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Geoscientific Model Development Discussions

Interactive comment on "Significant Improvement of Cloud Representation in Global Climate Model MRI-ESM2" *by* Hideaki Kawai et al.

Anonymous Referee #2

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This study by Kawai et al. takes an excellent approach to documenting model development and fits very well into GMD. My only very major concern is the excessive use of empirical relationships that arise as a consequence of a large number of physical processes in the stratocumulus and also the phase partitioning parameterizations in MRI-ESM2. While the underlying physics remain the same in a future climate, there is absolutely no guarantee that these empirical relationships can be applied to climate change (as detailed below). In my opinion, this point clearly deserves some discussion. In particular, there are indications that the empirical relationship between LTS (or EIS) and cloud cover that explains day-to-day variations does not hold in a climate change context (see below). Although day-to-day variations will still be governed by LTS in a warmer climate, the mean stratocumulus cloud cover might change in ways that have

nothing to do with this relationship (just think of two parallel and shifted regression lines perhaps with similar slopes, one of which represents the relationship in the present climate and the other the relationship in the future climate). While on a day-to-day basis larger LTS is associated with higher stratocumulus cloud cover, many climate models suggest that in a warmer climate, LTS increases while cloud cover in the stratocumulus regions decreases. Here, instead of the processes that are responsible for creating the day-to-day relationship, the actual present-day empirical relationship between EIS and cloud cover is baked into MRI-ESM2. This may cause a spurious negative contribution to the cloud feedback in future climate simulations, even if the relationship is only used to determine a threshold in the turbulence scheme. While using empirical relationships can serve to make results look good, using them is clearly not an ideal approach when constructing a climate model. Thus, in a climate change context, one should not only evaluate the performance of a model by comparing it to observations, but also take into account to what extent the parameterizations are based on simple physics and to what extent they are based on empirical relationships that arise from a combination of many processes. A climate model with imperfections in the representation of the current climate might in the end be preferable to a model that uses many empirical relationships to constrain the results. Using empirical relationships such as the relationship between LTS (or EIS) and cloud cover can lead to spurious climate feedbacks.

On the whole, however, I very much enjoyed reading this manuscript because of the systematic approach taken to evaluate the effect of changing the individual parameterizations. I think this can clearly serve as a good example for other groups. Probably most groups take an iterative approach to model development. In this process, often each step is evaluated separately, sometimes several changes are combined, and sometimes changes are reverted if they do not yield the desired effect. Ultimately, the successful groups certainly also form a fairly clear picture of the role of the individual changes. However, until recently a larger fraction of the resulting insights tended to be hidden in a few peoples heads. Kawai et al. use their new model version as a base model and then revert each development step, always starting from the same base model. To me this seems a very nice and perhaps also novel way to make the steps more transparent to others. I think this is very useful. Rather than with just tuning (as in Mauritsen et al.), this manuscript deals with actual model development (although I do see some caveats as outlined above).

Perhaps, in a follow-up study the authors could take a similarly systematic approach to study the effect of each change on climate sensitivity and the aerosol.

Specific Comments:

1. The partitioning of detrained cloud water into liquid and ice in MRI-ESM2 follows Hu et al. (2010). The same data by Hu et al. (2010) was also used to evaluate MRI-ESM2. It is found that the mass and the frequency ratio in Fig. 4 are both fairly close to the Hu et al. data. Thus overall, the WBF process appears to have a rather small influence (which might be partially explained by still using a mixing ratio threshold). Could you please briefly comment on this? Is the WBF process allowed to act on detrained cloud water in the same time step?

2. Hu et al. (2010) present an empirical relationship that arises as a the consequence of a large number of physical processes. While the underlying physics remain the same, there is absolutely no guarantee that such a relationship will hold in a future climate. Updrafts and/or aerosol may influence such a relationship and they may change in a systematic way.

3. Many climate models show a decrease of cloud cover at the same time as LTS increases in a future climate. In a regional model, Lauer et. al 2010 (https://www.doi.org/10.1175/2010JCLI3666.1) also find that changes associated with global warming do not follow empirical relationships between LTS and cloud cover. Therefore, such empirical relationships should not be used in climate models.

4. I know that this would mean breaking a tradition, but I would be very interested in seeing a plot that shows the effect of the bug fixes mentioned in 3.7.

5. Do the the changes due to the individual model modifications add up linearly to yield the final result? In Fig. 11, a plot could be added that compares the sum of the individual contributions to the difference between MRI-ESM2 and CGCM3.

6. Fig. 10 shows a strongly decreased time step dependency of the ice water content in MRI-ESM2 compared to MRI-CGCM3 which the authors attribute to the I2S conversion term. This seems very plausible to me. However, if I understand it right, in MRI-ESM2, the partitioning of IWC into IWC>100 and IWC<100 is still performed at every time step (according to Eq. S10), right? As far as I can see, in the absence of a threshold below which conversion from cloud ice to snow starts to be active, this could in principle still cause a problem similar to the one described in lines line 423f. Nevertheless, at least the time step dependence seems to be addressed. Without first looking at Fig. 10, I would have expected that in the case of a long-lived non-precipitating cloud the ice water path would still become depleted during a successive time steps because of this partitioning. And, on the other hand, does partitioning every time step mean that a part of the IWP will always be be particles smaller than 100 micrometer (IWC<100)? Again, all of this seems less of a problem than what has already been addressed here.

p. 2, l. 46: ratio of supercooled water -> ratio of supercooled water to cloud (liquid and ice) water

p. 9, l. 281f: I completely agree on this (https://doi.org/10.5194/gmd-2018-307-RC1).

The advection term in Eqs. 2 and 6 is in flux form and not in advection form. In order to convert an advection equation to flux form, it is necessary to use the continuity equation. This implies that the flux form equation is valid in cases in which the tracer (here: ice) is advected with the flow. The flux from equation is not valid for sedimentation.

p. 13, l. 396f: Morrison and Gettelman, 2007 (https://www.doi.org/10.1175/2008JCLI2105.1) use substepping.

p. 15, l. 445f: this is not unavoidable. see my comment regarding l. 396f.

Fig. 4, caption: should the Hu et al. data be compared to the mass or the frequency weighted ratio?

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