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Discussions

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Interactive comment on "From the Caribbean to West Africa: Four weeks of continuous dust and marine aerosol profiling with shipborne polarization/Raman lidar – a contribution to SALTRACE" *by* Franziska Rittmeister et al.

Anonymous Referee #1

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In April-May 2013 the R/V Meteor did a $\sim$4000 km long transatlantic cruise from Guadeloupe to Cape Verde, carrying a PollyXT lidar which took continuous measurements of the dust vertical structure and optical properties. A hand-held sunphotometer was also used (Microtops), and daily radiosondes were launched. The main datasets and results are published in a GRL paper, together with some elements of meteorological interpretation with COSMO-MUSCAT and HYSPLIT (Kanitz et al, 2014). That paper highlighted the properties of fresh and aged dust, and characterised it in terms of Angstrom exponent, lidar ratio, particle depolarization ratio, aerosol optical depth, and vertical distribution.

The paper by Rittmeister et al builds on that dataset to conduct further analysis of the results. In particular, the optical properties of the marine aerosol are briefly investigated in addition to dust, and four vertical profiles distributed along the ship's track are investigated in more detail in terms of structure, temporal evolution, and airmass trajectories. A conceptual model is discussed, and a nice sketch of the atmospheric layering across the Atlantic is given (Figure 9). A further follow-on paper is announced within the article text, where more detailed derivations will be shown, including the mass concentrations of fine and coarse dust, and their comparison with aerosol transport simulations.

I have a few concerns with the paper. There is a very large degree of overlap with the previous paper by Kanitz et al, with several figures in common as well as repeated information given in the text, and I think that the paper would really benefit from highlighting new findings rather than going again through material already covered in that paper. Moreover, I believe that the presentation of the material would benefit from a major rewrite. I find that the structure of the paper is not always optimal and that some statements are given as granted whereas a justification or a reasoning could be highlighted. Finally, as a follow-on paper is foreseen, I would recommend submitting those two at the same time and with an organic plan. Possibly, after trimming large parts of the present manuscript as I suggest, they could be combined in a single paper.

This is promising research, and it is not light heartedly that I recommend the rejection of the paper at this stage. I believe that it will benefit from a careful revision and restructuring, and I will be happy to review it again, if it is resubmitted along the lines that I suggest.

MAJOR POINTS:

1) Title: This title does not highlight what is new in this paper compared to the previous paper by Kanitz et al. Moreover, if the plan is to have two papers I would encourage submitting them as Part 1 and Part 2 more or less at the same time.

2) Abstract: As for the title, the abstract in the current form does not highlight what is

new in this paper, and describes findings which are similar to those of the Kanitz et al paper. The first four lines drive the reader to believe that this is the first description of this transatlantic lidar transect, whereas this is untrue. Moreover, the findings that are reported in the abstract (lidar ratio and depolarization ratio) add very little. Both title and abstract should focus on new findings.

3) Several figures are simply reproductions of figures in Kanitz et al (2014). I suggest to omit them here, and write the paper around the new findings instead. In the specific: Figure 1 corresponds to Figure 1 of Kanitz et al; Figure 2 corresponds to Figure 2b+c of Kanitz; Figure 3 corresponds to Figure 2a of Kanitz. Moreover, Figure 6 first and last rows are very similar (although not identical) to the data in Figure 3 of Kanitz et al.

4) Many findings are presented as new findings and discussed at length, whereas they are instead previous findings from Kanitz et al. Section 2 also does not describe much new material compared to that paper. I would replace the current section 2 with a short summary of the findings by Kanitz et al (not longer than the abstract of that paper). This can then also permit to reduce some of the reproductions of those findings in the current version of section 3.

5) P3 L30: It is true that a shipborne East-West lidar study has never been performed before 2013, but this has been reported before. This is therefore not the highlight of the present paper. I suggest instead to use an approach like the one on P5 L19: "Kanitz et al (2014) already provided an overview and first results".

6) P5 L21-22: This interpretation of the lidar data to indicate a MBL and a MAL comes as a surprise here as no reasoning behind it is given. With some experience of lidar signals I can easily recognise where the SAL and the MBL are (with help of the depolarization plot) but you cannot assume that all readers have this knowledge. The MAL is an new concept to me, I fail to see it at a glance in this plot, and I have not found a convincing explanation in the paper of how the data presented prove its existence.

7) There are other interpretations of the data given with little explanation in the text;

some of them are listed below (e.g. P5 L29, P6 L8-9, P6 L12-13, P6 L25 and L26, P7 L5-10, P7 L19, P7 L30, P8 L7-8, P8 L34, P9 L3-5, etc.).

