Response for: "Evaluation of Asian summer precipitation in different configurations of a high-resolution GCM at a range of decision-relevant spatial scales"

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Response from authors

Many thanks to both reviewers for your anonymous reviews. We have closely followed the modifications we set out in our original responses to you. We feel that this has improved the overall structure of the paper and hope that we have addressed the points you raised. Our responses below are in blue. We have reordered the figures in the Supplement so that they appear in the same order as they are referenced in the main paper.

Reviewer 1

(a) Muetzelfeldt et al's manuscript evaluates high-resolution (grid-spacing ca. 14 km) GCM simulations with parameterized convection, with only shallow and mid-level convection parameterized, and explicit convection (i.e. without any convection parameterization) over a South and East Asian area in the months June, July, August. The results are interesting as the challenge of simulating the diurnal cycle of precipitation and its characteristics with parameterized convection is well known. Therefore, the models are pushed to higher and higher resolutions into the convection-permitting regime where deep-convection parameterization can safely, and also shallow and mid-level convection are often switched off. The convection-permitting regime is usually assumed to be at grid-spacing below about 4 km. But, as the authors discuss, this has been relaxed recently (e.g. as in Vergara-Temprado et al. 2020). It might be interesting for the potential reader to note that already Bougeault and Geleyn (1989) did experiments with and without deep convection paramaterization using grid spacing of 10 km in the, as they call it, resolvable domain.

> We have added relevant links to Bougeault and Geleyn (1989) in Sects. 1 and 3.3. Many thanks for bringing this paper to our attention.

(b) What I missed are figures for the entire investigation domain showing frequency and intensity for all three variants (explicit, hybrid, and parameterized). As Figs. S3 and S4 show for south-eastern China in comparison with Figs. 9 and 10, the hybrid did the diurnal cycles as good as the explicit, and the frequencies and intensities look comparably better. This is what I expected, given the applied grid-spacing very far from resolving shallow convection. Thus, I am not sure that the explicit variant performs better overall than the hybrid one, as the authors imply.

> We have included N1280-HC in the main figures in the paper (Figs. 4, 6 and 7), and made minor updates to the text to remove references to the Supplement. Correspondingly, we have removed some figures from the Supplement as they are no longer needed. Additionally, we have included CMORPH and all three simulations in the Supplement for the detailed study of precipitation amount, frequency and intensity and their diurnal cycles over China (Figs. S4 and S5 in the updated Supplement). The reason for keeping the three simulations in Figs. 9 and 10 as they are (and not replacing them with Figs. S4 and S5) stands: we wish to make the comparison with Li et al. (2018) Figs. 2 and 3 as easy as possible.

(c) Sec. 2.3: Given the definition, the amount A is a rate. This wording is a bit confusing given the usually different units of precipitation amount and precipitation intensity. It is also a bit confusing that some of the figs. show precipitation and some show amount, which is almost the same, but the reader might have to think twice.

> We have updated the wording in caption of Fig. 3 (the one place where the mean precipitation, and not amount of precipitation, was used) accordingly.

(d) Why the special section 4 on precipitation over China and no special section for the other areas?

> As laid out in our original response, the reason that we focused on China was to facilitate comparisons with Li et al. (2018). We also had opportunities for discussion and collaboration with Chinese colleagues due to the nature of the funding of this work.

(e) The discussion Sec. 5 misses a discussion on the simulation variants and their representation of the different processes and process scales. This discussion might help the reader to understand better the shown results.

> We have added two paragraphs to the discussion on this point. We speculate as to why the frequency and intensity of precipitation are different for the parametrized and explicit convection simulations, and note that this could be followed up with future process-based analysis. This point prompted further thought on our side which led to the second paragraph, in which we suggested some further experiments which may help to a) better assess the differences between fully parametrized and fully explicit simulations, and b) shed some light on how to make parametrized convection scale-aware.

(f) The conclusions lack a conclusion on how to proceed in very high-resolution GCMs. What was learnt? Shall GCMs at O(10km) run without any convection parameterization? Should consistent PBL, shallow convection models be further developed?

> First, we have renamed the final section "Summary and conclusions" in light of reviewer 2's comments. Second, we have added a final "Conclusions" subsection and raised the points you mentioned and our original response to them. Some further thought on this matter has lead to this being slightly expanded in its scope compared with our original response.

Fig S3 refers to Fig. 9 not 10.

Line 440: of convection -> of precipitation?

Line 444: "Fig. ??"

Figure 7: Amount of precipitation -> The diurnal cycle of .... ?

> We have fixed these minor issues.

Reviewer 2

(a) The manuscript structure at present is very difficult to follow. I had to go back and forth with the figures to understand what especially authors has to say. This comment applies to both the main manuscript and supplementary figures.

> We have attempted to minimize the extra material in the Supplement by including more of the figures directly in the main paper, which we hope makes the paper more accessible. Additionally, we have reordered the figures so that they appear in the same order as they are referenced in the main paper. In response to your second comment (b) we have renamed and modified the final section (see below). The rest of the paper is, in our opinion, rationally structured. The methods includes the data sources, and then works through analysis methods from the simplest to the most complex. Section 3 includes the grid-point followed by catchment basin analysis for precipitation and its diurnal cycle respectively. We accept that there are other ways this could be structured but think that the current way is sufficiently logical. Section 4 has our detailed regional investigation of precipitation over China, again looking at precipitation, then its diurnal cycle.

(b) The authors should substantially change the conclusion part. They are just repeating the already mentioned aspects from the results section. I did not get what the authors have to conclude from the work.

On review of your comment, we take the point that a lot of what we wrote was a summary, and correspondingly we have renamed this section "Summary and conclusions". We have left the summary as it was, and added a new subsection "Conclusions" with some more general thoughts on how the results should be interpreted and what can be done following this work.

(c) The usual challenges faced by the policymakers are the information at the fine spatial resolution. The analysis conducted by the authors seems that there is significant bias – specifically over the indo-china peninsula, at a very fine resolution for all the configurations. As the author mentions in their paper that it is computationally difficult to perform such analysis, researchers, therefore, tend to use some of the other techniques such as statistical downscaling (See Kulkarni et al., 2014 for example). There should be some discussion about this in the revised manuscript.

We have added a short discussion of statistical downscaling (final paragraph of Sect. 5), noting its strengths and weaknesses.

References

Bougeault, Ph and JF Geleyn (1989). "Some problems of closure assumption and scale dependency in the parameterization of moist deep convection for numerical weather prediction". In: *Meteorology and Atmospheric Physics* 40.1, pp. 123–135.

Li, Puxi et al. (2018). "The diurnal cycle of East Asian summer monsoon precipitation simulated by the Met Office Unified Model at convection-permitting scales". In: *Climate Dynamics*, pp. 1–21. DOI: 10.1007/s00382-018-4368-z.