We would like to thank all the reviewers for their comments, as we found them very constructive and we really think that they have helped us to improve the quality and the clarity of our paper. Below, there are our answers to reviewers' comments (their comments are in **bold**). Note that when we mention page or line number, these refer to the ACPD paper.

Anonymous Referee #3

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 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.

The following paragraph has been added after Equation 1 in order to account for the raised issues. "In Equation 1, according to previous paragraphs, the number of layers (at a specific altitude z or in total inside the 1 degree bin) stands for dust layers (desert and polluted dust) detected by CALIPSO with the highest quality discrimination from clouds (Feature type QA=3) and after accounting for the vertical overlap in case that occurs. Also, it should be noted that the lidar signal does not penetrate optically thick clouds, thus the systematic presence of clouds in some regions and/or at specific altitudes impacts DOF (one can note that all remote sensing instruments measuring aerosols are affected by clouds and generally the given results are biased towards cloud-free conditions). The presence of semi-transparent clouds is much less an issue as the lidar can penetrate and the backscattered signal from below can be analyzed to retrieve aerosol layers (Winker et al., 2013). Nevertheless, the clouds' impact is expected to be minimum here, as only the number of dust layers with the aforementioned quality criteria is used in the estimation of DOF, instead of the total number of CALIPSO observations or cloud free observations only."

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 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?

We agree that that there is some noise in these figures, but the noise is mainly observed on the edges of the SAL or farther away from it, and may also reflects the natural variability (see also the 2nd comment of referee V. Shcherbakov). Also, we agree with the reviewer that choosing a high resolution leads to empty boxes, as related to the very small swath of CALIOP, which does not cover the whole surface of the Earth even after averaging (in contrast to all the other satellite remote sensing instruments measuring aerosols). It was our choice to use dust aerosol layers of the highest quality regarding discrimination from clouds (thus decreasing the number of available observations, but increasing quality). Smoothing out the noise would give a more presentable view, but it would not change our conclusions. Furthermore adding several AOD contours may lead to a more confuse image (we have tried it before the initial submission of the paper), but it does not add any further information (at least to the purpose and discussion of our paper). The magenta color is used because it is not part of the used colorbar. We thus propose to keep the figure as it was to show the higher possible resolution results without smoothing. However, if the reviewer believes that a smoothed version shows better our conclusions, we are ready to smooth the data.

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?

We agree with the reviewer that this information was missing in our paper. The following sentence has been added on Pg. 4746, line 15.

"The mean altitude and the mean geometrical thickness of the SAL are calculated from the respective vertical frequency distribution at each bin in latitude and longitude, between the surface and the altitude of 7 km, and then averaged to 10° latitudinal zone for every 1° in longitude. The choice a maximum altitude of 7 km for the exploration range comes from the results of previous sections, based on Figures 3-6."

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.

We agree with the reviewer that Figures 8-9 and those of the appendix are somewhat redundant, but they also provide complementary information. The reason for the existence of all them comes from the difficulty to plot in the same figure the wind speed and vectors from ECMWF analyses, the DOF from CALIPSO and the AOD from MODIS. We do not think that it is easy to plot all of them in the same figure and have a presentable figure, as we have tried it before the submission of the paper. Also, we think it is important for the discussion of the paper to give the information on the wind direction, which does not permit to present wind arrows with length analogous to speed. The reason for using Figures 8-9 in the main text instead of those from CALIPSO (and put these more detailed figures in the appendix), is that they are simpler in their presentation.

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 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.

This paragraph has been rewritten in order to take into account the reviewer's comments and present more clearly the analysis done. The estimation of uncertainties in our calculation of the effective dry deposition velocity of dust particles is derived by taking the uncertainty of the observed slope α to be 4 $mdeg^{-1}$, as determined from the adjustment of the regression in Fig. 7, and for the error on wind δu_z to be 1 ms^{-1} (see the text below for the explanation of the symbols). This yields an uncertainty of 0.05 cms^{-1} for the effective dry velocity during summer transport (which is the largest). However, we believe that given the assumptions applied also to the other methods for the calculation of dry deposition velocity (e.g. Prospero et al., 2010), our results are in agreement with them at first order. While it should be kept in mind that we calculate here an effective dry deposition velocity of dust aerosols.

It now reads:

"Results of SAL mean altitude and its decrease with westward transport (Fig. 7), which is linear at first order, coupled to the mean wind speed (Figs. A1–A4) allow estimating the effective dry deposition velocity (combining dynamical forcings and dry sedimentation) of dust particles. The estimation is based on the simple assumption that both the effective dry velocity and the wind speed can be thought as almost constant during the westward transport of the SAL. By using the following equations:

$u_d = \frac{\Delta Z_{SAL}}{\Delta t}$, $u_z = \frac{\Delta x}{\Delta t}$ and $a = \frac{\Delta Z_{SAL}}{\Delta x}$

we obtain:

$u_d = a * u_z$ (2)

and its uncertainty $\delta u_d = \alpha * \delta u_z + u_z * \delta \alpha$

where u_d stands for the effective dry velocity, u_z is the mean zonal wind speed, Δx and ΔZ_{SAL} are the zonal and vertical displacements in the time period Δt and α the decrease of SAL mean altitude (Z_{SAL}) with the westward transport.

The mean altitude of SAL is between 1.5–2 km in winter, 1.5–2.5 km in spring and fall, and 1.5–3 km in summer (Fig. 7-left). These values correspond to pressure levels of about 800–850 hPa in winter, 750–850 hPa in spring and fall and 700–850 hPa in summer. At these pressure levels the mean wind speed from ECMWF (for the same zones as for the mean altitude) is 6.6 ms^{-1} in winter [longitudinal range: 5.5-8.5 ms^{-1}], 6.6 ms^{-1} in spring [longitudinal range: 6-8 ms^{-1}], 8 ms^{-1} in summer [longitudinal range: 7.5-8.5 ms^{-1}] and 5.2 ms^{-1} in fall [longitudinal range: 5-5.5 ms^{-1}] (for wind speed at 700 hPa and 800 hPa see Figs. A1–A4 and also Fig. 9-left of the next section). By taking that 1 degree of longitude equals about 110 km near the equator, this means that 1 degree is covered in about 5 h for wind speed of 6ms^{-1}. In the same time period, the SAL mean altitude decreases with the values mentioned in the previous paragraph. Thus, after accounting for the average seasonal wind speed and applying Equation 2, the effective dry deposition velocity of dust particles is 0.07±0.04 cms^{-1} in winter, 0.14±0.05 cms^{-1} in spring, 0.2±0.05 cms^{-1} in summer and 0.11±0.04 cms^{-1} in fall. Note that the summer effective dry deposition velocity is about 3 times the winter one.

