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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 #1

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The paper tests two hypotheses about convective mixing within the SAL: one that assumes no mixing (H1), and the second that does assume mixing. This second hypothesis (H2) assumes heating due to absorption of sunlight by the dust particles, causing convective mixing during the day, and settling during the night. I think the paper is overall well written, especially the methods and model used are described in detail. However there are some issues that I want to discuss in the review below. The conclusion of this paper is that the scenario as modeled in H1 is unrealistic, leaving H2 to be the most viable option. A real-world scenario can't be as simple as described in H1. However, in many ways H2 also does not seem to be a perfect fit, and in the future perhaps more aspects of the model could be tested, to get insight in what determines

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Discussion paper

the measured lidar profiles.

Specific comments:

Comments described as page number/line number(s)

2/8: Why is particle size described here as radius? The cited reference (Maring et al., 2003) also uses particle diameter.

3/2-3: Do you have a reference for this data collected during the SALTRACE field campaign?

3/13: Six irregular dust particle shapes, is this enough to accurately represent Saharan dust?

3/24: What is the particle density based on? Is there a reference for this?

3/24-25: What about the drag force of an aerosol particle larger than r = 10 μm? There are many studies that observe 'giant' dust particles being transported from the Sahara over the Atlantic Ocean (e.g. (Glaccum and Prospero, 1980;Betzer et al., 1988;Goudie and Middleton, 2006;Mahowald et al., 2014;Middleton et al., 2001;Kok, 2011)).

5/Figure 2: Maybe this figure can be mirrored, so that it resembles the E-W transport over the Atlantic Ocean, as is described in the paper.

5/12: HSAL also decreases westward (Adams et al., 2012), which may affect the modeled results? (As also shown in in the supplementary data, Figure S-11).

6/Figure 3: At inight = 0, the particle concentration of every size class is 1. Is this realistic, since particles are never distributed equally in any given sample? (e.g. (Stuut et al., 2005)). And are these number concentrations or volume/surface distributions?

6/6-7: Can you show the grain-size distributions that you have used in your model? What is the maximum grain size used?

7/15-16: Do you have a reference for this? E.g. Prospero (1996), Schütz (1980).

8/8-9: This may also be true for other aspects, like chemistry (and refractive index) and particle size. In how far have you tested how representative that data is for realistic cases?

9/14-17: Yes, there seems a minor effect when the shape conversion factor is taken as an average, but there is a substantial difference between the different shape mixtures (different colors in Fig. 6). What shape mixture was eventually used for the comparison with POLIS and CALIOP data?

10/10-11: Is there a reference that describes this field campaign and its data?

11/9-10: Why are the backward trajectories not shown? At what heights were the air layers modeled? Maybe satellite imagery can help to track the dust layer? (It seems like the dust was emitted at July 4th) see: https://worldview.earthdata.nasa.gov/

11/18-19: Isn't the particle concentration measured as well? (Section 4.2, page 13) Can't that be used as input for the model?

11/19-20: Why was the top of the SAL set at exactly 4660 m? What was this height based on? Based on the potential temperature it is said to be at 4600 m (11/13-14).

11/20: Why is a SAL height of 4740 m modeled? Apparently it matches the data better, but the data implies the top of the SAL may be at 4600 m (11/13-14), so maybe other parameters have to be adjusted to let the model better fit the data?

12/3-8: It seems that for the lower atmosphere, H1 fits better with the measurements, for both β and δl (Fig. 8b and 8c).

12/14-16: In the case of Fig 8, I don't see why H2 would fit better than H1.

13:5-6: This size range of 0.25-25 μm (radius), is this the same size range as used in the model?

13/8-9: "[...] allowing us to test more directly the size distributions resulting from our hypotheses H1 and H2." Why not do this before the model calculations, and incorporate the data in the model?

13/10: Again, is there a reference for this aspect of the SALTRACE project that describes the data and how it was obtained? Because there are not many details about this in the current paper (e.g. location, flight plan, etc.)

13/11: "[. . .] the data was grouped in size ranges that are affected differently by particle settling." What is this based on?

13/12-14: Why were these particle sizes considered? Why not the even coarser particles (up to r = 25 μm)?

14/1-3: Again, where is this effect of size on particle settling based on? What determines these boundaries?

14/9: This seems like a very low amount of particles per size bin, how reproducible are these numbers? Have more of similar flights been performed in this area to compare the data to?

14/22-23: If this data from SALTRACE is available, why not look at more case studies as in section 4? Since every case is unique, this could give more general insights in the processes involved during transportation of dust within the SAL, related to convective mixing. Why was the case of July 11th chosen for comparison with modeled data?

15/5-6: "We again analyze the upper 1 km of the SAL [. . .]", I assume this was also done for averaging the POLIS data, however it is not mentioned in the previous section (5.1).

16/9-10: "No significant differences in the average profiles were found between daytime and night-time data (not shown)." What does this tell us? That there seems to be convection during both night and day? (which is not assumed in your model)

17/19-23: To me it seems that the agreement between the POLIS data and H2 is also not good, just as the bad fit with H1. With the CALIOP data, however, the fit with H2 is much better.

17/23-24: You seem to discuss the comparison between CALIOP and H2 also in the next paragraph, so maybe this sentence is a bit redundant here.

18/10-14: Could you say a bit more about the supplementary data? If not here, then in the supplement itself?

18/24-25: As mentioned for the previous sections as well, I think the results when comparing the model to POLIS data are not that conclusive, for both the case study and the average POLIS data.

18/28-29: This relation with particle size isn't really mentioned in the discussion before (except for Figure 9).

Technical corrections:

9/8: do you mean "[. . .] the importance of the shape dependence ON the gravitational settling for δl profiles."?

11/15: "The relative humidity AT 4500 – 4600 m [. . .]"

16/Fig 10 (caption line 4): The statistical uncertainty of the mean, do you mean standard deviation? Also, in the text you refer to CALIOP data, however in the legend now it says CALIPSO, which is not very consistent.

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