near the surface" – suggest replace with "in the boundary layer" as these biases extend through several kms

6. Page 15, line 18: "local emissions sources not resolved by the coarse model resolution" seems an unlikely explanation for discrepancies. Sub-grid plumes may not be resolved by the model, but these should not impact medians. Furthermore, emissions inventories generally do include point source emissions and while specific localized sources may be missing from inventories, they are not de facto globally biased low.

7. Page 15, line 25: might insufficient marine organics also contribute to this bias?

8. Figures 5, 6: it would be useful to include the number of points in these sub-set comparisons on the figures.

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