The term effective is used here because the velocities are based on SAL mean altitude decrease and the wind speed from ECMWF, which account for all the processes relevant to the deposition of dust particles, like gravitational settling, turbulent mixing, Brownian diffusion, particle inertia, particle drag (Noll and Aluko, 2006; Foret et al., 2006) and the atmospheric subsidence. It should be noted that according to PRIDE observations, Stokes settling is too strong and an upward velocity is needed to account for the changes in dust particle size distribution (Maring et al., 2003). Generally, dry deposition velocities for dust particles based on collection of samples at local scale have been estimated to be close to 1 cms^{-1} with possible range for a case study over Mediterranean between 0.1 and 6.9 cms^{-1} depending on the used aerosol distribution for its calculation, which in turn is modulated by the contribution of large particles (Dulac et al., 1992). Our results lie within this range. Prospero et al. (2010) reported dry deposition velocities for different stations over Florida in the range 0.23-0.89 cms^{-1} during summer, with their 'best' stations yielding very similar values of 0.23 and 0.30 cm^{-1}. These values are in accordance with our results. However, for winter months they found very large deposition velocities in the range 1.30–3.13 cms^{-1} (with their 'best' stations yielding values 1.30 and 1.72 cms^{-1}), which are much higher than our results. In addition, their winter results are higher than the summer ones, which is in contrast with our findings. It should be noted that during winter Florida is not in the main pathway of SAL (Section 3.1), and it is possible that their results either are affected by local dust sources or reflect a limited number of Saharan dust outbreaks reaching Florida during winter. Furthermore, our estimation considers spatial analysis, which includes larger scale dynamical forcings, thus it may be different than local ones, estimated from time analysis. In any case there are not readily implemented techniques to measure dust deposition to the ocean (Prospero et al., 2010) and this is the reason for the limited number of observation studies dealing with dry deposition velocity and consequently its relatively high uncertainties. Thus, further studies are needed at several locations, especially during the winter period close to the northern South America."

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.

As in section 3, the same threshold of 0.35 in the occurrence frequency is used in order to define the limits of SAL (transition from green to yellow color according to our colorbar) and in this case the SAL does not extend northwards of wind-shift line (we suppose that the reviewer refers to the summer season, when dust reaches higher altitude). To further support our results, we provide an additional version of the Appendix figures by only showing the DOF values above 0.35. Concerning, the impact of eddies this is eliminated in a statistical study like ours. However even in specific cases of SAL, their impact should be limited northward of the wind-shift line, as then the wind direction change prevents them to develop farther northward.

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 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.

We agree with the reviewer that the sentences are not depicting clearly our ideas. For this reason they have been rewritten (note page and lines refer to ACPD version submitted paper).

Abstract (page 4728, line 27 – page 4729, line 1) now reads:

''During winter, the trade winds transport SAL towards South America, while in spring and summer they bring dust-free maritime air masses mainly from North Atlantic up to about 50°W below the SAL.''

Page 4749, line 25 – page 4750, line 2 now reads:

''Farther west, dust aerosols are found in contact with the surface due to the decrease of the SAL altitude with transport, while the relatively high speed (> $7 ms^{-1}$) of dust-free air masses in mid Atlantic between 10–20°N (Fig. 8-right) delays the efficient mixing of the SAL dust aerosols inside the marine boundary layer westward to around 40°W (Fig. 3-right).''

Page 4750, lines 12-14 now read:

''The high speed of the trade winds ($>7ms^{-1}$) prevents the efficient mixing of dust aerosols inside the marine boundary layer westward to 40°W for the southern part and up to 50°W for the northern part, in accordance with Fig. 4-left.''

Page 4755, lines 6-10 now read:

''During winter the trade winds transport SAL towards South America, while in spring and summer they prevent its efficient mixing inside the marine boundary layer and erode the lower part of SAL by bringing dust-free maritime air masses from North Atlantic westward to about 50°W, which is in agreement with previous studies. The trade winds from the Southern Hemisphere erode the low levels of SAL southern part less efficiently , but its structure can be still clearly observed, especially during summer.’’

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.
In section 4.5, our main result is that the ITCZ is effectively not a rigorous south boundary of the SAL during winter, summer and fall over eastern Atlantic. The previous studies mention the ITCZ as the south boundary of SAL, so we should emphasize this difference. It is indicated here by the use of the word ‘rigorous’. We agree with the reviewer in our conclusions, when he states that ITCZ ‘**seems like a pretty effective boundary**’ or later ‘**a reasonably effective barrier**’, so to our understanding we are saying the same thing with non-appropriate words. Regarding the south meridional flow during winter, maybe there is possibly a contamination from biomass burning aerosols up to 900 hPa, but at higher altitudes e.g. at 800 or 700 hPa the flow is easterly, which excludes this possibility (our results with DOF only from the desert dust class, confirm this statement). In order to further clarify our conclusions the lines 6-14 (pg. 4752) now read:
‘‘The possibility of an artifact during winter by the inclusion of polluted dust class (and thus biomass burning aerosols) in the analysis has been excluded in Section 3.1. During summer and fall, it can be noticed that the number of dust layers from CALIPSO is reduced south of ITCZ (located at 10° N and 8° N, respectively) in comparison to northern latitudes (Fig. 2), while also only a part of SAL is found south of 10° N at about 3 km during summer (Fig. 4-left). To summarize, ITCZ does not appear as a rigorous southern boundary of SAL near Africa, meaning that it rather prevents a large number of dust layers to run through it than to totally exclude the presence of SAL inside it. Possible reasons to the fact that SAL penetrates into ITCZ are manifold. It can be due to the influence of eddies related to African Easterly Waves, the different definition of ITCZ over Atlantic and West Africa and consequently the improper use of tropical rain-belt from remote sensing studies to denote the ITCZ over West Africa (Nicholson, 2009) or the displacement of ITCZ during time (e.g. Doherty et al., 2012). Especially during winter, the weaker intensity of ITCZ over east Atlantic may play a role (Waliser and Gautier, 1993, see their Fig. 1).’’

