

## ***Interactive comment on “Aerosol influences on low-level clouds in the West African monsoon” by Jonathan W. Taylor et al.***

### **Anonymous Referee #2**

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The presents and summarises cloud and aerosol observations in low-level clouds over the coast of southern West Africa made during the DACCWA campaign. Further, an analysis of cloud cover in the wider area is presented based on satellite data and the impact of local aerosol emissions on cloud properties is investigated using a parcel model. The authors draw the conclusions that there is discernible impact of local emissions on the cloud properties and that changes in local emission strength would only have a small impact on CDNC and cloud properties.

In general, the paper is well written, the methods are mostly suitable, and the results are for the most part presented well and discussed logically. My main concerns regarding the main conclusions is a lacking discussion of whether the area proportion affected by local aerosol emissions is reflected roughly correctly in the aircraft data.

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Also in several places, it is not clear how the authors arrive at certain statements (s. comments below). If the necessary clarifications are included, I recommend to publish the paper.

## 1 Specific comments

1. p. 2, l. 4 ff: I believe the authors summarise the typical diurnal cycle during the campaign. This should be stated somewhere.
2. p.5, l. 18 & Fig. 1: The text states Fig. 1 is showing the flight tracks of the aircraft, while the caption says it shows the locations of cloud observations. Please clarify this inconsistency.
3. p. 5, l. 26: It would be helpful to state the actual number of days, for which observations over the sea are available here. Both for aerosol observations and cloud observations. Also can you clarify, how and why you arrive at the conclusion that the number of aerosol observations is sufficient to estimate offshore aerosol variability?
4. p. 6, l. 14 ff: For a better understanding of the relevance / meaning of the statistical comparisons, it would be good to include at least a short statement on the sampling times (hours of day), locations and strategies of the different aircraft.
5. p. 6, l. 21 ff: Can you also provide a comparison of the statics of  $R_{\text{eff}}$  other than the mean?
6. p. 7, l. 25: Is this the percentage difference in cloud fraction or the absolute difference in cloud fraction?
7. p. 8, l. 17 ff: The limitations of the modelling scheme are described. However, a discussion on how these assumption could impact the results is not included.

- Could you at least speculate on the impact of the limitation on the results (maybe in the discussion section)?
8. p. 9, l. 15: It maybe would be interesting to have a figure similar to Fig. 2 with the standard deviation (or variance) in cloud fraction. This would allow a better judgement on, how robust the analysis of the mean field is.
  9. p. 10, l. 5ff: Can you elaborate on how you determine whether there is or is not a clear effect on the cloud cover?
  10. p. 11, l. 13: Is there some means of quantifying this fraction? Is it over- or underrepresented in the observational data? Do analysis of cloud microphysical profiles in the polluted region only?
  11. p. 12, l. 27ff: I am a bit lost with the argumentation here. You say CDNC differences are “larger than differences in aerosol”, “correlate with differences in the aerosol vertical profile” and are not “directly related to differences in accumulation mode aerosol”?
  12. p. 13, l. 8ff: Are clouds over the ocean and the land multi-layered. If so the arguments regarding the connection between CDNC and  $R_{\text{eff}}$  need to be considered more carefully, as with observations at the same altitude and varying cloud base altitudes this connection is not robust.
  13. p. 13, l. 16 / 23: What is the basis for the claim of “little systematic spatial variability” and the statement of “fairly homogeneous” clouds inland?
  14. p. 14, l. 5: What do you mean with a “degree of correlation”?
  15. p. 14, l. 13: If this is solely based on Fig. 6b, I do not find the claim of an impact of the aerosol concentration on the daily CDNC very convincing. Just from looking at the plot, I do not think there is any correlation in this data.

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16. p. 15, l. 4: Can you provide any information regarding the position of the flight data relative to cloud base?
17. p. 18, l. 7: What part of the observed vertical velocity distribution are you using for this comparison? How does the observed vertical velocity relate to the cloud base vertical velocity?
18. section 3.1 & 3.2: It should be mentioned somewhere explicitly that the area analysed here is significantly larger than that covered by the aircraft observations. Orographic effects etc. discussed here have only a limited impact on the detailed cloud data.
19. Fig. 9: What does the blue shaded region around the inland line show?

## 2 Technical corrections

- p. 5, l. 23: “ ... included **measuring** emissions ...” (?)
- p. 8, l. 15: “... updraft velocities **and** aerosol number ... ”
- p. 12, l. 14: What is “it” referring to?
- p. 13, l. 5: I believe there is something missing from the sentence starting with “In the free troposphere ...”
- p. 13, l. 6: “This **is** due ...”
- p. 13, l. 8: Do you mean cloud observations at higher altitude were not necessarily higher above the cloud base than observations at lower altitude?
- p. 13, l. 9: “... which is **consistent** with ...”
- p. 14, l. 24: “ ... indicating **a** greater cloud ...”

- p. 16, l. 28: “... updraft **than** in aerosol ...”
- p. 18, l. 5: “... measurements **of** aerosol composition ...”

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