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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud macro-physical properties in Saharan dust laden and dust free North Atlantic trade wind regimes: A lidar study" *by* Manuel Gutleben et al.

Anonymous Referee #2

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General comments:

The manuscript "Cloud macro-physical properties in Saharan dust laden and dust free North Atlantic trade wind regimes: A lidar study" is focusing on measurements from the NARVAL-II campaign, which took place in the tropical Atlantic trade wind region. The authors use aircraft based lidar measurements to compare Saharan dust laden regions with dust-free regions and look at the influence on the macro-physical cloud properties. The main findings in the manuscript are some significant differences in cloud top height, cloud length and cloud gap length distributions between dust laden and dust-free regions. All in all, the manuscript is very well written. I would suggest

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Discussion paper

the manuscript to be published after major revision. This should address the following points:

Major comments:

Concerning the structure: The weakest part of this paper is the missing explanation about how the dust in the higher atmosphere is influencing the cloud macro-physical properties. The authors mention some papers in the introduction and compare their results to some publications in the conclusions, but there is a section missing which explains the results in detail. For example, the authors mention a paper which shows a convection-suppressing characteristic of the SAL with the main player being a dry anomaly in SAL-altitudes, but they cannot confirm a dry anomaly in SAL-altitudes with their measurements. For this reason, the authors should make clear why they still believe that SAL causes a suppressing characteristic on the clouds. In addition, the authors should make it very clear in the beginning (probably in the abstract or in the headline) that this study is a case study, which is based on four research flights.

Concerning the dataset: In this study, the authors focus on a dataset from the NARVAL-II campaign. In particular, they focus on four research flights where dust and dust free regions were sampled. If the authors would also look at the measurements from the first NARVAL campaign in 2013, they could extend their dataset and improve the statistics.

Concerning the analysis: The authors compare the dust layer properties with the macro- physical properties of the clouds and see some correlations. However, the authors don't exclude other possibilities, which could cause the changes in clouds as well. For example, in their case study from the 19. August 2016, the authors compare a dust-laden region with a dust-free region and argue that in the latter, the cloud top heights reach almost twice as high. But they don't mention that in addition to dust, the dynamical properties are completely different as well. Stevens et al. (2019) showed for the exact same flight the vertical velocity measurements, which explain why there are

more clouds in one region than in the other.

For the same case study, the authors focus only on two single dropsondes. I would recommend to average over the dropsonde data from the single circle patterns to get more robust results. Otherwise, it comes across as cherry picking.

Minor comments:

Page 1, Line 4: "...impact on the Earth's radiation budget..." Shallow trade wind clouds have also an impact on the total precipitation in the tropics (Short and Nakamura, 2000), which has an important role for the boundary layer (Jensen et al., 2000)

Page 1, Line 5: The abbreviation is easier to understand when you change it to: "Next-generation Aircraft Remote-sensing for VALidation".

Page 1, Line 5: You mention the NARVAL studies, which include measurements from 2013, the NARVAL South campaign. In your manuscript, you only analyze data from the NARVAL2 campaign in 2016. By mentioning both campaigns in the abstract, it gives the impression that you analyze both data sets.

Page 1, Line 10 – 15: You mention the macro-physical properties of the clouds (shallower, lower cloud fraction) in dust-laden regions, but you don't explain why and how Saharan dust is causing that.

Page 1, Line 24 to page 2, Line 7: These are the only lines where you explain how SAL influence the cloud macro-physical properties. In addition to that, you should spend more time (before your conclusions) discussing these theories and connecting them to your results.

Page 2, Line 10 – 19: It is good to mention the prior campaigns, even if they didn't focus on trade wind clouds. Nevertheless, did these campaigns look also at the cloud macro-physical properties? If they did, are the results similar to your results?

Page 2, Line 26: You mention CloudSat here. The sensitivity of CloudSat is too low to

measure trade wind clouds properly. That might also be a reason why nobody looked at the interplay between SAL and trade wind clouds before by using these kind of data. The upcoming EarthCARE satellite might change that in the future.

Page 3, Figure 1: You show all flights in the figure, including the Ferry flights. Did you use the measurements during the ferry flights for your study? If this is the case, then you cannot call the studied clouds “trade wind clouds” anymore, because the ferry flights were outside of the trade wind region. Maybe you should only show the area and the flight paths, which you took for your analysis.