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.
The new results of this paragraph are that only a part of SAL is transported by AEJ and the presentation of the connection between SAL and AEJ for all the four seasons (and not just summer, as in previous studies), as shown in Figures 8, 9 and those of the Appendix. All the studies that the reviewer mentions refer only to the summer period and we totally agree with him that the AEJ during summer is extending at least to 50° W (also obvious from our Figure 9-left). To clarify this point a new sentence has been added in the text.
Page 4752, line 28 now reads
"…firstly the AEJ is found mostly near western Africa, except for summer, in contrast to SAL that reaches…"
Also, we are not taking into account only the peak point of AEJ in order to justify that AEJ transports only a part of SAL. For example during summer at 600 hPa, where climatologically the AEJ peak value is observed (Afiesimama, 2007), the AEJ extends from 10° N to 20° N (Figs. 9-left and A3). On the other hand at the same level, the SAL extends from 5° N to 30° N close to Africa and between 10° N and 25° N close to Caribbean Sea (Figure A3). These results clearly show that AEJ does not cover the latitude band of SAL, confirming that AEJ transports only a part of SAL.

7) The manuscript could use the help of a good native English editor.
We did our best to correct grammatical mistakes.

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.
We agree, so we have modified the sentence in order to clarify it.
Page 4729, line 19 now reads:
"…aerosols with radiation, thus keeping the SAL relatively warm and stable in relation to its environment as it crosses…"

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.
Page 4730, lines 7-9 now read:
"Evan et al. (2006) reported that SAL can suppress tropical cyclogenesis, while Jenkins et al. (2008) mention the possible role of Saharan dust to invigoration of convective bands associated with tropical cyclogenesis."

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 nondeveloping disturbances.
Done.

4) Line 104: What do you mean by "or less focused"? Not sure what this refers to.
We mean that the main purpose for some of the mentioned studies was not to study SAL or Saharan dust transport to America.

5) Line 234: Change conformal to consistent.
Done.

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

7) Lines 323-324: Explain this further. Are dust events decreasing or not? Why are they not consistent?
The number of dust layers is decreasing, meaning that dust events occur less frequently there.
The following sentence has been added to Page 4741, line 7:
"This shows the weakness of DOF as it does not provide any information about the dust load, and the usefulness to couple it with MODIS AOD and the number of dust layers provided by CALIPSO."

8) Lines 326-327: Speculative, particularly in regards to Asian dust. SAL dust can 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.
The following sentence has been added to account for the comment, Page 4741, line 11:
"Although the possibility of dust from Sahara cannot be excluded, the wind pattern seen in Fig. A2, makes it less probable."

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.
In general, we have tried to compare our results with independent observations from CALIPSO (whenever possible). From the figures of these two studies the results appear rather similar. However, Adams et al. do not provide specific values for the maximum DOF (from their figures it is very difficult to see where it is observed), while Braun does not provide the maximum DOF close to America. For the abovementioned reasons we prefer to not add any comment to our paper.

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).
We mean that the dust aerosols seen by Ben-Ami et al. (2009), although present are not part of SAL. The threshold has been chosen in order to reflect the low values of DOF away from SAL, while it is in accordance with the MODIS AOD (Section 3). The two references have been added.

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?
We meant that large-scale subsidence induced by general circulation, may have different impact than clear-sky subsidence due to longwave radiative cooling. We have modified line 12, Page 4747, which now reads:
"…to the descent of the dust aerosols by sedimentation and large scale subsidence due to general circulation, with…"

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.
For us Figures 8-9 and those of the Appendix provide complementary information. The 500 hPa has been chosen in order to more clearly see the shift of the wind direction change. The 600 hPa is provided in the Appendix. The values of AOD less than 0.25 (0.1 or 0.2) do not provide more

information on the SAL, as other species like marine or continental aerosols interfere and make the picture more complicated (see our comment about marine aerosols optical depth on page 4738, lines 25-27). While the value of 0.5 for AOD collocates closely with the maximum wind speed of AEJ, another new element brought from our study. See also our response to major comment 2 of reviewer 3.

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 .
As mentioned in the previous comment, the 500 hPa has been chosen as it is easier to see the southward shift of the westerlies with altitude even if there is no significant amount of dust at this level above the Atlantic (except in summer). The discussion stays the same by using the figures from the Appendix. As one can see from the additional figures with DOF>0.35, our results hold.

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.
Done.

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. 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.
We do not mention something about the flow beyond 40^o W at 500 hPa. In order to clarify our sentence on Page 4750, lines 18-21 now read:
"At 500 hPa, the flow remains easterly up to 25^o N, while the wind magnitude is significant (>7 ms^{-1}) westward to 40^o W, meaning a faster westward transport of dust aerosols at this altitude up to this longitude."
The wind shift at 500 hPa can be observed 5^o southern in comparison to 700 hPa. If someone compares the DOF (Fig. 3-left) finds the same difference latitudinal displacement on the SAL between the two levels, which justifies our results. This means that the wind shift controls the shape of the distribution.

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.
These papers provide some newer results and a more complete description of the AEJ.

V. Shcherbakov (Referee)
Specific comments:
1) Page 4735, Eq. (1). Equation (1) needs thorough explanations in the text. I suppose that "total number of layers in the bin(x,y)" is computed using the parameter "Number_ Layers_Found" of the "Column Record" of the "Lidar Cloud & Aerosol Level 2 layer products". And, the "number of layers(x,y,z)" is computed using the parameters Layer_Top_Altitude" and "Layer_Base_Altitude" of the "Layer Record". The problem is that there are a number of possibilities to assign values of the "number of layers(x,y,z)". I suppose

that the DOF are computed for each pair of (x,y) according to the example given in the comment figure (aerosols were observed only two times at the bin(x,y)). If the example is not in agreement with the authors' computing, it means that even an experienced reader can be misled and Eq. (1) really needs thorough explanations.

