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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Particle settling and convective mixing in the Saharan Air Layer as seen from an integrated model, lidar, and in-situ perspective" *by* Josef Gasteiger et al.

Anonymous Referee #2

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The authors investigate processes affecting long-range particle transport in the Saharan Air Layer (SAL) by modeling aerosol property profiles for two scenarios: (1) gravitational settling only and (2) gravitational settling alternating with convective mixing in the SAL. Model results are compared with ground- and space-based lidar measurements as well as aircraft particle counter measurements to determine the most likely scenario.

The paper is overall very well written and a valuable contribution that is suitable for publication in ACP. Nevertheless, I have some comments (below).

(1) The authors initialize their model with a reference ensemble. As a result, there is no variation of the modeled profiles for different time periods (see for example Figures S-1 to S-5). Is there any way to initialize the model with measured profiles (for example

using CALIPSO data) and then compute the profile changes based on the assumptions outlined in the model description?

(2) The description of the computation of the fraction of removed particles (p. 5, l. 4-14) is not clear to me. I am not sure how Equation 5 has been derived and why H_scale has been set to 10 km. Some further detail might be beneficial. Also, it is said that f(r) is calculated for z_fallen(r) < H_SAL with H_SAL being the SAL depth. Have the authors considered a starting height of the particles (not all start from the top)? In the context of l. 4 on p. 5, it might also be worth stating that only one air parcel (vertically reaching throughout the SAL) is considered and horizontal mixing is being ignored.

(3) Two hypotheses are considered: (H1) profile changes are caused by gravitational settling only; and (H2) profile changes are caused by gravitational settling during the night and convective mixing during the day. Do I understand correctly, that gravitational settling is being neglected during the day in H2? I would think that gravitational settling occurs always, irrespective of whether or not convective mixing occurs in addition. Looking at Fig. 10, I could imagine that the additional consideration of daytime gravitational settling in H2 might reduce delta_I at large heights (small dz) and thus lead to a "smoother" profile which compares better to the measured profiles.

(4) The cross-sectional particle radius, r_c, is being used for the computation of the drag force (Section 2.1). However, r_c varies with particle orientation. Has this been accounted for in the model? As the authors assume a random particle orientation, the drag force might have to be calculated as an average over the drag force of single particles with different orientation. Is there any information about whether or not particles are being oriented randomly in nature or if the perhaps align with the flow in some way?

(5) The authors suggest that convective mixing in the SAL occurs due to "absorption of sunlight by the aerosol particles". Would this not lead to stronger heating at the top of the SAL compared to the bottom and thus to a stabilization?

(6) Oceanic measurements (e.g. van der Does et al. 2016 [http://www.atmos-chem-

phys-discuss.net/acp-2016-344/]) suggest, that particles of a few tens of microns can still be transported some distance across the Atlantic. Do the authors have any evidence of this from their measurements? If that is the case, then the Stokes regime might not apply anymore (for the largest particles). How would this affect the modeled profiles?

Minor comments:

(1) P. 2, l. 9-10; To me, the statement “cannot explain their measurements with Stokes gravitational settling alone” suggests that Stokes settling might be too weak. However, the subsequent statement, that they had to “reduce the Stokes settling velocity” suggests otherwise. Perhaps rephrasing would clarify this.

(2) P. 3, l. 27; Suggest using half-blanks between unit-parts to avoid Pa (times) s being read as Pas.

(3) P. 4, l.1; Setting F_g = F_d suggests that a particle would be suspended and not that it would be in “still air”, would it? Unless still air is interpreted such that the particle experiences no vertical movement.

(4) P. 6, l. 10; Suggest using micrometers instead of nanometers for consistency.

(5) P. 7, l. 27; The sentence “We find a decrease of linear depolarization ratio delta_l with height” is somewhat confusing as it is not clear if the authors mean with decreasing or increasing height (or dz). Perhaps reword. This occurs also at other places in the paper (e.g. p. 9, l.4)

(6) P. 9, l. 2; Please clarify which “smaller-scale features” are meant.

(7) P. 9, l.9; Why do two particle shapes need to be mixed within one ensemble? Could the shape-dependence of settling not be investigated using two model ensembles each having only particles of a particular shape? (Perhaps I misunderstood the sentence).

(8) P. 12, l.1; Suggest moving “(thin red line)” to after 4740 m.

(9) P. 17, l.25; is instead of in

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