Page 3, Line 1 – 2: The sensitivity of the HAMP-radar is not high enough to detect shallow cumulus clouds. In comparison to the lidar measurements it should miss a lot of these smaller clouds. Do you use the radar in your study at all? It sounds like you do, because you say: “ including radar and lidar systems - probably the two most important instruments for vertically highly resolved measurements of aerosol and cloud properties“ . Maybe you can add a sentence to explain why you only analyze the lidar measurements.

Page 3, Line 3 – 5: Here you mention that your study is focusing only on NARVAL-II. The abstract gives the impression that you focus on NARVAL-South and NARVAL-II. The reader might wonder why you don’t look into the campaign from 2013?

Page 3, Line 15: Replace “out” by “eastward”.

Page 3, Line 16: “cruising altitude of $\sim$15.5 km” Under empty (no crew and less fuel) conditions? What was the highest altitude during the campaign?

Page 3, Line 18: What does SMART stand for?

Page 4, Table 1: You mention the research objectives, but never mention the divergence flights, which were a big part of the campaign. For example, during flight 3 and 6 (see Stevens et al., 2019; Table 2). That’s why the flight patterns show so many circles.

Page 4, Line 1: How much is “a large number of dropsondes”? During NARVAL-II 218 dropsondes were launched from HALO (see Stevens et al., 2019).

Page 5, Line 3: “resolution of approximately 200 m” is important for the identification of the cloud size. Later, you look at clouds with a cloud length smaller than 500 m (Fig. 6). Does it mean that these clouds consider only 2 pixels/data points?

Page 7, Figure 3: All your labels use a “/” to separate the Label text from the unit. Why not just writing the units in brackets “[°]”, like you did it in Table 1 for UTC?

Page 7, Figure 3: You could use two different colors for the flight track to mark the dust and no dust regions.

Page 8, Line 5 – 6: Why were only RF4 and RF6 chosen to measure in dust free regions? Wouldn’t it be useful to also take RF1, RF5 and RF7 to RF10 into account for measuring in dust free regions?

Page 8, Line 16: “far south” – How far?

Page 8, Line 24: Why are the trajectories not shown here? You could add another panel to Figure 3 and show the trajectories.

Page 8, Line 28: Maybe you can mark the dropsonde locations in Figure 3. Page 9, Figure 4: The upper left panel should be labeled as “D1”, right?

Page 9, Figure 4: Why are you focusing only on two single dropsondes from each circle? It looks a little bit like cherry picking. I would suggest to average over all dropsondes for each circle.

Page 9, Line 6 – 7: You write “CF is 20 % in dust-free regions. In the SAL-region however, CF decreases to 11 % (including the clouds developing at the edges of the dust layer)”, which gives the impression that only dust is causing the differences. You should clarify that.

Page 10, Line 1 – 2 : “However, in dust free regions cloud top heights reach almost

twice as high and up to 2 km." – This is not a direct effect from the dust. When you look at Figure 4 in Stevens et al. (2019), you see that the vertical velocities are different. Updrafts in the region of the NE circle cause the higher cloud top heights.

Page 11, Line 1 -2: Your conclusion that "less and shallower clouds" are present in "Saharan dust laden trade wind regions" is based only on Figure 5 and the CF. I think this conclusion need more fundamental explanation. For example, Figure 5 shows only a higher CTH fraction for dust laden regions in an altitude between 0.5 and 1.5 km. But why are there higher CTH fractions for dust free regions in all other altitude bins? The dust layer is in all flights between 2 and 6 km altitude. For this reason, you would also expect a higher CTH fraction in dust laden regions between 1.5 and 2 km. I am also wondering about the clouds between 0 and 0.5 km. Are these caused by precipitation? Usually, you would expect a cloud base height at $\sim$700m in the trade wind region.

Page 13, Line 8: Replace "flight" by "flights".

Page 13, Line 12: It looks like you can replace 1.6 km by 1.8 km.

Page 15, Line 5 – 7: You write before this sentence that the main player is a "dry anomaly". And then you say that "Saharan air layers were not found coming along with dry anomalies". For this reason, I think you cannot use this study to explain your results.

Page 15, Line 19 – 21: As a future study you could mention the upcoming EUREC4A campaign.

Page 19, Line 21: Replace "Klingenbiel" by "Klingebiel" Page 19, Line 22: Paper is already online and has a DOI.

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