Reply to referee #2, Raphaela Vogel

Dear Raphaela,

We are grateful to you for the positive assessment of our study, for insightful remarks and for the valuable recommendations. We appreciate all the comments; we took them into account while preparing the revised version of the manuscript.

Below, the original comments are given in blue color.
The text added to the revised version of the manuscript is marked by red color.

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.

This is a helpful suggestion. We were aware of this study, and the fact, that the differences found in this study were mainly quantitative for varying horizontal resolutions. The same became apparent for ICON in a small test case, which was not included in the paper. Therefore, we did not study it in more detail, but should have mentioned this in the article. We added the following sentences at the end of section 2.2.2:

The same study differentiated between the effect of parametrization and the effect of horizontal resolution by comparing experiments with 12 km grid-spacing and both parametrized and explicit convection as well as explicit convection at 4 km. It was found that the dominating factor is the convective parametrization, which substantially alters the dynamics of the monsoon system, while the influence of the horizontal grid-spacing is mainly of quantitative nature. Building on these results, we will concentrate on differences between parametrized and explicit convection and pay less attention to resolution effects.

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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.

We tried to explain this better by adding the following lines:

This denotes a negative feedback mechanism, as a (here enforced) reduction of low cloud opacity leads to more cloud production, at least in the early part of the day. Recall that the modification was only applied to the cloud optical thickness, as seen by the radiation scheme.

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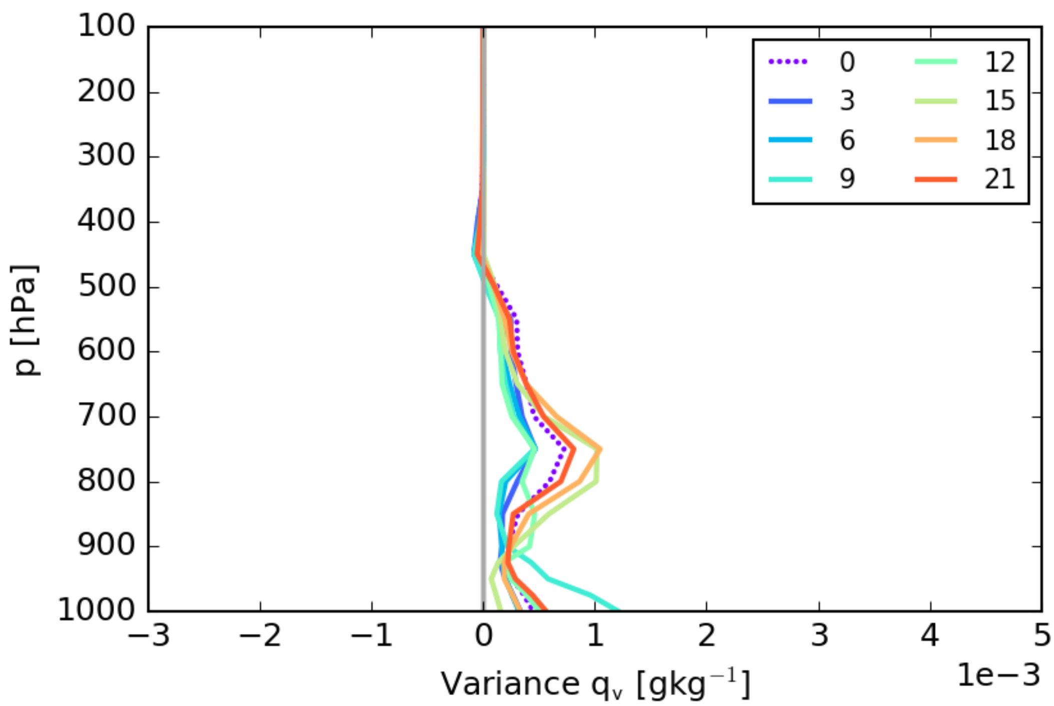
3. Influence of organization of convection:
I would like to see some more discussion about the influence of changes in the organization of convection on the results. From Marsham et al. 2013 I take that mesoscale convective systems and the associated storm outflows are a significant component of the 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.



More organised convection would quite likely lead to more extreme values in q_v (i.e. drier in some regions and enhanced concentration of moisture in others), therefore the q_v variance in EXPL should be greater than in PARAM. The figure above displays the average differences in variance (EXPL – PARAM). What one can see here is an always positive difference with some diurnal cycle. This confirms the assumption of higher organisation. But this short analysis is too indirect in our eyes to be included in the article. Therefore we added on p14l24:

The latter is reflected in a larger variance of q_v throughout the lower and mid-troposphere in EXPL than in PARAM (not shown).