Below we repeat our response to major comment 1 of referee 3.

The following paragraph has been added after Equation 1 in order to account for the raised issues.

"In Equation 1, according to previous paragraphs, the number of layers (at a specific altitude z or in total inside the 1 degree bin) stands for dust layers (desert and polluted dust) detected by CALIPSO with the highest quality discrimination from clouds (Feature type QA=3) and after accounting for the vertical overlap in case that occurs. Also, it should be noted that the lidar signal does not penetrate optically thick clouds, thus the systematic presence of clouds in some regions and/or at specific altitudes impacts DOF (one can note that all remote sensing instruments measuring aerosols are affected by clouds and generally the given results are biased towards cloud-free conditions). The presence of semi-transparent clouds is much less an issue as the lidar can penetrate and the backscattered signal from below can be analyzed to retrieve aerosol layers (Winker et al., 2013). Nevertheless, the clouds' impact should be minimized here, as only the number of dust layers with the aforementioned quality criteria is used in the estimation of DOF, instead of the total number of CALIPSO observations or cloud free observations only."

2) Figures 2 and A1 - A4. Personally, I do not consider Figs. 2 and A1 - A4 as noisiness. The authors of the discussion paper imposed the threshold of 240 layers to the bin of 1. Assuming the Poisson statistics, the statistical significance of the "total number of layers" is sufficient. At the same time, an experienced reader can see the natural fluctuations of the number of dust layers on the figures.

We agree with your comment. Below we repeat our response to major comment 2 of referee 3.

We agree that that there is some noise in these figures, but the noise is mainly observed on the edges of the SAL or farther away from it, and may also reflects the natural variability (see also the 2nd comment of referee V. Shcherbakov). Also, we agree with the reviewer that choosing a high resolution leads to empty boxes, as related to the very small swath of CALIOP, which does not cover the whole surface of the Earth even after averaging (in contrast to all the other satellite remote sensing instruments measuring aerosols). It was our choice to use dust aerosol layers of the highest quality regarding discrimination from clouds (thus decreasing the number of available observations, but increasing quality). Smoothing out the noise would give a more presentable view, but it would not change our conclusions. Furthermore adding several AOD contours may lead to a more confuse image (we have tried it before the initial submission of the paper), but it does not add any further information (at least to the purpose and discussion of our paper). The magenta color is used because it is not part of the used colorbar. We thus propose to keep the figure as it was to show the higher possible resolution results without smoothing. However, if the reviewer believes that a smoothed version shows better our conclusions, we are ready to smooth the data.

3) Page 4747, lines 24 - 30. The way, the effective dry deposition velocity was computed, will be clearer if the authors will add the corresponding equation.

Done. Below we repeat our response to major comment 5 of referee 3.

This paragraph has been rewritten in order to take into account the reviewer's comments and present more clearly the analysis done. The estimation of uncertainties in our calculation of the effective dry deposition velocity of dust particles is derived by taking the uncertainty of the observed slope α to be 4 mdeg^{-1}, as determined from the adjustment of the regression in Fig. 7, and for the error on wind δu_z to be 1 ms^{-1} (see the text below for the explanation of the symbols). This yields an uncertainty of 0.05 cms^{-1} for the effective dry velocity during summer transport (which is the largest). However, we believe that given the assumptions applied also to the other methods for the calculation of dry deposition velocity (e.g. Prospero et al., 2010), our results are in agreement with them at first order. While it should be kept in mind that we calculate here an effective dry

deposition velocity of dust aerosols.
It now reads:
“Results of SAL mean altitude and its decrease with westward transport (Fig. 7), which is linear at first order, coupled to the mean wind speed (Figs. A1–A4) allow estimating the effective dry deposition velocity (combining dynamical forcings and dry sedimentation) of dust particles. The estimation is based on the simple assumption that both the effective dry velocity and the wind speed can be thought as almost constant during the westward transport of the SAL. By using the following equations:

$u_d = \frac{\Delta Z_{SAL}}{\Delta t}$, $u_z = \frac{\Delta x}{\Delta t}$ and $a = \frac{\Delta Z_{SAL}}{\Delta x}$

we obtain:

$u_d = a * u_z$ (2)

