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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Profiling of Saharan dust from the Caribbean to West Africa, Part 2: Shipborne lidar measurements versus forecasts" *by* Albert Ansmann et al.

Anonymous Referee #1

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This is an interesting paper that reports comparisons of simulated and (lidar) measured particle extinction profiles across the tropical atlantic. It's an excellent addition to the literature, as such lidar measurements have not previously been available to dust modelers. The results presented here provide an independent line of evidence for two conclusions that the dust community has been converging towards: (1) dust is too fine in most models, and (2) dust (and especially coarse dust) in the Saharan Air Layer settles out of models too quickly. I recommend that this article be published after some moderate revisions.

Comments:

- The writing can be improved in places. In particular, at the beginning of sections 2 and 3 a brief explanation of what is to follow in that section and how that builds towards addressing the main objective would be helpful. Related to that, reiterating the paper's main objectives in those key locations would help focus the reader.

- I cannot comment on the validity of the lidar retrievals or the POLIPHON method, as this is outside of my expertise. This is quite critical to the paper, so I hope another reviewer on this paper or the companion paper can comment on this.

- The constants that convert measured backscatter coefficients to light extinction coefficients are given as single numbers. However, the light extinction results reported in this paper are quite sensitive to these conversion constants, and so it's important that the authors provide realistic uncertainties on these numbers (and propagate them) so that their results can be interpreted correspondingly.

- P. 5, l. 18-22: The description here of how the extinction-to-volume conversion factors are obtained is a bit problematic, because the cited paper (Mamouri and Ansmann, 2017) is in prep and not accessible. So more explanation should be given here. Does this conversion account for dust asphericity or does it assume spherical particles? Also, references should be included for the assumed particle densities.

- P. 5, l. 25-26: The reader is referred here again to an in prep manuscript (Mamouri and Ansmann, 2017) for details on the methodology to separate fine and coarse dust, so I'd suggest replacing this reference with the published Mamouri and Ansmann (2014). Since this is a key component of the methodology, a more detailed review of this method would be very helpful to the reader. It's unclear to me how the different depolarization ratios allow a decomposition of the signal into fine dust, coarse dust, and non-dust. That still seems like an underconstraint problem, so more explanation is necessary here.

- P. 5, l.28-30: Assuming a height-independent size distribution is an important simplification that requires experimental support. Is this shown by other SALTRACE mea-

surements? Some references are needed here.

- P. 5, l.30-31: the authors here claim “full agreement” with shipborne fine and coarse dust AOT. For this statement to be convincing, it should be shown, especially as the authors use this to further assert that their lidar observations “reflect very well the true fine-to-coarse dust extinction and mass conditions.” Otherwise, these statements should be removed.

- Section 3: Since the simulated fine and coarse dust abundances are compared against measurements in Figs. 7 and 8, the authors should provide the emitted fine and coarse fractions for each model. This helps the reader interpret whether the model discrepancies are due to errors in emission, transport, or deposition.

- P. 13, l. 23-5: I think the authors have enough information to at least hypothesize about the reasons for the model disagreement. For instance, the discrepancy close to source regions, where model errors in transport and deposition are minimal, suggests that models have a problem with their emitted size distribution. The fact that this discrepancy increases with distance from the source region suggests that dust in models is depositing too fast, which the authors already alluded to in other places in the manuscript. I consider these two of the paper’s main take-home messages, so I would suggest the authors sum that up here.

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