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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of low-level clouds in the West African monsoon system" *by* Anke Kniffka et al.

Anonymous Referee #1

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This study uses the ICON model to investigate the influence of low level clouds over southern West African on the monsoon system. The authors find that in the perturbation region, precipitation depends strongly on the optical thickness of the low clouds. When representing convection explicitly, the model was even more sensitive to the low level cloud thickness. Downstream of the perturbed region there is very little effect on precipitation, due to temperature and moisture having opposing signals. The manuscript is well written and within the scope of ACP and the DACCIWA special issue. I recommend publishing this work after minor revisions and addressing reviewer comments.

Major Comments:

While the explanation of how the clouds were altered in the model was very clear and

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innovative, I have several comments about the general setup of the model and how the simulations were performed and analyzed.

First, why was the ICON model used, and more specifically, why was a global model used for this experimental setup? For such short runs, I would think a regional or at least a nested global model would be sufficient. You state that computational cost limited your runs.

Do you used fixed SSTs or an interactive ocean (or ocean surface)? Since the simulations are so short, I don't expect the treatment of the ocean surface to have much effect, but it would be good to know.

For the EXPL runs, what is the domain of the nested grid? This is probably unimportant, but could have implications as odd things can happen on the boundary between nested grids.

Finally, what period is the data analysis averaged over? Is it the final four days of the five day simulations? This would be congruent with five day simulations having a single day overlap when the model was started every fourth day, however, it should be clearly stated. Is this period the same for analysis of both the local and regional response? The timescale of the local and regional response is an important factor in interpreting your results, so you should clearly state your averaging period for analysis and support your choice with evidence from your simulations (as in the supplementary material) and/or the literature.

When describing the ICON model base state and comparing it to obs and reanalyses it may be helpful to also know how the control simulations compare with the obs/reanalyses/Hannak et al. In terms of cloud fraction and LWP/IWP. Is it a model that produces a reasonable amount of low level clouds in the base state? The discussion on pages 8 and 9 somewhat address this, but since the paper is on clouds, it might be nice to just state how the control simulations cloud fields compare to obs/reanalyses.

The entire paragraph at the end of Page 12 and beginning of Page 13 is a bit confusing and unclear. (Page 12, Line 26 through Page 13, Line 5) Why do you connect the discrepancy in the longwave component with a dynamical response, but not the discrepancy in SSI? Could the change in upper-level clouds account for this difference? It is important to point out that July 2006 might not have been the most average year, but I'm a bit perplexed by why that can explain SSI and not OLR. Maybe I'm missing something here.

Page 19, Line 15: "most striking" is a bit subjective, don't you think? I agree that it is very striking, but I was immediately more intrigued by the low level qv and qc, whose signals can be interpreted as due to changes in vertical mixing, some of which can be explained by TKE. I think describing and explaining the signals in this manner might be more causal, but this, I suppose, is more personal opinion. This is also a massive paragraph and could probably be broken up between these two features.

The first panel in Figure 9 is really fantastic! However, I feel that the aspect of it that discusses the differences between ICON and TRMM as well as the description in the text (Page 21, Line 10 through Page 22, Line 1 "As already discussed . . . gap between the rainfall maxima") should be moved to Section 3.1 where you are discussing the differences in the control simulations and the TRMM. When inserted here it subtracts from the main point of the section which is to show how the perturbation effect precipitation into the Sahel.

Page 22, Line 11: "This may suggest that modulations to the WAM allow a slightly deeper penetration of rainfall into the continent but one month is probably too short to make any definite statements on this area." I am a bit confused by this statement. You are suggesting that only looking at July 2006 might not be enough to make a robust statement?

Page 25, Lines 32-34: "An interesting implication . . . no significant regional impacts." This statement makes it sound like you are generalizing your result to other regions,

how do you support that claim? Or are you focusing on the response to just altering the clouds in the DACCIWA region? Your statement in the summary (beginning Page 31, Line 10 with "Therefore these results . . .") is much more well supported.

Page 28, Lines 18-22: This paragraph feels out of place. Why are you concerned with how quickly the atmosphere returns to its normal state? The statement "impacts on higher and more remote regions can last days" is misleading as the supplemental material suggests that the residual impacts on remote regions are complicated by the chaotic nature of the atmosphere. I think the plot in the supplemental material is quite interesting and helps explore the timescales of the response, as well as the persistence of changes when the altered clouds are removed. It might be useful to plot the envelop of the anomalies in Figure S1, in order to show the variability between the ensemble members. This would help clarify what is a consistent response to the forcing and what is due to internal variability.

What is the remote (Sahel) response of precipitation for the positive values of f_op = 3 and 10 in the PARAM experiments? I realize these are extreme values, but it would be interesting to see if the same signals appear.