and its uncertainty $\delta u_d = \alpha * \delta u_z + u_z * \delta\alpha$

where u_d stands for the effective dry velocity, u_z is the mean zonal wind speed, Δx and ΔZ_{SAL} are the zonal and vertical displacements in the time period Δt and α the decrease of SAL mean altitude (Z_{SAL}) with the westward transport.
The mean altitude of SAL is between 1.5–2 km in winter, 1.5–2.5 km in spring and fall, and 1.5–3 km in summer (Fig. 7-left). These values correspond to pressure levels of about 800–850 hPa for winter, 750–850 hPa for spring and fall and 700–850 hPa for summer. At these pressure levels the mean wind speed from ECMWF (for the same zones as for the mean altitude) is 6.6 ms^{-1} for winter [longitudinal range: 5.5-8.5 ms^{-1}], 6.6 ms^{-1} for spring [longitudinal range: 6-8 ms^{-1}], 8 ms^{-1} for summer [longitudinal range: 7.5-8.5 ms^{-1}] and 5.2 ms^{-1} for fall [longitudinal range: 5-5.5 ms^{-1}] (for wind speed at 700 hPa and 800 hPa see Figs. A1–A4 and also Fig. 9-left of the next section). By taking that 1 degree of longitude equals about 110 km near the equator, this means that 1 degree is covered in about 5 h for wind speed of 6ms^{-1}. In the same time period, the SAL mean altitude decreases with the values mentioned in the previous paragraph. Thus, after accounting for the average seasonal wind speed and applying Equation 2, the effective dry deposition velocity of dust particles is 0.07±0.04 cms^{-1} in winter, 0.14±0.05 cms^{-1} in spring, 0.2±0.05 cms^{-1} in summer and 0.11±0.04 cms^{-1} in fall. Note that the summer effective dry deposition velocity is about 3 times the winter one.
The term effective is used here because the velocities are based on SAL mean altitude decrease and the wind speed from ECMWF, which account for all the processes relevant to the deposition of dust particles, like gravitational settling, turbulent mixing, Brownian diffusion, particle inertia, particle drag (Noll and Aluko, 2006; Foret et al., 2006) and the atmospheric subsidence. It should be noted that according to PRIDE observations, Stokes settling is too strong and an upward velocity is needed to account for the changes in dust particle size distribution (Maring et al., 2003). Generally, dry deposition velocities for dust particles based on collection of samples at local scale have been estimated to be close to 1 cms^{-1}, with possible range for a case study over Mediterranean between 0.1 and 6.9 cms^{-1}, depending on the used aerosol distribution for its calculation, which in turn is modulated by the contribution of large particles (Dulac et al., 1992). Our results lie within this range. Prospero et al. (2010) reported dry deposition velocities for different stations over Florida in the range 0.23-0.89 cms^{-1} during summer, with their ‘best’ stations yielding very similar values of 0.23 and 0.30 cm^{-1}. These values are in accordance with our results. However, for winter months they found very large deposition velocities in the range 1.30–3.13 cms^{-1} (with their ‘best’ stations yielding values 1.30 and 1.72 cms^{-1}), which are values much higher than our results. In addition, their winter results are higher than the summer ones, which is in contrast with our findings. It should be noted that during winter Florida is not in the main pathway of SAL (Section 3.1), and it is possible that their results either are affected by local dust sources or reflect a limited number of Saharan dust outbreaks reaching Florida during winter. Furthermore, our estimation considers spatial analysis, which includes larger scale dynamical forcings, thus it may be different than local ones, estimated from time analysis. In any case there are not readily implemented techniques to measure dust deposition to the ocean (Prospero et al., 2010) and this is the reason for the limited

number of observation studies dealing with dry deposition velocity and consequently its relatively high uncertainties. Thus, further studies are needed at several locations, especially during the winter period close to the northern South America."

4) Figures 3 – 6. Generally, the DOF distributions are in very good agreement with the MODIS AOD at 550 nm. At the same time, there are some differences, for example, Fig. 3 (MAM, 20 W) and Fig. 4 (JJA, 20 W). It means that further investigations of the same kind but using layers optical depth or layers integrated-attenuated-backscatter should be fruitful. (The aim of this comment is just to encourage the authors to perform further work. It does not cast any doubt on good quality of the work under reviewing.)
We explain in section 2.1 why we did not use the optical depth (uncertainty about the lidar ratio). However, we agree that this is an important question and we are presently working on it.

Anonymous Referee #2
General
The paper is well written, contains interesting, original, new information with focus on Saharan Air Layer (SAL) based on CALIPSO observations. But I personally find the result sections 3 and 4 too long. I would appreciate if a more condensed presentation of the results could be given (a factor of two reduction of text amount). All the detailed discussions on seasonal differences could be better summarized. We know already a lot about SAL so that one could keep the text short and one should concentrate on the very new aspects.
It is true that a lot was known about SAL, but this knowledge was based either on local observations or campaigns, thus restricted on time or space or both. Our study presents statistical results based on occurrence frequency (DOF), which is not a common measure for aerosols in contrast to clouds, while simultaneously covering the four seasons with relatively fine resolution. Our analysis may appear long, but it is difficult to shorten without losing important points, as at the same time we must present our results, which bring new elements, and indicate where and when the approach of DOF gives significant results by comparing them with previous independent studies. We hope that the referee will understand our opinion.

Another important point is that there is no SAL base height visible in all the figures, the dust layer reaches the Atlantic Ocean surface according to the CALIPSO observations. This is in contradiction with our knowledge of a lofted SAL which is typically above the marine boundary layer, at least during summer. Is that related to erroneous CALIPSO data processing (a bias in the analysis)?
We agree with the reviewer that the detection of the marine boundary layer should be more obvious. However, one can see it during summer and spring (at least for the northern part of SAL) looking to values of DOF smaller than 0.25 (even less than 0.15 during summer) below 2 km from the coast of Africa westwards to 40º - 50º W (Figures 3, 4 and 6). Remind that we are using a threshold of 0.35 for DOF to define the SAL. This is a conservative value, so that when DOF values below this threshold are observed near the surface, it is indicative of high depolarization (based on the classification of aerosol layers from CALIPSO). In turn this points out towards mixing between dust and marine aerosols. This is not astonishing, as SAL during its westward transport fertilizes with micro-nutrients the Atlantic Ocean (e.g. Kaufman et al., 2005). Also on Fig. 1, one can notice dust layers in contact with the surface (for example between 10° N – 15º N), where CALIOP signal, due to its high spatial resolution, can reach the sea surface between broken clouds. In addition, it should be kept in mind that DOF does not provide any information about the dust load, thus it is not clear how important is the dust load found inside the boundary layer, but that it is present, and possibly mixed with marine aerosols (at least on a statistical basis). Finally, there is a bias towards cloud-free conditions (see our response to major comment 1 of referee 3), which is common to all

remote sensing instruments observing aerosols and especially from space.

Section 1, Introduction:

The introduction is very long and very general. I would prefer to have a short general introduction into the topic (the importance of SAL is well known) and immediately introduce your contribution which you are going to present in this paper. And here one could say what is done so far (e.g., with lidar, LITE, AMMA, SAMUM, SHADE, AMAZON lidar, and other attemps). So the last two paragraphs of the introduction are ok. Here I would explicitely mention the campaigs that contributed to this field of research.

Indeed, the first 3 paragraphs (~1.5 page of the ACPD paper) describe the importance of SAL. Even if a lot of things are known, others are open to debate, like the influence of SAL on cyclones and hurricanes. We think that they permit the reader to understand not only the importance of SAL, without entering into too much details, but also to present why the vertical distribution of SAL is important (e.g. radiative effects (direct, indirect), atmospheric stability, deposition, interaction with trace gases). Thus, we propose to keep them.

