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Geoscientific Model Development Discussions

Interactive comment on "CAM6 simulation of mean and extreme precipitation over Asia: Sensitivity to upgraded physical parameterizations and higher horizontal resolution" *by* Lei Lin et al.

Anonymous Referee #2

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GENERAL COMMENTS

This is an interesting paper evaluating the performance of the CAM6 prototype in the Asian region at two horizontal resolutions and comparing against CAM5. The paper is reasonably well-written and the figures are clear. I have some concerns regarding the use of reanalyses data as a second benchmark, and it would helpful to clarify how some of the resolution comparisons have been made.

Finally, it would be helpful if the manuscript could be checked carefully by a native

English speaker to remove numerous errors.

SPECIFIC COMMENTS

page 2, line 25: Actually, GA6 is not the latest atmosphere model from the UK Met Office. Williams et al. (2017; https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017MS001115) state that the GC3 coupled configuration, which has GA7 as its basis, will form the basis for their CMIP6 submissions. There are substantial changes between GA6 and GA7. You can still quote the studies relating to GA6, but perhaps reword this paragraph.

page 3, line 1: Enhanced model resolution has not always been demonstrated as a means to reduced model biases, especially not in the tropics. For example, Johnson et al (2016; https://link.springer.com/article/10.1007%2Fs00382-015-2614-1) showed that increasing horizontal resolution was not a solution to the South Asian monsoon biases in the Met Office GA3 model and also stated, based on past studies, that "it is difficult to attribute the monsoon improvement to any particular physics or resolution change in the atmosphere or ocean components."

page 4, line 2: "time-varying observed sea surface temperatures and sea ice" - what is the time resolution of your forcing dataset? Is it monthly interpolated or actual daily mean values?

Section 2.2: I am a little concerned that there is really only one observational rainfall dataset employed here. Rainfall from reanalyses is very dependent on the model physics, and is therefore not really a suitable benchmark. I realise that you may be constrained by the time period and horizontal resolution of your simulations, but it would be good to include more caveats on the APHRODITE data, particularly the potential lack of gauge observations in mountainous areas: how reliable are the values over Tibet, the Himalayas and the Maritime Continent islands? While I would not expect a detailed comparison between observational datasets in your study, some additional discussion on this aspect is warranted, instead of simply says in lines 18-20 of page 4 that you

use the reanalyses as a benchmark, thereby implying that this is suitable.

Similarly, there are other global datasets for surface air temperature from in situ measurements (such as CPC: https://www.esrl.noaa.gov/psd/data/gridded/data.cpc.globaltemp.html) that could be used as a second benchmark.

Figures 1 and 2: Please confirm in the caption that you have interpolated the observations/reanalyses to the model's 1 degree grid, and the APHRODITE data to the JRA-55's 0.56 degree resolution, for panels (a) to (c) of these figures?

page 5, line 17: "...to fully capture the larger uncertainty of observational datasets" - see previous comment regarding reanalyses. I suggest "...as an estimate of the large uncertainty..." would be more appropriate.

page 6, lines 13-14 and 23-24: An increase in horizontal resolution between 1 degree and 0.25 degrees is grossly insufficient to avoid the use of parameterization of clouds and convection by resolving those processes. Even in a prognostic scheme such as CLUBB, the microphysical processes are still parameterized: a prognostic increment to the condensate and rainfall is calculated by making assumptions about (i.e. parameterizing) the relationship between the thermodynamic variables and the microphysical process. Increasing resolution could change the local circulations, the thermodynamic variables and their distribution, and may make them more realistic if the processes driving those are resolved better, but this is not actually resolving the precipitation process itself better. Ultimately, as you note in line 17, the partition between the large-scale and convective rainfall in any model (including reanalyses), and how this changes with resolution, will depend on the parameterization schemes employed.

There are also several statements in this section that imply that convection parameterizations are only there to mop up instability. I do not think that this is true. They should be representing the effects of sub-gridscale convective processes (as opposed to sub-gridscale stratiform cloud processes, which are handled by the large-scale cloud and

precipitation parameterization) on the environment in the gridbox.

Please consider rewording these misleading sentences.

page 6, lines 16-21 and Figure 4: Despite your caveat that JRA-55 is providing a partition that is model-dependent, its inclusion in Figure 4 implies that you are considering it as a benchmark for the model comparison. I recommend that you remove this and only consider the comparison between CAM versions.

Instead, you could include in Figure 4 some evidence that the change in timestep affects the partitioning in the way that you assert in lines 26-31 (by showing results from CAM6-1 with a 10 minute timestep, perhaps?).

page 7, lines 1-13, and Figure 5: Please confirm that you have compared all of these on the same 1 degree grid resolution? This is particularly important for the RX1day and R10 statistics that are measured using threshold values against the intensity distribution, which itself will depend on the resolution of the data (even for the observations).

page 8, line 4: "higher frequency fluctuations" - are these really higher frequency fluctuations? It is still an interannual variation, albeit of seasonal mean values.

Also, given the known influences of ENSO on boreal summer monsoon rainfall over Asia, it would be interesting to compare the regression of JJA mean rainfall against ENSO in Figure 7, as well as that of the annual mean rainfall.

Section 4.3 and Figures 11 and 12: Similar to my previous comment regarding Figure 5, please confirm that everything has been reduced to the same horizontal resolution before doing this analysis.

Further, you have suddenly taken the mean of the observations and reanalyses here, in what is perhaps the hardest test for the models. It would be advisable to state or show how these distributions vary between APHRODITE and the two reanalyses - is this really captured with the one standard deviation? There are only three datasets, so why not show the envelope as shading instead (and APHRODITE as the solid line)?

page 9 line 16: The CAM6-0.25 also looks worse for heavy rainfall (than CAM6-1) for the Sichuan and Tibet regions.

page 10, lines 3-5: "High-resolution simulations..." - where is your evidence for this statement? Even if it is true (of which I am not sure), there is also horizontal moisture advection which will be different at higher resolution.

page 10, line 6: Presumably the increased downward solar radiation in CAM6 is related to the improved diurnal cycle? Please state this, if it is the case.

Section 5: Overall, I do not find this section convincing, nor does it add much to the findings of the study. You make several statements about the impact of the new physics in CAM6 on the balance of processes and the large-scale/convective rainfall partition that are speculative and not supported by evidence. I would suggest that you remove this section and Figure 14 (and the associated bullet point (4) on page 11).

page 11 line 17: "...better performance over Sichuan basin..." - is this true? It looks worse than CAM6 at 1 deg in Figure 12.

TECHNICAL CORRECTIONS

[Note that, in addition to the points raised below, there are numerous wording and grammar issues that require careful editing by a native English speaker]

page 2 line 11: "regarding to" -> relating to

page 3, line 27: Please expand CLUBB, or at least mention which part of the model physics this relates to.

page 6, line 27: "...whenever large scale condensation that process removes all liquid..." - I do not understand this sentence.

page 8, line 16: "...edge of the positive correlation is less more northward..." - more, I think.

Figure 10 caption: This is not just within Southern China.

page 9, line 1: "which is missing the persistent in CAM5-1 (the jumping cliff over July to August..." ??

Table 2 - you have not referred to this table anywhere in the manuscript. It may not be needed if you remove section 5.

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