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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What controls the formation of nocturnal low-level stratus clouds over southern West Africa during the monsoon season?" *by* K. Babić et al.

Anonymous Referee #2

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General comments

This discussion paper questions the origin of nocturnal low-level clouds which are found in the boreal summer over southern West Africa. The topic has attracted a lot of publications in the last 12 years, with important advances but still uncertainties on the mechanisms explaining stratus formation. The authors clearly state the pending science questions they seek to answer. The interest of this paper is that it considers a comprehensive observation data set collected during a field campaign in 2016, as well as other relevant data like the recently released ERA5 reanalyses for instance. The authors justify this additional work by the fact that these new data provide observational

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Discussion paper

verification of modeling studies, and expand on the earlier observational studies based on data collected further north. Their aim is to enable a comprehensive overview for the whole of West Africa, which is partly achieved. There is still some limitation on the full bearing of the results since the analysis relies on the stratus detection at the station of Savè only, but the authors verified that, at least in the central part of the region, there is some spatial coherence in the occurrence of stratus / stratus-free (S/SF) nights.

The paper brings very useful new material which help to document the local vertical structure of the atmosphere on clear and cloudy nights, as well as the associated large-scale dynamics, using distant radiosonde measurements from both coastal and inland stations as well as the reanalysis data. Convincing observations are obtained on the role of the specific humidity of the airmass, and to some extent of the direction of the large-scale wind flow, which demonstrates the part played by synoptic-scale conditions. Results on the NLLJ are a bit more fragile. The timing of the jet onset is suggested to have an effect on cold-air advection, but the evidence is mostly based on case studies during intensive observation periods, and as discussed in section 7 there are some discrepancies with earlier findings. However the authors are generally careful in their well-balanced conclusions. On the whole the paper provides a wealth of new results which help to gain a better understanding of the low level clouds, and which are definitely worth to be published.

Specific comments

1. p.3, l.20 : the authors should perhaps downplay a bit the objectives " provide a comprehensive overview for the whole West Africa'.

2. Most of the analysis is based on the initial identification of stratus and stratus-free nights using ceilometer data. A brief discussion of the accuracy of these data would be useful.

3. The number of stratus-free nights (6) is small. The authors are aware in their conclusions of the limitations attached to this small sample, but an earlier discussion

on the issue would be welcome. For instance, is there any risk of seasonal bias? Several of these nights are located in the early part of the season.

4. The representativeness of the S/SF nights seems to be limited to the region around Benin/Togo, as shown in section 3. Perhaps the samples are a bit small, but it could be useful to indicate whether there is any statistical difference between the cloud covers averaged over S and SF nights at each station in figure 3.

5. p.6 l.10 “an earlier arrival of the maritime inflow”: how can we be certain that this flow is of maritime origin, and that the airflow observed earlier is not?

6. Section 5 (IOP cases): it shows very interesting observations, whose interpretation is perhaps more straightforward than for the composite analyses, but care has to exerted on a possible generalization from only two cases. The horizontal advection calculations refer to very different periods of time; is it relevant?

7. p.9, l.32: The pattern shown on the ERA5 wind composite difference between S and SF nights is interesting, with wind anomalies orthogonal to the monsoon flow. Is there any possible connection with the sea breeze?

8. Backward trajectories: how consistent are these findings with the hypothesis of a southern Africa origin of the air mass, as discussed in section 7?

9. Section 7: The authors propose some explanations to the discrepancies found between their study and earlier work. Perhaps the small sample of SF nights may also be taken into account. The authors actually underline this point in their conclusions.

Technical comments

- p.3, section 3.1: we believe that all the data discussed in this section were collected at Savè ?

- p.5, l.31: misprints : processes ; possibly

- section 6 p.9 : it is believed that the ERA5 analyses refer to the composites studied

in sections 3-4, not the IOP periods ?

- backward trajectories : p.5 refers to the levels from 10 to 100 hPa above the surface (i.e., about which elevation?), and p. 10 from 30 and 50 hPa.

- p.10, l.23 : “the two trajectories (during two consecutive nights. . .)” : unclear what are these trajectories and why they are only two.

- figure 4 caption : supersites

- figure 5: which location ?

- figure 6: (a) is supposed to be wind speed and (b) temperature, not the reverse

- figure 9: it seems that the scale of the vectors is not the same in (a) and (b), if one considers colour shadings. This may not be a major problem, but you need to draw the attention of the reader on it in the caption. We also guess that (c) and (d) display horizontal wind vectors.

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