Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-277-RC1, 2020

Geoscientific Model Development Discussions

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 #1

Received and published: 5 December 2020

General comments

This paper represents a comparison of cloud radiative heating rates and cloud vertical profiles in the tropics between the EC-Earth model and datasets derived from active satellite measurements. Differences between three configurations of EC-Earth (two model versions, one of them run at two resolutions) are also compared.

These kind of comparisons are still relatively rare, especially for the radiative heating rates, so in principle this study is a useful contribution. However, having read the paper carefully, I feel somewhat dissatisfied about it. The paper documents many results,

but with rather little of deeper-going analysis. Perhaps the paper would benefit from a sharper focus on a comparison of a single version of EC-Earth with the observations. The differences between the three configurations of EC-Earth are small compared to the model-vs-observation differences, and for the most part, not even statistically significant for these relatively short runs. At least I would restrict the ENSO-related analysis to a single model configuration. Another concern — and indeed a source of irritation — is that the readability of many of the figures is quite poor (see the technical comments). The English language is generally good, however.

Specific comments

1. line 12: I suppose this refers to the upper troposphere?

2. line 56: "poor spatial resolution". Do you mean "poor spatial coverage"?

3. lines 61–64: The introduction does not put the present paper properly in the context of previous research. In particular, the paper by Cesana et al. (2019) entitled "The vertical structure of radiative heating rates: a multimodel evaluation using A-train satellite observations" should be discussed here briefly (this paper is now mentioned only in the Conclusions). What is the novelty / additional value in the current paper compared to Cesana et al.? Analysis related to ENSO, perhaps?

3. lines 67–77: The most relevant point for the present paper is what is the difference between the PRIMAVERA version of EC-Earth and EC-Earth v. 3.3.1. Perhaps this is said on lines 76–77, but it should be formulated more clearly.

4. lines 81–82: "The vertical levels are not equally distributed throughout the atmosphere." This sentence is not necessary, since all atmospheric GCMs have non-uniform

vertical grids.

5. lines 115–117: “CloudSat and CALIPSO pass the equator at roughly 13:30 local time during daytime, so the model results are linearly interpolated from the two nearest output times to the fit the satellite overpass time”. Is it indeed so, that your results only represent local daytime (close to 13:30)? (The relatively large SW CRH values compared to LW CRH in Fig. 5 suggest to me that this might be the case.) This would have a major effect on the SW CRH, as the near-noon SW CRH is much larger than the diurnal-mean CRH, and also severely bias the net CRH. A procedure to mitigate this bias, that is, to calculate approximate diurnal-mean results, was introduced in your earlier work (Johansson et al. 2015, cited in the manuscript). Why not to use it in the present work? See also the discussion on p. 1575 in Cesana et al. (2019).

6. line 121: I assume that the clouds in the lowest 750 m are included in the computation of model heating rates. Please mention this explicitly.

7. Figures 1-2: These figures are difficult to interpret because the net CRH consists of SW and LW components, which may even partly oppose each other. I therefore recommend adding figures which show separately the SW, LW and net components of CRH. To avoid an excessive number of figure panels, it would be sufficient to show only the annual-mean results. (I am aware of Fig. 5, but since it shows mean values over the entire tropics, all regional features are lost).

8. A further suggestion would be to analyze the CRHs as a function of sea surface temperature, or mid-tropospheric vertical velocity, to distinguish between convective and non-convective regions (see also the analysis in Cesana et al. 2019). It is up to the authors to decide if (and how) they wish to pursue this suggestion.

9. line 168: “peaks in cloud fraction in August – October”. Does this sentence refer

to marine stratocumulus in general, or the southern hemisphere (Peruvian, Namibian) stratocumulus regions only?

10. line 199: “...despite the higher resolution in the latter”. This is actually not so surprising. I don’t see any obvious reason why increasing resolution would automatically lead to substantial changes, nor why these changes would show up as improvements in large-scale features when compared to observations (it mainly depends on your luck!). The situation might be different if the resolution was high enough to explicitly resolve deep convection, but even the finer resolution considered here (40 km) is far too coarse for that.

11. lines 211, 219: The overly positive LW CRH in the upper troposphere is a curious feature. It is suggested that this is due to underestimated ice water content, which leads to underestimated cloud top LW cooling. This does not however explain why the modelled LW CRH is positive (middle column of Fig. 5). One possibility is that due to underestimated cloud fraction in the midtroposphere, too much upwelling LW radiation from lower levels reaches the high clouds, leading to LW heating.

12. line 219: Regarding the large underestimate of IWC in EC-Earth compared to the satellite observations, I am wondering if precipitating ice is included in the IWC in the latter. In models it is generally not. There are also other potential reasons that could make the satellite profiles and the model profiles in Fig. 6 not to be fully compatible. For example, there is no satellite simulator in the model. Also, do the EC-Earth cloud fraction and cloud water fields include convective clouds?

13. In Figures 5 and 6, the results seem much the same for all seasons. Furthermore, in the text on lines 201–223, the seasonal differences are not discused at all. So why not simply show the annual-mean values? This would also allow combining Figs. 5 and 6 into one figure with six panels.

14. line 247: this should be “radiative heating and cooling rate anomalies”?

15. line 251: “a strong cooling (-0.75 $\mathrm{K\,day^{-1}}$) above 10 km during ENSOP”. I cannot find this large CRH anomaly over the Atlantic in Fig. 8, and at least it is not representative of the Atlantic region in general. Please also check that the other numerical values given in the text in Section 3.2.1 are consistent with Figs. 7 and 8.

16. lines 253–266 and Figs. 9 and 10. The comparison of ENSO-related cloud radiative heating anomalies between different versions of EC-Earth seems largely useless, owing to the low statistical significance of the results. The low significance itself is not surprising considering your small sample size. I recommend to eliminate this part of the manuscript.

17. line 267: The section title “Nino 3.4 region” is misleading, when you discuss separately Nino 3 and Nino 4 but not Nino 3.4.

18. caption of Fig. 13: This should be “Cloud water content and cloud fraction anomalies”.

19. line 292: “half of that” should be “twice that”?

20. line 304: It would be relevant to comment on how your findings compare with those seen in the multimodel study of Cesana et al. (2019).

21. line 317: Do you mean “two noticeable differences in net CRH”?

Technical and language corrections

1. Figures 2, 5, 6, 8, 11, 12, 13, and 14, and especially their labels, are painfully small to read. Please enlarge them. Too small figures are a sure method to make a reader (and a reviewer) *förbannad*.

2. The choice of colours in Figs. 5, 6, 11, 12, 13, and 14 is not good. It requires effort to distinguish SAT (a colder shade of purple) from E3PH (a warmer shade of purple/red). Please use clearly distinguishable colours.

3. line 284: Replace “excessive” with “extensive”. “Excessive” implies that there is too much cloudiness in the observations.

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