The campaigns have been added explicitly in the text by the following phrase in Pg. 4731, line 6: "Some campaigns have used lidar observations in order to describe the vertical distribution of SAL e.g. LITE, SHADE, AMMA, SAMUM, AMAZE-08, SALTRACE."

Page 4731, line 5: please add Baars et al., GRL, 2011 (further evidence for smoke tranport to Amazonia)

This paper of Baars is already cited in our paper (page 4740, line 22). However in the abovementioned line we refer to studies that have used CALIPSO data and Baars et al. do not use CALIPSO data.

Page 4735m line 22: biases: : :. Here one should cite Wandinger et al. (GRL, 2010). Because multiple scattering is the main driver for biases in CALIPSO desert dust observations, I believe.

It is one possibility. Another contribution may be through the variability of the lidar ratio depending on the dust type (Schuster et al., 2012). We have added the reference.

Sections 3 and 4, is that an artefact of CALIPSO observations that one does not see the marine boundary layer in all the plots? SHADE observation (Leon et al) and SAMUM observations (Tesche, 2011) clearly see the marine boundary layer below the lofted SAL. Why is there no marine boundary layer in the plots? Is that related to the CALIPSO data processing?

The marine boundary layer maybe observed with different properties closer to the coast, dust being transported aloft (mostly during summer), which may not be the case at large distances, where dust has sedimented and merges with marine aerosols. Also see our previous response (second general comment).

Page 4739: there are a lot of SAMUM winter observations that should be included in the discussion (Tesche 2011a,b, Gross, 2011 a,b, Weinzierl 2011).

The references Tesche et al. (2011) [already included in our paper, but no mentioned in this sentence] and Weinzierl et al. (2011) have been added. The paper of Gross et al. (2011) was mentioned already.

Page 4740, line 7: Here one should provide references again, Ansmann 2009, Baars 2011.

Done.

Figure 1: If possible, improve the figure, colors are not just easy to distinguish. In figure 1 one can see the marine boundary layer!!!!

For the figure, we are using the standard colorbar provided by MATLAB. While we are using different colors for the triangles in order to distinguish between successive detected layers in the same profile (magenta corresponds to first detected dust layer, black to the second and brown to the third one). As we show the overlaps (wherever occur) by white line, it's hard to use another color for the clouds. If the reviewer can propose something we are ready to apply it.
Concerning the marine boundary layer, please see our previous response (second general comment).

Figures 3,4,5,6: Now the marine boundary layer is gone: : :! Why?
It is not gone at least for summer and the northern part of SAL in spring. Please see our response previously (second general comment).

Additional figures

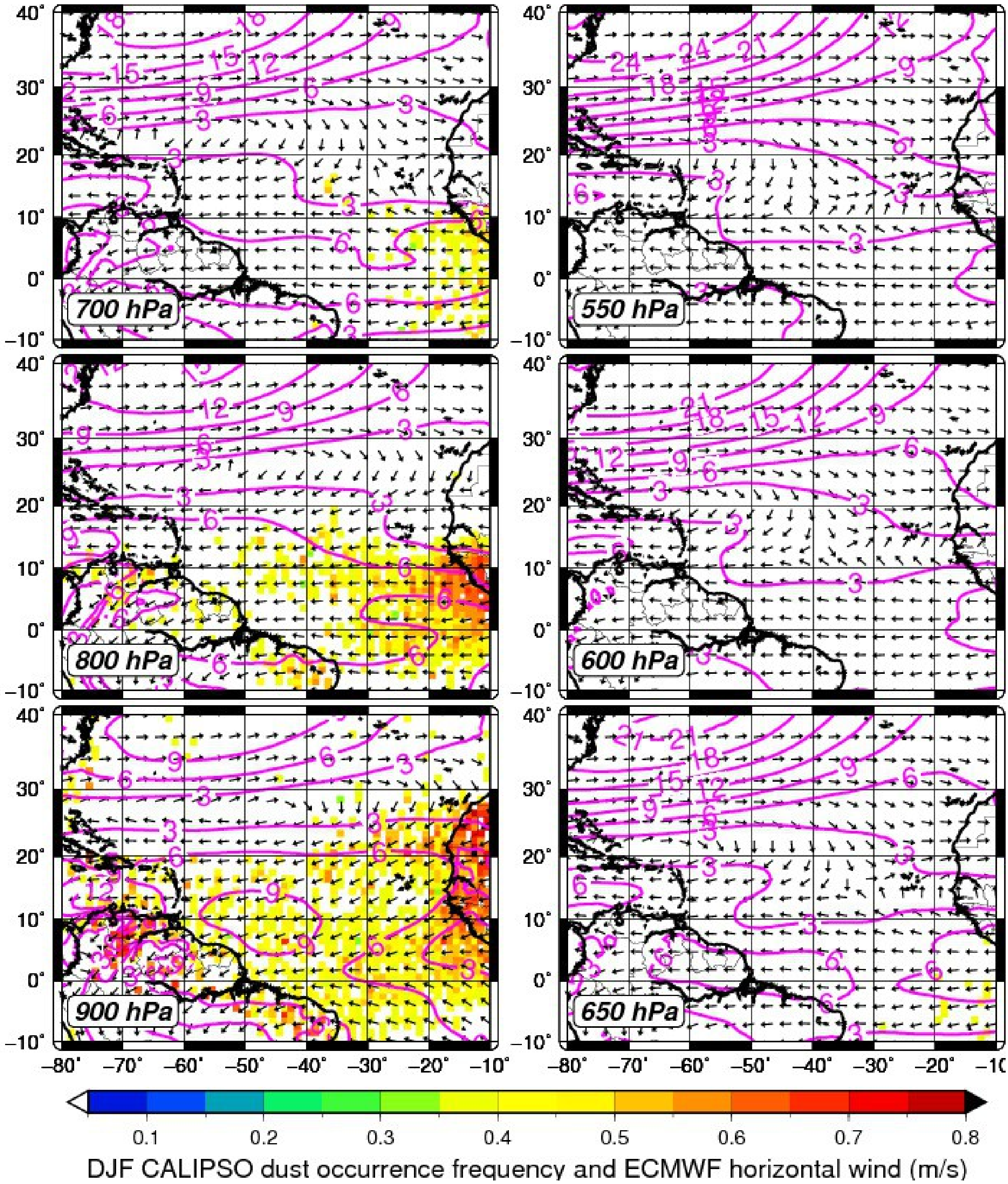

Fig. S1. Same as Fig. A1 of the ACPD paper but only DOF>0.35 is shown.