8) The "conceptual model" presented in section 4 (P9 L12-32) should really be explained in the introduction.

9) Conclusions: Only 10 lines of conclusions for this work? This is the most important part of the paper. Here you could tie your results with previous research (which you already indicated in the introduction), discuss the caveats and implications, suggest further research, etc. Here is where you justify the benefits of this research in a wider scientific context.

OTHER IMPORTANT POINTS TO CONSIDER:

10) P1 L19: dust as "surface-near" plumes because it is part of the BL: please note that the Saharan BL can be extremely deep (up to 5-6 km in a summertime afternoon); therefore this description is inaccurate.

11) P1 L21: I would think that dust lifted to tropopause height is not as common as the paper describes it; most frequently dust plumes are encountered between the surface and 5-6 km.

12) P2 L1: why do you describe dust as "omnipresent"? Although dust is abundant, I would not think that it is found everywhere (indeed most atmospheric layers around the globe are dust-free).

13) P2 L21: the distance from the W coast of Africa to the Caribbean is about 6000 km; therefore 10,000 km sounds a bit large. The same observation applies to P3 L3 where a distance of 5-8,000 km is indicated.

14) P2 L27-30: the change of topic from dust to smoke is a bit sudden at this point in the paper.

15) P4 L15: between "are available" and "the marine boundary layer" you could add

"from 250 m (full overlap) to XXXX m (limited by SNR), covering ..."

16) P4 L24: you mention separating dust from other aerosol based on the particle depolarization ratio (I suppose this is the method by Tesche et al, 2009) and you base this on an assumption that the depolarization ratio is 0.3 for pure dust. I would challenge this, as ageing along the Atlantic path could change the depolarization ratio of the dust component (and you confirm this fact in this paper actually). I would therefore recommend to take ageing into account when applying this method.

17) P5 L8: I suggest to specify that the radiosondes were launched from the same ship.

18) P5 L13: I recommend to use a word like "prediction" or "computation", because "tracking" usually refers to remote sensing observations (radar, satellite, etc.)

19) P5 L15: I recommend to say "used in conjunction" rather than "combined", unless a new aggregate product has been designed that combines both.

20) P5 L29: The colour scale in the plot indicates the magnitude of the range corrected signal, and NOT the different layers. The attribution of the SAL to "green and yellow colours" (i.e. range corrected signal between 3 and 4.5) is an interpretation, and as such I think it deserves an explicit explanation in the text.

21) P6 L8-9: This statement is substantially correct, but it is not formulated in a useful manner; it has to be clear that it is our interpretation that an AOT of 0.05 corresponds to a dust-free pure marine condition, and that 0.7 corresponds to a major dust outbreak. The lidar data, the backtrajectories, and correct wording can help support this statement.

22) P6 L10: AOT is up to 0.7 (not 0.3)

23) P6 L12-13: larger Angstrom exponent is indicative of smaller particles (and viceversa), not of a given aerosol type. The suggestion that this indicates sea salt or dust is an interpretation, and should be presented as such. In particular, it is reasonable

that as dust travels away from source (as presented in this paper), larger particles undergo deposition and therefore the Angstrom exponent increases but the aerosol type remains "dust". This needs probably to be clarified and accounted for.

24) P6 L14: I appreciate the effort in rationalising what is observed, but before calling the four lidar sections "key stages" I believe that some explanation and discussion could be useful.

25) P6 L16-18: It may be worth specifying that this is deduced from backtrajectories. These trajectories pass over hot spots, and therefore are not capable of ruling out a biomass burning component: this could be explicitly discussed.

26) P6 L20: To give dust an age (7-9 days), how do you determine at which point along the trajectory it was lifted?

27) P6 L25: It is unclear how the statement about mass concentrations is justified.

28) P6 L26: How is the MBL top identified from Figure 4 and how is it found different from the dust base height?

29) P6 L31: smoothing window (365 to 458 m): this is in contrast to the figure, where 457.5 and 562.5 are indicated.

30) P7 L3-4: as Rittmeister et al (2017) is not yet published, may I suggest to cite other existing references about the conversion of optical properties to dust mass concentrations? See e.g.

http://onlinelibrary.wiley.com/doi/10.1029/2007JD009551/full
http://onlinelibrary.wiley.com/doi/10.1029/2000JD900319/pdf
http://onlinelibrary.wiley.com/doi/10.1002/qj.777/full

31) P7 L5-10: which analysis showed that smoke does not dominate this airmass? This is not presented at all in this paper. It is definitely not obvious why these fires do not contribute.

32) P7 L12-13: you say that the backscatter wavelength dependence is due to long-range transport. However, in Figure 6 this applies also to case 1.

