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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.

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Summary:

This study investigates the influence of low clouds on the West African monsoon system by performing various sensitivity experiments with the ICON model. The authors show that a decrease in low cloud optical thickness leads to a strong local increase in rainfall through associated changes in the diurnal cycle of convection. By contrasting simulations with parameterized and explicit convection, they make out important differences in the sensitivity of convection to the applied low cloud modification.

Recommendation: Minor revisions

Assessment:

This is a nice paper that fits well into the scope of ACP and its DACCIWA special issue. The manuscript is well structured and presents novel findings that support the notion that the misrepresentation of low-level cloudiness in climate models with parameterized convection likely leads to biases in precipitation and the thermodynamic structure in Southern West Africa. I list some general comments below, as well as a number of detailed comments and typographic suggestions that I would like the authors to address before the manuscript is published.

General comments:

1. Distinction of parameterization influence and resolution influence:

At the end of Section 2.2.2, I was missing a discussion of the influence of the changing resolution between the PARAM and EXPL experiments. Only on p.13, L6 you mention that "differences between PARAM and EXPL in Fig. 4 illustrate the sensitivity of the response to horizontal resolution and the use of convective parameterization", but everywhere else you neglect the potential influence of the changing resolution on the results. Marsham et al. 2013 isolated the influence of the convective parameterization by comparing experiments with 12kmPARAM, 12kmEXP and 4kmEXP. I'd suggest you refer to their study noting that the most important differences between the experiments are due to the convective parameterization, and that the increasing resolution between the experiments with explicit convection merely leads to quantitative differences. It's of course a bit trickier than that, but I think you wouldn't need to go in much more detail.

2. Negative & positive cloud feedbacks:

On p. 19, L6&L34 I stumbled over the sentences referring to the negative and positive low-cloud feedbacks. The way it is written, one thinks that you actually enforced a reduction in low cloud, rather than just a change in their opacity. By making the low clouds less opaque, you just manipulate their radiative effect, but e.g. not their effect

on the moisture budget or the microphysics. This should be made clearer.

3. Influence of organization of convection:

I would like to see some more discussion about the influence of changes in the organization of convection on the results. From Marsham et al. 2013 I take that mesoscale convective systems and the associated storm outflows are a significant component of the WAM system and I assume that they will also affect differences between your PARAM and EXPL simulations. On p.31, L5 you mention that you find effects of convective organization in your simulations. I understand that a detailed analysis of the role of convective organization would be beyond the scope of the manuscript, but maybe you can already appreciate some of the differences by looking at profiles of moisture variance and their diurnal cycle (similar to Figure 7). This might also be important for radiatively-driven secondary circulations that likely contribute to organizing the convection. I'd be surprised if changes in convective organization wouldn't be important in your experiments.

4. Use of commas:

I'm not a punctuation-expert, but I feel that there is a strong lack of commas throughout the manuscript. This distorts the flow and rhythm of reading. Examples are: p.3, L2 (season, low-level); p.4, L2 ((Sect 2.1), followed); p.6, L21 (set, ICON); p.7, L11 (given, concentrating); p.8, L8 (box, area-averaged); p.12, L9 ([...] Figure 4a), ranging); p.13, L32 (EXPL, but); p.16, L14 (Sect 3.2, contain); p. 25, L26 (maximum, changes); p.28, L3 (hPa, differences).

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More detailed comments:

p.1, L20-23: You should be a bit more specific here. Interactions of the WAM with the land surface? Representation of the hydrological cycle in West Africa?

p.2, L13: I don't understand what you mean with low-level processes. Do you mean

boundary-layer processes or land-atmosphere interactions? Or do you already refer to the local factors and surface characteristics that are the topic of the next paragraph?

p.3, L4: What does "this phenomenon" refer to here? The low-level stratus or the NLLJ?

p.3, L7: I would remove the details of the radiative transfer model (âĂŽusing the two-stream radiative transfer model SOCRATES (Suite Of Community RAdiative Transfer codes based on Edwards and Slingo; Edwards and Slingo, 1996)')

p.3, L13: What do you mean with "but feedbacks were not considered explicitly"? Where they not represented, or not analysed? Please clarify.

p.6, L13: Maybe again refer to Figure 1 here.

p.7, L2: You haven't explicitly mentioned the control experiment yet. Maybe add a sentence on p.6, L19, saying that "f_op=1 corresponds to the control experiment."

