Response to Reviewer #1:

We thank the reviewer for the constructive criticism that led to improvements in the manuscript. Following the suggestions from the both reviewers, we have carried out a major revision. The discussions and readability are improved and the redundancies in figures are removed. Among other things, Figs. 4, 5, 6, 8, and 11 are revised, while Figs. 9 and 10 are removed to declutter the discussion. Please note that few of the reviewer comments may not be valid anymore due to the revision of either the underlying figure or the text.

Please find below our point by point reply to your 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 signif-icant 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.

We find it encouraging that the reviewer thinks that this is a useful contribution. The evaluation of radiative heating rates, although they are at the core of net radiation budget, is indeed rare and we would therefore like to highlight how well a typical climate model simulates them. We have revised the manuscript to include deeper analysis, to improve readability and the quality of figures.

In our analysis, we found that there are either no statistically significant differences in the heating rates between the high- and low-resolution EC-EARTH model versions or the biases are enhanced in the high resolution version. This is in fact one of the main points that we would like to highlight in the study that incorporating processes are more important than just improving the resolution. We do however understand how one could get a feeling of having a redundant information. Following the reviewer suggestion, we have presented the figures of ENSO-analysis only for one model version, while mentioning the results from the other version only in the text form in the revised manuscript.

Specific comments

1. line 12: I suppose this refers to the upper troposphere?
Yes, this is clarified in the revised manuscript.

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

Yes. “Coverage” is a better word in this context.

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?

We agree. We have now mentioned Cesana et al in the Introduction. The ENSO analysis presented here is entirely new and we also hope that our focus on the meridional differences is of interest to the scientific community. Furthermore, one of the model versions we compare is a part of the CMIP6 group and our new comparison is hopefully of interest to the climate modelling community. We also show the insensitivity of simulated heating rates to the model resolution, which is not shown in any study before.

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.

We have updated the paragraph to make it clearer what model versions we are using and the differences between them.

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.

It is true that for most people working with GCMs this fact is common knowledge. However, we still believe it can be important to point out this in this paper, since we have a strong focus on the vertical structure, to reduce the risk for misunderstandings.

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).

Yes, our results only represent local daytime (13:30). Cesna et al. used accumulated daily (and night separately) values from the models and therefore needed to normalise the satellite data in a similar way as we did in the Johansson et al. (2015). To compare daily mean values or instantaneous values (for a specific time) have its pros and cons and in this manuscript we decided to focus on the instantaneous values. Johansson et al. (2015) was based only on the observational data, but here the main aim is to evaluate a climate model. Given that the convective cloud regimes in the tropics have stronger diurnal cycles, we thought it would be better to do a comparison at the instantaneous level. This would then also highlight the differences arising from the out of phase diurnal cycles. This is clarified better in the revised manuscript.

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.

They are included in the computation, we only excluded them in our analysis. We have now clarified this.

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).

This is a good idea and we have added these components separately in the revised version.

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.

It is indeed an interesting idea and we thank the reviewer for pointing it out. We believe it deserves to be a separate exercise of its own. Given the limited time we have for revision, it would be difficult to fit it in the current context. If the Editor could grant us extra time to incorporate this analysis, we would be happy to do it.

9. line 168: “peaks in cloud fraction in August – October”. Does this sentence refer marine stratocumulus in general, or the southern hemisphere (Peruvian, Namibian) stratocumulus regions only?

This statement refers to those stratocumulus regimes in the southern hemisphere. The seasonal peak for marine stratocumulus in northern hemisphere is usually a little bit earlier, May - June. We have clarified the sentence in the revised manuscript.

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.

It is correct that the resolution is still too large to explicitly resolve deep convection. We were hoping that the high resolution version would be closer to the reality due to the improved subgrid variability in the surface parameters, especially the SSTs, which is the main driver of convection in the tropics. However, we found that, in some cases, the estimates of heating rates in the high resolution version are even further away from the satellite estimates and we thought it would be worth documenting this.

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.

This could very well be the case and is something worth mentioning in the discussion.

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?

Following the reviewer suggestion, we used different flags in the CloudSat data to filter precipitation. Fig. 6 is revised in order to show cloud water estimates with and without the precipitation. We see that, depending on whether we consider precipitation contaminated profiles or not, the models either underestimate or overestimate the cloud water, both in the liquid and ice phases. Cloud Fraction is already matched with the satellite overpass time, so it is a fair comparison. EC-Earth cloud fraction and cloud water fields analysed here 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.

We agree. The figures and the associated text are revised in the revised manuscript.

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

Yes, this is corrected in the revised manuscript.

15. line 251: “a strong cooling (-0.75 K day) 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.

We have checked the numerical values in the section to make sure that they are consistent with the Figures.

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.

We agree. Given the very small differences, we have used the results from only one model version and mentioned the results from the other version only in the text.

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.

True, we have changed the title in the revised version.

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

Yes, this it is corrected.

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

Corrected. Thanks for pointing it out.

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).

A brief note on such a comparison is added in the revised version.

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

We agree that it is clearer to specify the “… net CRH” and have therefore added this to the sentence.

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.

The last thing we would like is to make the reviewer förbannad. Hopefully, the figures in the improved manuscript are up to the reviewers standard.

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

Excessive is replaced with extensive in the new manuscript.