33) P7 L19: whereas it is reasonable to think that large particles fall out during long range transport, how does the data support this strong statement?

34) P7 L30: besides the potential mixing with marine particles, the decrease of depolarization ratio could also be ascribed to the ageing of dust (removal of larger particles; coating with water and/or other species, etc.)

35) P8 L1-2: I would remove the hard numbers here and limit to saying that larger/smaller depolarization ratios are expected.

36) P8 L4: In my opinion, the intrusion of marine particles in the SAL has not really been demonstrated in this paper.

37) P8 L7-8: To say that the radiosonde data are in agreement with the lidar observations is again to skip a logical step. Whereas it is clear to me what the authors want to say, I would not think that it is correctly formulated, and as such other readers may find this difficult to understand. I think that the correct statement should be that radiosonde profiles show a consistent layering of the atmosphere with the lidar dataset.

38) P8 L10-11: I am not sure I understand this. In Figure 6, we see that the RH is large below the SAL base and is small above the SAL base (if we take the depolarization profile as indication of where the SAL boundary is). In P10 L4-5 you clearly acknowledge the sharp change in depolarization ratio at the SAL base: this should be evidence against these vertical exchange processes.

39) P8 L14-16: Omit.

40) P8 L18: Here you mention 16 analysed cases. These come as a surprise because they were never mentioned earlier in the paper.

41) P8 L26: Cite literature on the marine LR around 20 sr. Many references exist on

lidar ratio of different aerosol types. For example:

http://onlinelibrary.wiley.com/doi/10.1029/2006JD008292/full

http://www.atmos-chem-phys.net/15/3241/2015/

http://www.sciencedirect.com/science/article/pii/S1352231011006108

http://www.atmos-meas-tech.net/6/3281/2013/

42) P8 L32-33: Maybe removing the lower and upper 250 m of the SAL could prevent the fact that smoothing with a ∼500 m window introduces information from layers below or above?

43) P8 L34: I think it is really an overstatement to say that a LR of 40 sr "clearly" indicates an impact of marine particles. The LR of dust is very variable depending of source region (see e.g. http://onlinelibrary.wiley.com/doi/10.1002/grl.50898/full). Moreover, the authors themselves have already acknowledged in this paper that the ageing of dust can reduce its LR.

44) P9 L1-3: Again I believe that the comments on the depolarization ratio are too sharp, and I would moderate them in terms of possibilistic statements.

45) P9 L3-5: I believe once again that the authors have no evidence for saying that in proximity of the African continent there is no MBL. Indeed, models and campaigns indicate that such a layer exists near the coast. A more plausible explanation could be that the large depolarization ratio is due to fall out of large dust particles from the SAL above.

46) P9 L9-10: This concept has been repeated several times throughout the paper, but I am not persuaded by the arguments as already commented. Ageing mechanisms are plausible causes. There is also no need to repeat a same concept so many times.

47) P9 L19: unclear: "except in disturbance".

48) P10 L14: smoke? not discussed much in this paper

49) Figure 1: omit figure as it is part of Kanitz et al. Continents are not clearly visible.

50) Figure 2: omit figure as it is part of Kanitz et al. The caption does not describe the figure, instead it tries to interpret it.

51) Figure 3: omit figure as it is part of Kanitz et al. The gray-shaded areas are hardly visible when this is printed. It is unclear what criteria where used to delimit them. A longitude x-axis would probably be more useful than a time axis.

52) Figures 4-6 and 9-10: I suggest reversing the order of cases 1-4, to reflect the order of the discussion in the text (P6 L16-22). This would also have the benefit to have the Easternmost panel on the right (and the Westernmost on the left) in Figure 9, i.e. like one would see it on a map.

53) Figure 4: Caption does not explain what is shown (RCS and VDR), does not clarify how the MBL and MAL are distinguished. The data within the incomplete overlap should be treated as missing data instead of commenting on the "blue area" at the bottom. An indication of longitude for each case would be useful. Blue areas in the right hand panel indicate low VDR, which is indicative of dust-free layers; they do not directly indicate dust-free layers.

54) Figure 5 caption: why do you say that the 500 m level is always within the MBL and the arrival heights 1500-3000 m are always in the SAL? I suppose this is indicated by the lidar profiles, but if it is the case it should be clarified explicitely.

55) Figure 6: the difference between the green and light green curves is hard to see. I recommend a better choice of colours. There is a mismatch between the vertical smoothing windows given in the text and those in the caption

56) Figure 7: The large MBL depolarization ratio near the coast is a very interesting features that could deserve more investigation. The figure could benefit from using longitude on the x-axis, instead of time.

ACPD

Interactive comment

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Discussion paper