p.8, Figure 2: I would suggest a few changes in this figure. I'd recommend using a white background for the maps and a different colour scale (e.g. the âĂŽYlGnBu' palette from https://betterfigures.org/2015/06/23/picking-a-colour-scale-for-scientific-graphics/). Furthermore, the DACCIWA box could be shown in every panel.

p.9, L33: I don't really agree with the conclusion that ICON PARAM looks more consistent with the observations and ICON EXPL less. Together with CERES, ICON EXPL has a very good agreement with the few surface observations. This is in clear contrast to ICON PARAM, which tends to overestimate SSI compared to the surface observations. Maybe you can provide a more balanced conclusion of this paragraph.

p.9, L27-L29: Please clarify the sentence "brighter than the surface (except for snow) but in this region is likely still contaminated by clouds."

p.12, L32: Depth of cloud modification layer: I thought you were modifying clouds

below 700 hPa rather than below 750 hPa (see p.6, L13).

p.14, Figure 5: This figure has a relatively wild mix of colours and line types. Where applicable, I'd suggest to use more consistent colours throughout the paper, e.g. greenish colours for PARAM and reddish for EXPL (as in Figure 6). Further, I'd restrict the use of dashed lines in Figure 5 to the simulations with f_op=0.1.

p.16, L24-26: I don't really understand what you want to say here. The convective parameterization is by design responsible for vertical moisture transport. But also explicit convection transports moisture in the vertical. So I don't understand how this would explain the lower sensitivity. Do you want to say that "the convective parameterization more efficiently transports moisture [...] compared to explicit convection."?

p. 19, L18 onwards: I don't know exactly how TKE is treated in the parameterization, but as you say that the "mixing through convection is not reflected in TKE fields in PARAM", it's not surprising that the TKE profiles are very different. For me, the most striking difference between Figure 7 & 8 instead lies in the qc profiles. I would suggest some restructuring of this paragraph. It was also not always clear to me whether you are comparing PARAM and EXPL or the response to the opacity change for PARAM. This should be clarified.

p.21, L3: You didn't state the sign of the modification of low clouds, but then say that it leads to substantial increases in precipitation. I'd suggest to reformulate the sentence as follows: "[..] how moderate reductions in low-cloud opacity [...]".

p.22, L7: I wouldn't use the word "impressive" here, especially as you stress in other parts of the manuscript that a quantitative interpretation of the results is questionable. Maybe just use "an increase of 560%". The same is true for p.30, L13 ("an impressive factor of 5!"). I also don't really like the use of the word 'enormous' (e.g. p.25, L24; or p.31, L26), but that might be a matter of taste.

p.22, L31: I don't see how the sentence "This may explain the general tendency..." fits

in the discussion of the EXPL simulation here, as I assume that this might be different between explicit and parameterized convection.

p.27, Figure 13: Change legend in panel (e) to f_op=1.0 & f_op=0.1.

p.28, L 13-14: I don't understand what you mean with "effectively removing tropospheric surplus and depositing...", maybe something is missing here?

p.30, L21: I would assume that air advected from the ocean is moist, not dry. Am I missing something here?

S1, p.1, L27-29 and Figure S1: maybe add a measure of spread between the different runs to indicate the variability.

Typographic suggestions:

p.2, L7: and ITD shift –> and the ITD shift

p.2, L14: Eltahier –> Eltahir

p.3, L4: Omit either realistically or correctly.

p.5, L23: allows –> allow

p.5, L24: terrain following –> terrain-following

p.6, L21: remove grid ("a grid spacing of 13.2 km grid...")

p.7, L3: first (Sect. 3.1) –> first section (Sect. 3.1)

p.7, L17-L18: add a "the" in front of "adjacent ... highlands"

p.9, L1: "by on the order of" –> by about

p.12, L18: from –> of

p.13, L21: following –> followed

p.14, L4: clod –> cloud

p.16, L5: by on the order –> by about

p.16, L8: with values –> with absolute increases

p.16, L18: remove "than“

p.16, L18: results –> result

p.19, L18: hardly any change at all above –> hardly any change above

p.22, L26: with values –> with decreases

p.25, L32: aerosol-radiation or –cloud interaction –> aerosol-radiation or aerosol-cloud interaction

p.28, L21: impacts on higher and –> impacts on higher levels and

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