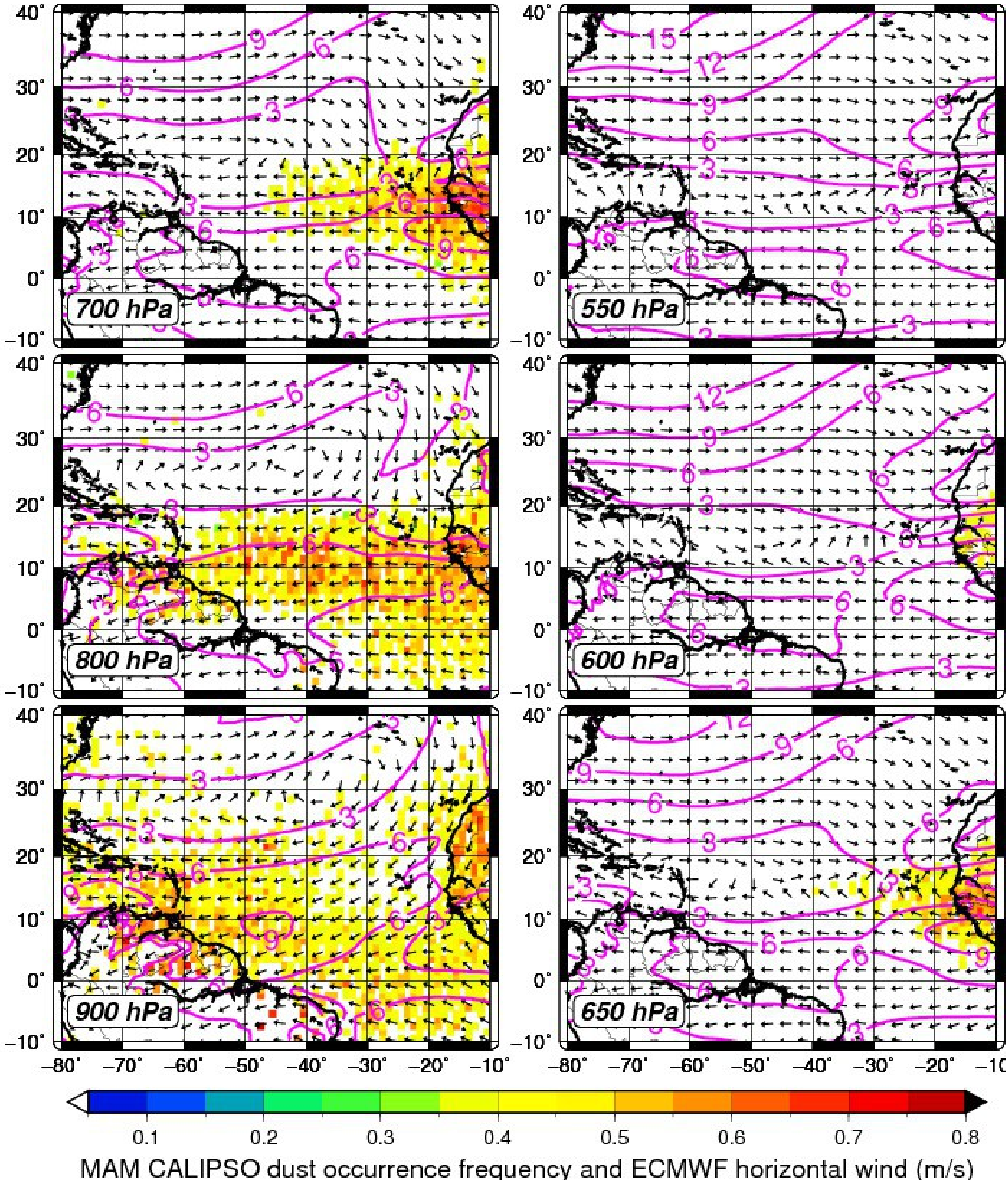

Fig. S2. Same as Fig. A2 of the ACPD paper but only DOF>0.35 is shown.

