Review of "Evidence of the complexity of aerosol transport in the lower troposphere on the Namibian coast during AEROCLO-sA" by Patrick Chazette et al.

Summary of manuscript:

This paper uses ground-, aircraft-, and space-based lidar systems to evaluate the vertical structure of aerosol plumes around Henties Bay, Namibia, and the surrounding southeast Atlantic region during the time of the AEROCLO-sA campaign. The observations are divided into three periods with different column loadings and vertical distributions characteristic of each period. Back trajectories are run using the HYSPLIT model at the primary plume altitudes for each period to evaluate transport pathways. The authors interpret the trajectories from the highest altitudes in period 3 as evidence that smoke from South America contributes significantly to the column loading over the southeast Atlantic and argue this must be taken into account in regional studies of biomass burning aerosol radiative effects.

Unfortunately, the trajectory analysis does not directly compare the same altitudes between the three periods, making it difficult to determine whether the transport pathways truly are distinct between these three periods. In addition, the result that elevated smoke in period 3 is of South American origin may be a misinterpretation of the ensemble HYSPLIT trajectories.

Recommendation: This manuscript requires major revisions, particularly in the analysis of the HYSPLIT trajectories. It will, however, be a valuable addition to our knowledge of the complex aerosol vertical structure in the southeast Atlantic region and should be accepted upon adequate revision.

Major issues:

The major issue with the manuscript is the interpretation of the HYSPLIT trajectories, and in particular the interpretation of the result that elevated smoke from South America is of major importance for radiation over the southeast Atlantic.

Section 5 and Figure 11 cannot be interpreted as showing differences in transport pathways between periods, as the authors attempt to do, because the comparisons are "apples-to-oranges" in that the trajectories are initialized at very different altitudes. Even if the circulation had been perfectly steady throughout all three periods, vertical wind shear alone would lead to apparent differences using this methodology. The correct "apples-to-apples" comparison would be to compare trajectories at the same altitudes for all three periods.

One solution could be to divide Figure 11 into three figures: one with the trajectories from 1500-3000 m for all three periods, another from 3000-4500 m, and a third from 4000-6000 m. 2000 m is a fairly large vertical area to lump together, so I would further suggest subdividing from the original three altitude bins. For instance, binning by 1 km increments (1.5-2.5 km, 2.5-3.5 km, 3.5-4.5 km, 4.5-5.5 km, 5.5-6.5 km) could be more illustrative. These are all just suggestions: there are many different ways to expand the analysis to make more meaningful comparisons between the periods.

Another concern relates to the interpretation of the trajectory ensembles. Figure 11c) is currently being interpreted by the authors as showing that some of the elevated smoke is coming from South America. However, there appear to be a significant number of trajectories that are limited to re-circulations around the African continent. Are all of the re-circulating trajectories from below ~5 km? If not, then the ensemble is actually telling us that it is plausible that the air came from South America but equally plausible that it re-circulated locally. In a similar vein, Figure 12 shows that South America is a plausible source for the elevated smoke observed in period 3 but certainly does not prove that this must be the source.

Even if a more rigorous evaluation of the trajectories does confirm that there are meaningful circulation differences between the three periods (which is extremely likely) and that the elevated plume in period 3 is very likely of South American origin (which I am open to but more skeptical of), the significance of these results is either inflated or incompletely explained. In the conclusion, the idea that there may be some influence from elevated South American smoke is deemed “of paramount importance” for the region in the context of aerosol-radiation and aerosol-cloud interactions. However, even accepting the hypothesis that the elevated smoke is South American in origin, it still only comprised ~10% of the column loading of aerosol and is too high in altitude to plausibly influence low cloud microphysics or have relevant semi-direct effects (unless high clouds occur more frequently over Henties Bay than I assume). Given the pre-existing uncertainties in aerosol loading, vertical distribution, and optical properties over the region, I find it hard to argue that an occasional 10-15% contribution to the direct radiative effect is “fundamental.”

Major revisions likely need to be made to the abstract and conclusion based on the updated trajectory analysis. Below I detail additional specific comments that are independent from the issues identified above.

Specific comments:

1. Page 2, Line 38: What metric are you using to determine that southern Africa is the “most important source” of biomass burning aerosol?
2. Page 2, Lines 49-51: There is now an accumulation of evidence from the LASIC (e.g., Zuidema et al. 2018, GRL) and ORACLES (e.g., Diamond et al. 2018, ACP) campaigns that the MBL in the southeast Atlantic often contains quite a bit of smoke.

In particular, the ORACLES campaign in September 2016 observed some very smoke-polluted MBLs not far from the Namibian coast. The MBL-FT dichotomy in this sentence is a bit oversimplified.

