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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The seasonal vertical distribution of the Saharan Air Layer and its modulation by the wind" *by* C. Tsamalis et al.

Anonymous Referee #3

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Recommendation: Accept after major revisions. The manuscript provides a fairly unique climatology of the dust associated with the SAL, including detailed vertical and seasonal variations. Those results are the strength of the paper. The relationship to the large-scale wind patterns is less well developed, particularly in relationship to the ITCZ and African Easterly Jet. This section (section 4) needs to be improved significantly or removed from the paper.

Major comments:

1) Equation 1: Numerator should read 'number of dust layers'. You do not say whether the denominator is the number of all CALIPSO layers or cloud-free layers. Lin et al. (2008a) and Braun (2010) described the fact that clouds frequently prevent detection of dust. Lin et al. used the number of cloud-free scenes in the denominator, with the

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disadvantage that in some places samples became quite small where cloud cover was frequent. Braun used the number of CALIPSO passes, but stated that resultant frequencies would be diminished in areas of frequent cloud cover. There is no discussion in this paper about the impacts of frequent cloud cover on the DOF values, but there should be.

2) Figures 2, 10-13: The noisiness of the data suggests that there still may be too few samples to be using 1° boxes. It seems to me that you can easily go to $\sim$2.5° boxes to get smoother results without loss of information about the patterns. With all of the noise (and the contour color), it is very hard to see the AOD contours. Instead of showing just one value of AOD, why not show multiple contours (say at values of 0.2, 0.3, etc.) so that more of the pattern can be seen?

3) Pg. 14, lines 454-456: You do not give definitions of the height and depth of the SAL. The height appears to be defined by the peak of the DOF values, but that is a little misleading since the dust extends up to the top of the dust layer, which might also be a reasonable definition of height. Does thickness mean top minus base of the DOF using the arbitrary DOF threshold? If so, how sensitive are the results to changes in this threshold?

4) It is not clear why Figs. 10-13 are in the appendices rather than part of the main paper. They appear to be of greater use than and redundant with Figs. 8-9, so seems like Figs. 8-9 could be eliminated.

5) Pg. 15, lines 487-507: These calculations appear to be very “back-of-the-envelope” in nature. It is not clear if you just estimated the mean wind values visually or did an explicit calculation from the ECMWF data. Given the uncertainties in the inputs, what are the uncertainties (which you refer to in line 502) in the effective fall velocities of the particles and how do these uncertainties compare to the differences between the seasons? Are the differences statistically significant? If the uncertainties are large enough that you feel you are in accordance with Prospero et al (whose values are

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several times your own), then it would seem that the differences between seasons are likely not statistically significant. Better explanations are needed for why your results are sometimes an order of magnitude smaller than the previous studies mentioned.

6) Section 4: The results in this section are fairly cursory, not delving into much detail. The premise seems to be that the shape of the DOF field is largely governed by the mean zonal flow, with the edge of the SAL governed, at least on the northern side, by the transition from easterly to westerly flow. Figures 11-13 show that the dust (e.g., at 700 and 650 hPa) can extend northward of this wind-shift line in the eastern Atlantic, so it is not clear that the wind shift creates a clear boundary for the SAL. A more convincing case might be made if the zonal winds are overlaid on the DOF fields in Figs. 3 and 4. In addition, your analysis ignores the fact that eddies (departures from the mean) may contribute to transport that is not accounted for by the mean flow.

You make some non-sensical statements (pg. 16, lines 541-542; lines 551-552) about strong winds countering subsidence in leading to low DOF values beneath the elevated dust layers closer to Africa but descending to the surface much further westward. This problem is likely just semantics or grammatical, but needs to be corrected. I am assuming that you mean that the stronger winds are just able to transport the dust farther westward before subsidence is able to bring the dust to lower levels. The higher winds do not prevent or lessen subsidence (which your statements seem to imply). You also seem to imply that low-level flow from north and south of the SAL leads to lower DOF beneath the elevated SAL layer by somehow removing dust. In fact, you say in the abstract and conclusions that this flow “scavenges” dust, which does not make sense. The word scavenge implies that dust is removed by this flow, but that is not really true. Instead, the frequent occurrence of these flows simply means that low-level easterlies that might carry dust westward are not common during these periods at these levels. In addition, you seem to neglect what is likely a major reason for the elevated dust layer: that dust is lofted above the moist marine layer near the coast rather than being removed at these lower levels by meridional flows. The flow coming off of Africa will

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follow the isentropes vertically as the dry and warm Saharan air overrides the cooler air over the ocean. Although Braun (2010) shows temperature perturbation rather than potential temperature in his Fig. 2a, one can readily see how the dust base rises at the coast as the hot SAL air moves over the cooler marine layer.