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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).

We tried our best to improve the punctuation throughout the paper, including your specific suggestions.

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?

The first statement is explained by the following two sentences, which enumerate some of the difficulties in modeling of the WAM system. We modified the first sentences to the following:

Modelling the West African monsoon (WAM) system is a challenge, as reflected for example in large disagreement in rainfall, surface air temperature and cloud cover between models participating in the Coupled Model Intercomparison Project phase 5 (CMIP5) (Roehrig et al., 2013). Climate and weather models show a considerable inter-model spread when studying for example the influence of sea surface temperatures (SSTs) on the WAM circulation (Xue et al., 2010; 2016; Rodriguez-Fonseca at al., 2015), interactions of the WAM with the land surface (Boone et al., 2009) or the representation of the hydrological cycle in West Africa (Meynadier et al., 2010; Poan et al., 2016).

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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?

We added:
Several studies stress the importance of low-level processes, such as near-surface moisture advection or turbulent fluxes for the development of the WAM (Perill´e et al., 2016; Eltahir and Gong, 1996).

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

The evolution of the low-level stratus, which is connected to the low-level jet. We added:

Climate models struggle to realistically represent the diurnal cycle of the stratus in terms of cloud amount and occurrence as well as wind speed (Knippertz et al., 2011; Hannak et al., 2017).

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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)')

Since the results can depend on the radiative transfer method that was used, we would like to keep this information. A delta-two-stream method could not account for radiative effects of cloud edges, for example.

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

The sentence was modified to read "analysed" instead of considered.

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

Done.

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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."

Thank you! We added on p6,l29:

f_op is varied from 0.1 to 10, where f_op=1 corresponds to the control experiment.

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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.

We modified the figure as suggested.

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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.

You are right but ICON PARAM agrees better with the observations in terms of area-averaged solar surface irradiance, as explained in the respective paragraph:
"ICON EXPL shows the lowest SSI values with an area average of 164.7 W m^{-2} (Fig.\ \ref{solclim}a), much lower than PARAM with 191.6 W m^{-2} (Fig.\ \ref{solclim}b).
….
Evaluating this with observations is a challenge due to the many assumptions made in satellite-derived SSI and the few surface observations.
…..
In contrast, CERES does not seem to suffer from this problem due to a

different retrieval strategy (Fig.\ \ref{solclim}d). The box-averaged SSI is 188.4 Wm^{-2} and therefore very close to the ICON PARAM value, although with much less fine structure."

Therefore we change only the last sentence to clarify this:

> Overall this analysis demonstrates a significant observational uncertainty and suggests an overestimation of clouds in ICON EXPL leading to low average SSI, while ICON PARAM fields are more consistent with observations in this regard.

> 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."

Due to the high reflection, cloudy pixels appear brighter than cloud-free pixels for a satellite, therefore the surface albedo can be determined from the lowest irradiance measurements. In the DACCIWA region, unfortunately, it is not easy to find a cloud-free irradiance in a specific pixel (usually one month of measurements is used to find the surface albedo, this has to be done for each pixel and each hour of day). We modified the explanation to make this clearer:

> As cloudy pixels appear brighter than cloud-free ones for SEVIRI, the surface albedo is estimated from the lowest irradiance measurement found per pixel in a given time period. In SWA, however, it is often difficult to find cloud-free scenes, leading to an overestimation of surface albedo (see also discussion of this problem in Hannak et al., 2017).

> 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).

Well spotted. We corrected this to 700 hPa instead of 750.

> 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.

We modified the Figure to be more clear and updated the figure caption accordingly.

> 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 parametrization more efficiently transports moisture [...] compared to explicit convection."?

The parametrization scheme transports more moisture, than the explicit convection does. We modified the sentence to read:

To first order, the convective parametrization appears to transport moisture more efficiently out of the low- and mid-levels to deposit it into the convection-fed cirrus layer, as compared to explicit convection.

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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.

We agree, and reviewer 1 commented on the same paragraph that was admittedly not easy to read. We restructured it as such (this time in green):

Figure 8 shows the corresponding profiles for PARAM. Despite the overall consistent signal in rainfall and radiation as documented in Fig. 4, there are many substantial differences between the two sets of experiments.