Minor Comments & Typographic Corrections:

Many passages would be improved by the introduction of commas, especially the Oxford comma. I've identified a few below, but if you'd like, I would be glad to mark up the submitted manuscript with where I believe commas would help separate clauses and help clarify the manuscript.

Page 1, Line 7: effect

Page 1, Line 24: "do not show skillful forecasts of precipitation for the next days", is awkward, perhaps "do not produce skillful short term precipitation forecasts" is clearer.

Page 2, Line 9: "determined by the WAM system", is unclear, what about the WAM system determines these characteristics? "are connected within the WAM system"

emphasizes the challenge of the interconnectedness of the WAM.

Page 2, Line 32: I would remove “which is currently gaining increasing attention”.

Page 3, Line 17: misrepresenting

Page 3, Lines 19-30: I would reorganize this entire paragraph to flow better.

This study is part of the Dynamics-Aerosol-Chemistry-Cloud Interactions in West Africa (DACCIWA) project (Knippertz et al., 2015) that aims to better understand the consequences of the rapid increase of anthropogenic emissions in West Africa on the local air quality, weather and climate. To the best of our knowledge, it is the first to analyze the radiative impact of the low-level cloudiness over southern West Africa on the thermodynamics and dynamics of the regional atmospheric system in a fully non-linear and systematic way. The analysis is based on a number of targeted sensitivity experiments using the numerical weather prediction model ICON (Icosahedral Nonhydrostatic), systematically changing the optical thickness of the model clouds. This allows us to clarify the impact of the inter-model spread in cloudiness found in Hannak et al. (2017) on the overall monsoon development in both parameterized and explicit regimes of convection. Although aerosols are not directly modeled in our experiments, the effects found for imposed changes of cloud optical thickness also help to understand variations in the natural system brought about by aerosol effects on cloud properties and radiation, which in a similar way control the amount of shortwave radiation reaching the surface or interact with clouds through modifications in the diurnal cycle of the PBL (e.g. Deetz et al., 2018a).

Page 6, Line 4: Green’s

Page 6, Line 7: (Dee et al., 2011), and do not use data assimilation. (the comma helps separate the idea that the simulations are not using assimilation instead of ERA-I, which clearly does)

Page 6, Lines 10-11: Initializing ICON runs at 00 UTC would mean starting the runs

during the development phase of the low-level clouds and therefore the runs were initialized at 12 UTC. (“would mean to start directly” is awkward and “preferred” makes it unclear that the runs were in fact started at 12 UTC)

Figure 2: maybe outlining the box in each subplot would help highlight the similarities and differences

Page 9, Line 11: remove “Similar to Fig. 2” it’s not needed

Figure 4: why is the axis dimension for SLI so large? All the other subplots are so well framed, but this one has so much empty space it looks weird. Maybe 400-430 W/mˆ2 would suffice?

Page 12, Line 6: How do the fully nonlinear processes represented in the ICON . . .

Page 12, Line 26: I feel this might read better as: “The simple linear model used by Hill et al. (2018) allows a rough estimate of how much of the change in the ICON radiative fluxes are due to direct . . . ”

Page 12, Line 29: I think it would be more proper to not capitalize “Increases” after the colon, to signify that it is not a sentence fragment. Usually, you should only capitalize the first word after a colon if the clause is independent.

Page 16, Line 23: equal to

Figure 10: The legend for subplot (e) should have fop = 0.1 for the red curve.

Page 24, Line 4: south-north profiles of Figs. 10a-c for each hour

Page 26, Line 1: There needs to be a better transition here. Begin with a clear sentence that you are shifting back to looking at the PARAM simulations and why, before introducing the figure.

Page 28, Line 9: remove “just mentioned”

Section 4, First Paragraph: The second sentence introduces the idea of representation

of clouds in models, but then immediately returns to the idea of describing the clouds in reality. The paragraph might flow better this way:

In the present study, we analyzed the role of low-level clouds over southern West Africa on the local meteorology and larger monsoon system. They frequently form during the night close to the surface and often persist long into the following day. At their maximum diurnal extent, they cover a vast area of about 850 000 km2 in southern West Africa (van der Linden et al., 2015). Their formation is linked to cold advection and turbulent mixing associated with the NLLJ and radiative cooling (Schrage and Fink, 2012; Schuster et al., 2013; Kalthoff et al., 2018). These clouds play an important role in the energy budget and diurnal cycle during summertime and tend to be badly represented in many climate models (Hannak et al., 2017). The role of these clouds in the WAM system was assessed here for the first time in a fully nonlinear way via sensitivity experiments using the ICON model from the DWD in NWP mode for July 2006.

Supplemental Material, Page 1, Line 4: Would read better as: “To investigate this we use EXPL experiments, in which f_op = 0.1 is applied for the first 4 days . . .” The phrase “as in EXPL” makes it unclear what type of experiment is being done here.

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