

Response to Reviewers

We would like to thank the reviewers for taking the time to revise our manuscript and give feedback that has helped to improve the manuscript.
Please find below our point by point reply to your comments.

Response to Reviewer #1:

General comments

The authors have addressed adequately most of the concerns that I raised in my original review. There is one point I disagree on: adding the SAT-WP curves to Fig. 7 does not actually seem very meaningful (see the specific comment 6). My other comments are all very minor and mainly related to “fine-tuning” the presentation.

Specific comments

1. lines 62-65: While Cesana et al. (2019a) is a relevant paper, it would be even more important to discuss briefly Cesana et al. (2019b) here (i.e., the heating rate paper). This is what I suggested in my original review.

We have now changed the discussion to Cesana et al. (2019b)

2. lines 75–86: The flow of the text would be smoother, if you first described what EC-Earth v3 is, and how it is related to the CMIP5 versions EC-Earth v 2.3 — and only after that, start discussing the different versions and resolutions used for EC-Earth v3 in this study. A simple reorganization of the text would do the trick:
move the contents of your current 2nd paragraph (lines 81–86) right after the first sentence (“The atmospheric model. . .”). Then, in a new paragraph, continue with the text on lines 76–80.

The text is rearranged.

3. lines 125–129: I think it would be useful to say here explicitly that the use of data at roughly 13:30 local time emphasizes the relative role of SW heating rates as compared with LW heating rates. It is not self-evident that the readers will note this point otherwise.

We have added a sentence about this at the end of the paragraph.

“However, when using data from early afternoon (~13:30), when the incoming shortwave radiation is close to its peak, the contribution of shortwave radiation to the total heating is emphasised.”

4. Caption of Fig. 5: “averaged over the whole season” should probably be “averaged over all seasons” or “averaged over the annual cycle”.

“all seasons” are now used in the caption for Figure 5-7

5. Caption of Fig. 6, 3rd line: “read” should be “spread”?

Yes

6. lines 250–255: I don't think that the statement “the cloud water content from the models does not include the contribution from precipitating clouds” characterizes the situation accurately (literally, this would mean that whenever there is precipitation, the LWC and IWC diagnosed by the model would be zero — which is certainly not the case!). Rather, the modeled LWC and IWC always include cloud water and cloud ice (i.e., cloud droplets and ice crystals small enough to stay in the cloud), whether or not the clouds are precipitating, but presumably not rain and snow (i.e., water drops and ice particles large enough to fall out of the cloud due to the effects of gravity). Therefore, screening out entire columns of CloudSat/CALIPSO data when there is some precipitation does not provide a meaningful comparison with model data – it probably eliminates a large part of the condensed/frozen water that would be diagnosed as LWC and IWC by the model. Therefore, I recommend eliminating the “SAT-WP” curves from Fig. 7. Just mention in the text that a potential reason contributing to the underestimate by the model is that the modeled LWC and IWC do not include rain and snow.

Thanks for the suggestion. We agree. We have removed the SAT-WP from Figure 7 and replaced the paragraph (lines 250-255) with the sentence (at line 239-240):

“An explanation for the models underestimating liquid and ice water content is that the models do not include the contribution from rain and snow.”

7. Caption of Fig. 12, 1st line: Add “anomalies” after “Cloud water content and cloud fraction.”

“anomalies” is added.

8. lines 293, 294, 309 and 310: replace “models” with “model”. You discuss the results for only one model version in this section.

Absolutely, the “s” is removed.

Response to Reviewer #2:

I would like to thank the authors for substantially revising the manuscript and improving the overall structure. In the revised version, the authors address all the points mentioned during the first revision round of the manuscript and now the overall presentation and discussion of the results is improved in the revised version. Furthermore, the authors improved substantially the communicated messages of their study. As I mentioned in the first revision of the manuscript, this study is very useful and would be a valuable contribution to the increasing body of literature that deals with the coupling between the circulation and cloud-radiative heating. Thus, I recommend this paper for publication, and I briefly mention some minor comments for the authors below.

Minor comments:

Lines 55-68: Maybe worth mentioning also a similar recent study of Voigt et al., 2019 (<https://doi.org/10.1175/JCLI-D-18-0810.1>) where different climate models are compared with CloudSat/CALIPSO observations, as well as Zelinka et al., 2018 (<https://doi.org/10.1175/JCLI-D-18-0114.1>) where also multiple climate models are compared to CloudSat/CALIPSO retrievals?

Thanks for the suggesting these relevant references. We have cited them in the revised version.

Figures 2, 5, 9 could be slightly wider because in the current form they look smaller than the others.

The width of the figures is regulated by the GMDs latex style guide (not exceeding 12 cm, which they currently are).