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Comment on gmd-2021-117

Robin Hogan (Referee)

Referee comment on "Concurrent calculation of radiative transfer in the atmospheric simulation in ECHAM-6.3.05p2" by Mohammad Reza Heidari et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-117-RC3, 2021

This paper presents a study of the impact of calling radiation concurrently with the other physical and dynamical components of a climate model. The idea was first proposed for weather forecasting by Mozdzynski and Morcrette (2014) in a technical memo, although with very limited evaluation. Here the authors go much further in their evaluation. Increasing the coupling timescale between clouds and radiation from 0-2 hours to 2-4 hours would surely degrade the skill of a weather forecast model, but the authors show that apparently there is little effect on the broad-brush metrics used here to evaluate present-day climate simulations.

My main criticism of the paper is simply that it is too long so some effort to shorten it should be made. In particular, 30 figures is far too many, especially for a paper based on a fairly simple idea, and the fact that most of the results plots show that concurrent radiation has little effect. Points 6 and 8 suggest how four figures could be removed from the introduction/method sections, and points 10, 12 and 14 make further suggestions for reducing the figures in the results section. If this can be addressed the paper will be suitable for publication.

SPECIFIC COMMENTS

1. Abstract: comment on the potential meteorological impact (even if neutral) due to radiation seeing an older atmospheric state. Currently there is no mention of the impact of the scheme on the scientific content of model simulations.
2. L17: Radiation is not always expensive, especially in the case of high resolution weather models: Hogan & Bozzo (JAMES, 2018) reported that 1-hourly radiation accounts for only 3.5% of the computational cost of the ECMWF model at full (9 km) resolution. This is probably largely due to running the radiation on a coarser grid, since the ratio of radiation timestep to model timestep is the same (2h/15min for ECHAM and 1h/7.5min for ECMWF).
3. L21: It is an overstatement to say that the shortwave and longwave are *widely*

separated; in fact there is around 12 W m-2 of solar energy at wavelengths longer than 4 microns, which is traditionally regarded as the longwave domain.

4. L35: There are more recent studies than this that might be of interest: on the impacts of coarse temporal sampling in the ECMWF model (e.g. Fig. 6 of Hogan & Bozzo 2018) and how to mitigate them (e.g. Hogan & Bozzo, JAMES 2015; Hogan & Hirahara, GRL 2015).

5. L46: Some mention must be made here of the potential down-side of radiation in parallel, which is that the fluxes and heating rates fed to the rest of the model will be "older" by around one radiation timestep than in the traditional approach of radiation in series. The impact on forecast skill was not really studied by Mozdzynski & Morcrette, but could be important. In the ECHAM context, the classical configuration involves radiation fields computed at a particular time being used in the rest of the model for the following 0-2 hours (with some corrections for surface temperature and sun position, but not for clouds). In the concurrent scheme, the radiation fields are not 0-2 hours but 2-4 hours old. The impact on model fields is something you address later in this paper, but it needs to be mentioned here in the introduction as an important consideration. One physical process that benefits from a tighter coupling in time with radiation is boundary-layer clouds, particularly stratocumulus: when they form they are maintained by longwave cooling at cloud top. This could have been one of the reasons why Hogan & Bozzo (2018, Fig. 6) found that calling radiation more frequently led to more skillful forecasts of near-surface temperature *and* low cloud cover.

6. Fig. 1 reproduces Fig. 1 of Giorgetta et al. (2013), except for the addition of a small radiation box - in the interests of shortening the paper it should be removed. Fig. 5 is a small change that doesn't really illustrate the concept of concurrent radiation - all you need is Figs. 2 and 6, which could be combined into a single figure with two panels.

7. I understand that the red line in Fig. 9 should be the ratio of the red and blue lines in Fig. 8, but it doesn't look like that in that it is always larger than 1.6, when Fig. 8 shows that concurrent radiation is sometimes slower than classical radiation. Is this because the X axis is different, i.e. in one it is the total number of MPI processes and in the other it is the number used for just one part of the model? Surely it should be the total number of MPI processes allocated in both instances, but perhaps I misunderstand something. This needs to be clarified, and a fair comparison shown.

8. Fig. 16: It is worth pointing out that there would be likely little to be gained in terms of model accuracy from running the radiation scheme in a climate model every 15 minutes. Is this figure needed, since the principle can be explained easily in the text?

9. In the evaluation of the concurrent radiation scheme (Figs. 17-30) for a particular variable, the bias is shown for the concurrent and classical model versions, and the reader is expected to try to pick out the differences by eye which is not really possible. Far more useful would be to show the bias for just one of these versions, and then the difference between concurrent and classical, plus, crucially, some stippling to show where the changes are statistically significant. A particular area of interest would be in the marine stratocumulus regions where radiation and cloud processes are coupled on quite a fast timescale. From what I can see in the figures shown, there appears to be no significant effect of concurrent radiation on any of these variables (except possibly in Fig. 22), but it would really help to show difference plots to be sure.

10. Figs. 19-21: I don't see the need to show the total cloud radiative effect in addition to the longwave and shortwave components, since the latter two fingerprint specific cloud errors in models, whereas the total is simply a messy mixture of the two. Therefore I suggest Fig. 19 is removed. The captions for Figs. 20 and 21 are misleading as they should say they are the bias in cloud radiative forcing rather than in fluxes.

11. Figs. 19-21 show surface cloud radiative forcing estimated from CERES. The longwave CRF from series is very uncertain at the surface, since there is an assumption about the location of cloud base, which is unknown for passive satellite sensors. It would be much better to compare to the top-of-atmosphere CRF values from CERES, which are much closer to what the satellite measures. Note that in the shortwave, the surface and top-of-atmosphere CRF values are very similar.

12. In the interests of reducing the number of figures, is there really a need to show both

the CR and LR models? While the difference between these two resolutions is interesting to some, it is not the topic of this paper and is a bit of a distraction from the effect of concurrent radiation. As far as I can tell, concurrent radiation has only a limited effect at either resolution, so isn't it enough to show just one resolution and then say that concurrent radiation doesn't affect the other one much either? Or possibly plots for one of the resolutions could be consigned to Supplementary Material?
13. Why do the plots for the LR model, which should be higher resolution than CR, all show Gibbs fringes? Is this a genuine feature of the model fields, or is it some kind of degradation in the extraction or plotting?
14. Figs. 26-30: Are all of these figures needed? I would have thought that in the interests of shortening the paper some could be omitted, especially if there is no significant effect of concurrent radiation, in which case the results could simply be stated in the text.