

# ***Interactive comment on “Vertical cloud radiative heating in the tropics: Confronting the EC-Earth model with satellite observations” by Erik Johansson et al.***

## **Anonymous Referee #2**

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This study evaluates the performance of various versions of the EC-Earth model against satellite retrievals from CloudSat/Calipso focusing on the clouds and cloud radiative heating rates (CRH). The authors evaluate the intraseasonal and interannual variability of CRH in the tropics. This study is useful and would be a valuable contribution to the related and increasing body of literature that deals with the coupling between the circulation and CRH. However, the authors mostly illustrate a large amount of results without providing enough insights and deeper analysis of the results. Additionally, my impression was that the authors did not properly illustrate the gained knowledge from this study (potentially by showing many results without a clear story?) and did not communicate the main key points of this study to the reader. Another general remark could

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be the fact that the comparison among the different model versions was not giving any extra information to the paper and thus the authors should reconsider to focus on a specific model version and try to provide more deeper analysis and explanations for the differences between the EC-Earth model and the observations. As a final major remark, the readability of the vertical profiles was very bad and the authors should reconsider using more distinguishable colors.

Major comments:

1) There is a confusing part in this study that comes from the fact that the authors do not clearly explain the method that they use to compare the models with the observations. Is it monthly mean, 3-hourly data that the authors use to compare the simulated CRH with the observed ones?

2) Another general remark is that the authors in the results section mostly describe the figures without conceptually connecting the key points of the study and without providing more insights and explanations.

3) The authors illustrate the changes in CRH across seasons and different ENSO phases, however they do not illustrate the gained knowledge from this changes and do not properly communicate the key message from these comparisons in the context of previous studies.

4) The readability of the figures is really bad (particularly those that include vertical profiles), thus more distinguishable colors should be used.

Minor comments:

Line 3: Typo: coupling

Line 148: Typo: analysis

Line 159: It could be easier for the reader if the figures and the text had different longitude values to something like 45E, 90E, 90W, 45W.

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Figure 2 (e-h): To make the comparison easier since the differences are larger than 1K/day wouldn't it make more sense to use the same colorbar as in panels (a-d) so that the reader can compare the differences compared to the absolute values?

Line 179: Do the authors refer to the temperature tendency due to convective parameterizations or the CRH? It should be properly phrased here.

Line 180-181: Since the model overestimates the magnitude of the CRH over both convectively active and stratocumulus regions is there a reason to separate them by using "but"?

Lines 183-184: Any potential explanation for that?

Lines 184-186: This is confusing. Isn't it the case that panels (e-h) in Figure 2 show monthly mean differences between the model and the observations? See major comment 1

Lines 189-190: It is not obvious that the convection is strong. This is not shown anywhere. Moreover, the authors do not provide enough evidence when is the dry and the wet season for a location. Maybe show this somewhere?

Lines 187-199: Any further insights about these differences?

Lines 204-205: Isn't it the case that the magnitude in the lower troposphere (below 2 km) is smaller across the model versions, thus underestimating the magnitude of the cooling?

Lines 210-212: Any insights about these differences? Could you potentially explain why the models simulate the maximum heating higher than in the observations? Moreover, the different model versions seems not to be sensitive to CRH. Is there any explanation why these differences are so small?

Figure 6: It might be better to use letters to show which panels show IWC, LWC and CF.

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Lines 218-220: This is somehow confusing. Despite CF differences in the upper troposphere are small, the models seems to underestimate IWC compared to the observations and and still producing stronger CRH than the observations. Could you explain why is this the case?

Lines 214-215: This underestimation of CRH is mainly evident in the mid-troposphere. However, in the lower troposphere CRH agree more despite the fact that cloud properties differ substantially. Any explanation regarding that?

Lines 244-253: Could the authors provide more insights that would explain these differences?

Figure 7: As before, either adjust the colorbar to make the comparison more straightforward or mention in the figure caption the different colorbar for the differences. Additionally, in the figure caption it would be more appropriate to mention the satellite (CloudSat/Calipso) instead of mentioning CRH from satellite.

Lines 259-260: Could the authors providence more evidence why is this happening?

Line 262: Why the differences are mostly evident over the ENSO region? Is this consistent with previous modeling studies? What could be the reason behind these differences?

Line 274: Which figure shows that? Maybe add the figure at the end of the phrase?

Line 282: As before, which figure shows that?

Lines 295-298: Maybe use the same wording for anomalies? Using anomalies and changes in the same phrase might be confusing for the reader.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-277>, 2020.

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