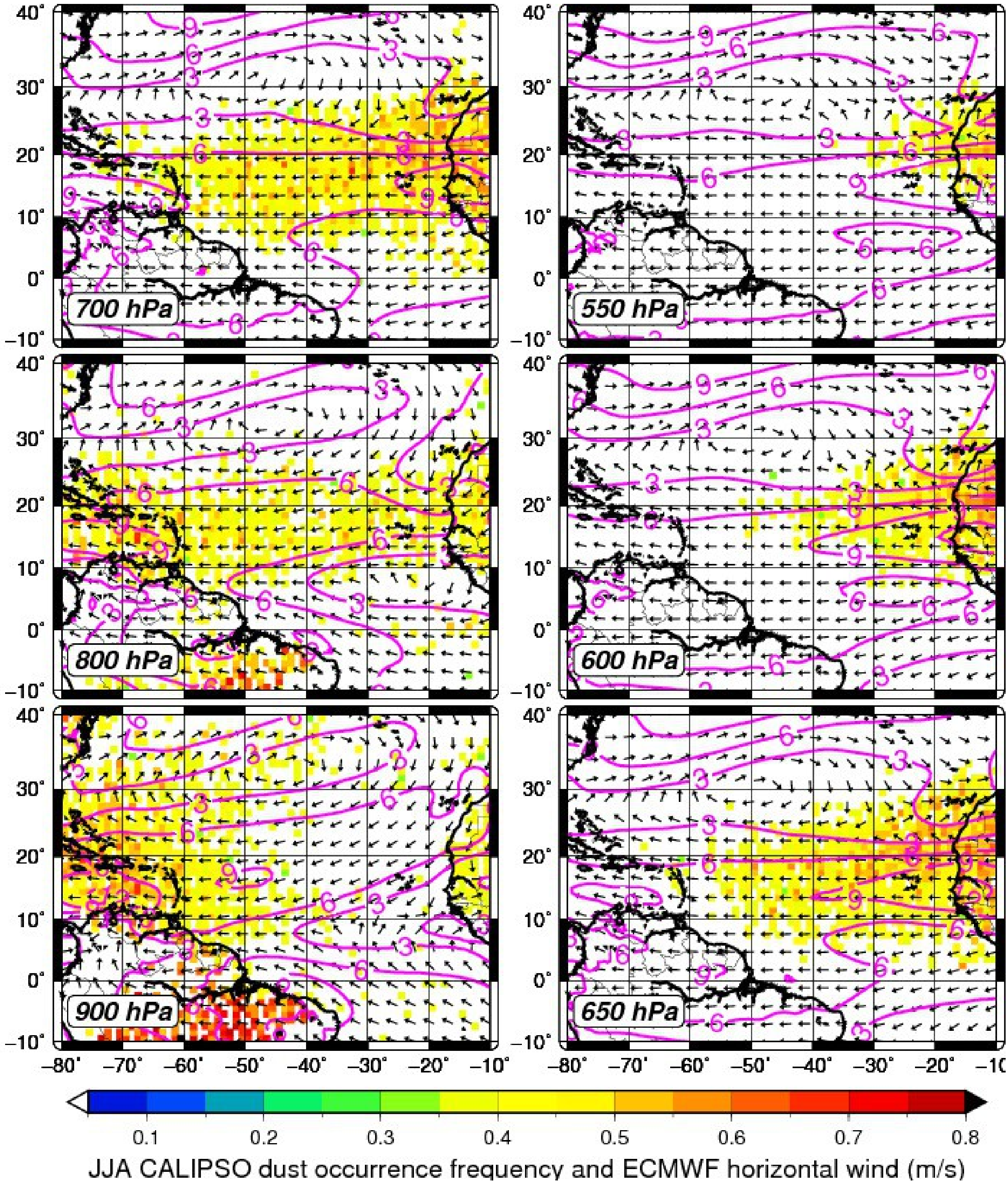

Fig. S3. Same as Fig. A3 of the ACPD paper but only DOF>0.35 is shown.

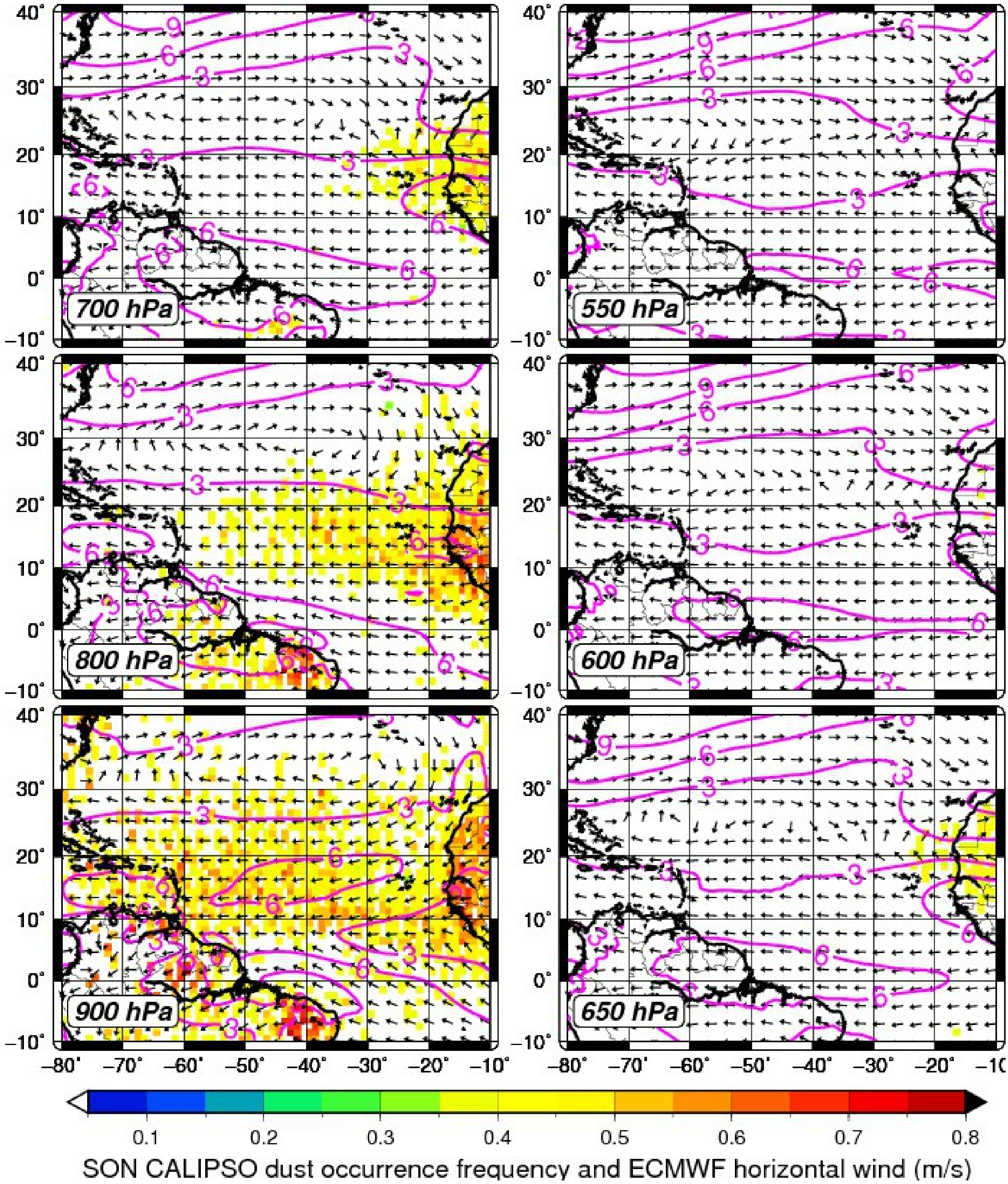

Fig. S4. Same as Fig. A4 of the ACPD paper but only DOF>0.35 is shown.