In section 4.5, you seem to contradict yourself somewhat, saying in lines 589-592 that the ITCZ is not a restriction or boundary for the SAL only to say the opposite in the next sentence. For the most part, it seems like a pretty effective boundary in all seasons but perhaps winter. In winter, there seems to be higher AOD south of the ITCZ, but it is not clear that this is dust. How would dust get that far south so close to Africa when the average meridional flow is from the south? This higher AOD would seem to more likely be related to other aerosol sources like smoke or pollution, in which case, the ITCZ might still be a reasonably effective barrier. Ultimately, you are saying that it is a leaky boundary, but you fail to really explain why. You ignore the role that eddies (e.g., African easterly waves or other departures from the mean) might play in transporting dust across this average boundary. You also fail to relate your findings to previous studies like Adams et al. (for all seasons) and Braun for summer. In fact, it is difficult to see that you add anything really new to our knowledge in this section, so this section could be deleted.

Section 4.6 is even less insightful, largely just summarizing findings from other studies. Particularly worrisome is that you seem to imply that the AEJ is found only close to Africa, even during summer. At that time of year, the jet can be found to extend well westward over the Atlantic (hence, the stronger mean easterlies during summer). Individual examples of the jet extending to at least 50°W or farther westward can be found in Karyampudi and Carlson (1988), Fig. 6 of Dunion and Velden (2004), and Fig. 7 of Braun (2010). You also imply that the dust layer extends higher than the jet, and is therefore not fully transported westward by the jet. However, the jet is more than its peak value. The jet max is part of the deeper easterly flow associated with the SAL temperature gradient and that extends from the top of the boundary layer to about the top of the dust layer, with the peak of the jet found within the dust layer. So the jet is responsible for most of the westward transport of the dust, just not necessarily all at the peak level of the jet. Overall, I find this section to offer nothing new and ask that it be deleted.

7) The manuscript could use the help of a good native English editor.

Minor comments

1) Line 41: The SAL itself (from base to top) is relatively unstable since the temperature profile is dry adiabatic. The high stability arises from the fact that the SAL overrides a cooler marine boundary layer.

2) Line 55: Jenkins et al. argued for convective invigoration but could say nothing about whether the dust actually led to weaker storms. Any such statements would have been pure speculation.

3) Line 60: Braun only looked at storms that became tropical storms rather than all disturbances, and didn't address dust impacts, so this should probably read "noticed that the SAL's thermodynamic and kinematic properties are not a determining factor for the intensity change of tropical cyclones once they became named storms". Otherwise, it implies that they were also talking about differences between developing and non-developing disturbances.

4) Line 104: What do you mean by "or less focused"? Not sure what this refers to.

5) Line 234: Change conformal to consistent.

6) Lines 241-244 and 253-259: This is all in the figure caption, so do not repeat in the text.

7) Lines 323-324: Explain this further. Are dust events decreasing or not? Why are they not consistent?

8) Lines 326-327: Speculative, particularly in regards to Asian dust. SAL dust can

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extend to northern latitudes when it gets caught up into recurving hurricanes or pull up by mid-latitude systems. So I would not discount them as real dust events.

9) Lines 373-375: Although not specifically mentioned by Braun (2010), these results are very similar to his, so perhaps some comparison is warranted. The same might be said for Adams et al.

10) Lines 449-451: Not sure what you mean by it being below the SAL. The dust clearly shows that the SAL is sometimes at these lower levels, but just below your arbitrary frequency threshold for the SAL. Line 452: Results are also consistent with Adams et al. (2012) and Braun (2010).

11) Lines 478-479: Not clear on the distinction between large-scale subsidence and clear-sky subsidence. How are they different and how, if at all, can you tell them apart?

12) Lines 512-514: These levels do not necessarily correspond to bottom, middle, and top since they vary by season and longitude. Since 500 hPa is usually above SAL top (except maybe during summer), why not use 600 hPa, which on average is closer to the top? For the MODIS data, why use a threshold of 0.5 when it is rarely seen? Instead, use values at 0.1 or 0.2 intervals so that more of the structure can be seen.

13) Lines 545-548: Since dust does not extend up to 500 hPa, these winds do not explain much of anything and it seems that you cannot reach this conclusion. Why not discuss the 600 hPa winds in Fig. 11 instead of talking about Fig. 8? At 600 hPa, the wind shift occurs near the axis of the DOF max, which seems to counter your apparent argument of little dust north of this wind shift .

14) Line 551: The phrase “up to 40°W” does not make sense. With latitude you can say “up to”, but with longitude, it is better to say “westward to 40°W”. I recommend changing this here and in other places where it is used.

15) Lines 556-558: Beyond 40°W, there is virtually no dust at this level, so most of the wind is above the SAL and the flow has little relevance to the shape of the dust layer.

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When you say that the wind magnitude is significant, what do you mean? Significant in what respect? Or do you just mean that it is strong? Also, what is meant by more efficient transport? Is faster transport considered more efficient? Lines 559-560: The wind shift from 700 to 500 hPa doesn't change much, so the shift is really only from very low levels to 700 hPa. As a result, it is not clear that the wind shift controls the shape of the distribution except at lower levels.

16) Line 603: The term African Easterly Jet, and descriptions of it, can be found in many earlier papers, so it is not clear why these much later papers are being used as the key ref

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