3. Page 2, Line 53: The papers from Costantino & Bréon do not show that clouds in this region are particularly sensitive to aerosol increases — indeed, their aerosol-cloud interaction parameter estimates are well within the range of the other literature they cite. They do show an apparent inverse relationship between cloud effective radius and aerosol index when smoke is near cloud tops, which is consistent with a widespread Twomey effect. If the point here is more that marine cloud radiative properties should be particularly sensitive to aerosol increases, the paper from Oreopoulos & Platnick (2008) cited below may be a more appropriate reference, among other suitable choices.
4. Page 2, Line 56-60: The vertical distribution is also incredibly important for indirect effects, not only semi-direct effects. Without contact between the plume bottom and cloud tops, smoke cannot entrain into the MBL and influence cloud microphysical properties. The Costantino & Bréon papers would be good to cite here. Diamond et al. (2018) and Painemal et al. (2014) may also be relevant.
5. Page 2, Line 72: What does "aerosol activation" mean in this context?
6. Page 3, Line 89: Namib desert, not "Namibia" desert
7. Page 3, Line 91: Please define "ALS."
8. Page 3, Line 93: Please define "LEANDRE."
9. Page 3, Table 1: Please explain what the "X" and "-" symbols mean in the table caption.
10. Page 5, Figure 1: In the caption mention that the Henties Bay and Walvis Bay locations are marked by orange dots.
11. Page 8, Line 200: How does the non-colocation of the MODIS area average and Henties Bay affect your results, if at all?
12. Page 8, Line 205: Move "only" to after "aerosols are."
13. Page 9, Lines 251-254: This period of disagreement between the observations and CAMS may be worth exploring further. What is the circulation like then? Does it seem like the FT air is being sourced from a non-biomass burning affected area, or is there perhaps loss of aerosol occurring (e.g., precipitation scavenging) that CAMS may not be capturing?
14. Page 9, Line 265: Why do you refer to the observations as biased with respect to CAMS? Couldn't the CAMS value be off? It might be helpful to explain why either estimate may be different than "truth."
15. Page 9, Table 3: It would be helpful to explain what the uncertainty range is in the caption. Also, for the profiles encompassing a long period of time, how much of the uncertainty in average value comes from remote sensing uncertainties versus real variability over the time period?
16. Page 10, Table 3: How are these profiles divided? For instance, why are the 22/08 profiles divided at 1607?
17. Pages 12-13, Figures 3-4: I don't understand why the figures are divided in this manner. Also, not all potential profiles from Table 3 appear to be plotted (e.g., it

seems like there is GBL data for 22/08 from 1608-2400 that is not included). It might make more sense to divide this into a number of separate figures for each period.

18. Page 14, Figure 5: The color scale here makes the figure very difficult to read. Perhaps using one that ranges from a very light color to a darker color for high AOT would both be more intuitive but also make it easier to read the wind markers.
19. Page 14, Line 294: What do you mean by “evolution”? That term may be misleading because you are not really showing changes over time but rather a time-space cross section following the aircraft.
20. Page 15, Lines 307-309: I cannot find evidence for this statement in the Haywood et al. (2003) paper cited. Parmar et al. (2008) does establish that water vapor is emitted at fire sources along with carbonaceous species but does not claim that this could humidity an entire well-mixed continental boundary layer. Why would the high relative humidity not simply be characteristic of continental air whereas low humidity air be from subsiding tropical or midlatitude air that has been depleted of moisture via prior precipitation?
21. Page 16, Line 342: In what way does elevated RH suggest the aerosol layer must be distinct?
22. Page 16, Line 362: In what way does the existence of non-negligible AEC values suggest that the aerosol has a different origin than the other aerosols observed?
23. Page 18, Figure 9: It would be helpful to make clear which profile corresponds to which dropsonde in Figure 8.
24. Page 19, Line 395: Do you mean “back-trajectory” instead of “retro-trajectory”?
25. Page 20, Line 447: What do you mean by “nebulosity”?
26. Page 22, Figure 12: I would suggest using whichever color scale you choose for Figure 5 here as well.
27. Page 23, Lines 482-483: Couldn’t high observed AOT also be due to aerosol humidification at high RH, versus thin clouds?
28. Page 23, Lines 497-498: What do you mean by “trapped in the FT”?
29. Page 23, last paragraph in its entirety: This is very interesting, but not appropriate to introduce and discuss only in the conclusion. This could potentially be a great addition to the paper as a separate section with some new analysis of the meteorological fields to provide evidence for the claims made.

References:

Diamond, M. S., Dobracki, A., Freitag, S., Small Griswold, J. D., Heikkila, A., Howell, S. G., . . . Wood, R. (2018). Time-dependent entrainment of smoke presents an observational challenge for assessing aerosol–cloud interactions over the southeast Atlantic Ocean. *Atmospheric Chemistry and Physics, 18*(19), 14623-14636. doi:10.5194/acp-18-14623-2018

Haywood, J. M., Osborne, S. R., Francis, P. N., Keil, A., Formenti, P., Andreae, M. O., & Kaye, P. H. (2003). The mean physical and optical properties of regional haze dominated by biomass burning aerosol measured from the C-130 aircraft during

SAFARI 2000. *Journal of Geophysical Research: Atmospheres, 108*(D13). doi:10.1029/2002jd002226

Oreopoulos, L., & Platnick, S. (2008). Radiative susceptibility of cloudy atmospheres to droplet number perturbations: 2. Global analysis from MODIS. *Journal of Geophysical Research, 113*(D14). doi:10.1029/2007jd009655

Painemal, D., Kato, S., & Minnis, P. (2014). Boundary layer regulation in the southeast Atlantic cloud microphysics during the biomass burning season as seen by the A-train satellite constellation. *Journal of Geophysical Research: Atmospheres, 119*, 11288-11302. doi:10.1002/2014JD022182

Parmar, R. S., Welling, M., Andreae, M. O., & Helas, G. (2008). Water vapor release from biomass combustion. *Atmos. Chem. Phys., 8*(20), 6147-6153. doi:10.5194/acp-8-6147-2008

Zuidema, P., Sedlacek III, A. J., Flynn, C., Springston, S., Delgadillo, R., Zhang, J., . . . Muradyan, P. (2018). The Ascension Island boundary layer in the remote southeast Atlantic is often smoky. *Geophysical Research Letters, 45*, 4456–4465. doi:10.1002/2017gl076926