Despite a larger SSI (see Fig. 4a), PARAM has a lower daytime increase in near-surface temperature, particularly at 15 and 18 UTC, suggesting a possible impact of the earlier triggering of convection in PARAM (see Fig. 5). Near surface q_v (Fig. 8b) is strongly decreased at 09 UTC, probably due to the earlier onset of PBL mixing with transparent clouds, and then strongly increased at 12 and 15 UTC, possibly due to the lack of deep mixing as in EXPL, leading to very large differences between the two sets of experiments. Combined, the changes in temperature and moisture lead to overall less pronounced changes in RH at low levels (both negative near the surface and positive above; Fig. 8c), associated with mostly negative changes in q_c (Fig. 8e) except for 09 UTC. These explain the somewhat unexpected results for q_c discussed in the context of Figs. 7 and 6. In contrast to EXPL, PARAM operates a positive feedback mechanism, where a reduction in low cloud leads to a further reduction. This may clarify, why so many climate models show very large negative biases in cloud cover (Hannak et al., 2017).

Increased vertical mixing can be observed via TKE (Fig. 8d). Positive signals are restricted to the low levels during the day (09, 12 and 15 UTC), with the latter time showing indications for increased mixing reaching midlevels. All hours from 18 UTC to 09 UTC show decreased TKE below 600 hPa and hardly any change at all above that. One needs to bear in mind, however, that the mixing through convection is not reflected in TKE fields in PARAM. Nevertheless, the PARAM signals, at least at low levels, are in clear contrast to EXPL (Fig. 7d) where TKE increases everywhere. These differences are strong indicators that the interplay between PBL turbulence, shallow and deep convection fundamentally differs between the two model configurations. Particularly during nighttime, PARAM shows a slight stabilization in the temperature profile (Fig. 8a) above 925 hPa that appears to suppress turbulence generation in this layer. This cooling may be related to the enhanced NLLJ (Fig. 8f), but it is not clear why this effect does not work in EXPL, where an even more enhanced NLLJ and also a stabilization is observed (Figs. 7a and f). The change in mixing have profound impacts on many low-level fields, whereas more agreement between EXPL and PARAM is found at mid- and upper-levels, except for some changes in the diurnal cycle.

Overall this discussion demonstrates the enormous importance of vertical transport and mixing in a moist tropical environment where the PBL, low clouds and deep convection are closely coupled through radiative effects.

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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 [...]".

Done as suggested.

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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.

We deleted the adjective impressive in "in the northern half of the DACCIWA box corresponds to an impressive 560%, while the southern half... " but kept it in the sentence "Particularly in the northern half of the modification region, rainfall increases by an impressive factor of 5!" because it should stress the strong effect that this small modification causes. We replaced the first enormous by a more neutral "large" but kept the second "enormous" because we think it to be appropriate in the given context.

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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.

In climate models, we found for this region in Hannak et al. (2017) that a too strong developed nocturnal low level jet is connected to a too low cloud cover. The discussion in the respective paragraph in our paper may give hints on the processes and explain why this happens, which could be used as an explanation for climate models, too. But you are right, since the convection is parametrized, other factors can be involved, too. Therefore, we modified the paragraph as such:

Assuming a similar behaviour also in models with parametrized convection, these processes may explain, why an underestimation of low clouds is often found together with an overestimation of NLLJ for many climate models (Knippertz et al., 2011; Hannak et al., 2017), but this needs further study.

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

Thank you for spotting this!

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

We added “surplus moisture”.

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

Due to the cold temperatures and subsidence over the ocean at this time of the year, the advected air from the ocean is in fact drier than air that has already resided over land (Schuster et al., 2013)! We added “dry subsided air from the ocean” to make clearer how this is possible.

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

Done.

Typographic suggestions:

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

Done.

p.2, L14: Eltahier –> Eltahir

Done.

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

Done.

p.5, L23: allows –> allow

Corrected.

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

Done.

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

Done.

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

Done.

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

Corrected.

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

Corrected.

p.12, L18: from –> of

Done.

p.13, L21: following –> followed

Corrected.

p.14, L4: clod –> cloud

Done.

p.16, L5: by on the order –> by about

Done.

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

Corrected.

p.16, L18: remove "than“

Done.

p.16, L18: results –> result

Corrected.

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

Done.

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

Done.

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

Corrected.

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

Done.