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Comment on gmd-2020-384

Anonymous Referee #1

Referee comment on "Vertical grid refinement for stratocumulus clouds in the radiation scheme of the global climate model ECHAM6.3-HAM2.3-P3" by Paolo Pelucchi et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-384-RC1, 2021

In this paper, two related approaches to improve biases in stratocumulus cloud cover in ECHAM-HAM are presented. They both rely on a vertical grid refinement step. This allows the determination of the inversion level and then a remapping step, where the two methods differ. The updated vertical profiles are used in the radiation calculation. This is an interesting study. The results are basically negative, but are presented well. Given the nature of the results, I do not think it is incumbent upon the authors nor reviewers to try to "fix" these schemes or to subject the schemes to much additional scrutiny in this paper. The text identifies some of the difficulties with the approach. In my estimation, these boil down to (1) adjusting the cloud cover just for the radiation calculation does not seem to be enough to push the model toward a more realistic climate (in terms of stratocumulus cover) and (2) there are probably too many concessions in only applying the vertical refinement to the gridscale clouds to make it worth the effort for minimal improvements to the climate. I think only minor revisions are need for this paper. There is an opportunity here to make the work more broadly applicable by (1) making some comparison to other global models that might use similar approaches and (2) possibly making more assertive statements about what are likely to be productive paths toward improved stratocumulus representations in global models.

On these two general points, I'll just add a couple thoughts. I noted in my specific comments below a connection two two other models. First is the NCAR CAM5 that used the UW moist turbulence and shallow convection schemes, which I think are based on Grenier and Bretherton. It would be interesting to know whether the results here could be related to results with that model (where the ambiguous layer must be used for determining turbulent mixing and possibly cloud cover). The other is the UCLA model that used a mixed layer model for their boundary layer scheme; that model isn't really relevant any more, but there are some shared assumptions with the scheme used here, so I wondered if there was any merit in making that connection? On the second point, I think the conclusions here could be expanded a little bit. In particular, I wonder whether any recommendations could be made beyond the possibility of also applying the refined grid to the microphysics. Below I note some skepticism about that path, as it would seem to just lead to wanting to apply the refined grid to the rest of the physics too. One could also ask whether there is value in trying to better match the physics and dynamics by better including the inversion reconstruction in the vertical advection? Finally, and maybe most simply, how much benefit would there be to just increasing the vertical resolution in the boundary layer versus trying to reconstruct the smaller-scale structure?

SPECIFIC COMMENTS

1. around line 105: the "invgrid" with the squished clouds seems like it would be problematic from the outset. Since the optical depth of the clouds is directly related to the LWP, when the cloud volume is conserved but made geometrically thinner but broader, the cloud fraction goes up, but the LWP would be reduced, correct? Seems like that could end up radiatively warming (via shortwave) the subcloud layer (a small effect) and the surface (potentially large effect in coupled settings, and would work against the goal of increasing cloud cover).

2. around line 130: The column detection method is fine, but is this the complete description? Is there an ocean mask or a latitude limiter applied? Otherwise, it seems like non-subtropical-stratocumulus would be selected often (at high latitudes, for example). Oh, I see that later in the results, the changes outside the subtropics are noted. Was there any attempt to more directly limit the application to stratocumulus?

3. Line 160: a small notational thing, I always reserve q for specific humidity while r is used for mass mixing ratio. It's a convention that is known, but not always followed. I just want to confirm that it is mass mixing ratio that is being used (mass_water / mass_dry_air) and not specific humidity (mass_water / (mass_dry_air + Sum_i(mass_water_i))).

4. Line 200: in climatologies stratocumulus will be thicker, but instantaneously, wouldn't we expect to often find much thinner layers?

5. Line 249: I don't think that "heat content H" is an appropriate thermodynamic description for Equation 13; isn't this more correctly called the enthalpy?

6. Sec. 2.2.3 (grid refinement): I'm interested to know whether regridding the aerosol fields was considered? I don't remember much about how ECHAM-HAM does aerosol, but I assume that there are a number of species that are advected and interact with the clouds and radiation. In some stratocumulus regimes, for example over the southeast Atlantic during biomass burning over central Africa, there are important aerosol direct (and possibly indirect) effects that influence the cloud/boundary layer structure. This seems like it could be problematic for this approach, since the aerosol will interact with clouds separate from the radiation, so it would not be obvious how to regrid the aerosol. If the aerosol is left homogeneously distributed in the grid cell, it could alter the radiative forcing

in the column.

7. Sec. 2.2.3 (grid refinement): The other question I had was whether rain or snow are radiatively active in the model. If so, it seems like they would need to be regridded as well -- for example, to avoid the situation where drizzle is falling into the stratocumulus that made it (!).

8. Another question about the scheme itself is what it looks like for stratocumulus that are multiple grid levels deep? I assume this occurs frequently (it does in other models with similar resolution). It would seem like this would impose a structure that would be more like cumulus rising into stratocumulus in some circumstances. Or maybe I missed a detail, and there is some adjustment to the lower cloud layer, too? Later in the paper this is shown a little bit (Figure 5), and is kind of addressed in the discussion of the difference between the VOLUME and SUND schemes, but not completely. In the AMIP runs, I would expect multi-level clouds to occur frequently, and I'm still not sure if anything is done for the lower cloud layer in the case when the inversion level pushes the cloud top down into a level (rather than popping it up to the next level as in Fig 5b and c).

9. L330 / Fig 3: The failure of the LCL diagnostic is interesting (although probably secondary to the main topic). It would informative to include in Figure 3 and indication of where "cloud base" is in the actual model. That is, mark the bottom of the level with non-negligible liquid water. This seems to be indicated in Fig 5, so maybe it isn't worth adding to Figure 3.

10. Also, a 6-day SCM run isn't very convincing in terms of the success of the inversion reconstruction. Were other cases like DYCOMS-II or ASTEX also investigated? At L334, the comparison with previous results is noted. It is hard to have much confidence in this improvement based on what is shown. Another option would be to re-run the EPIC case a bunch of times with perturbed initial conditions (as in Hack & Pedretti 2000) to get a better sense of the statistical properties of the inversion reconstruction.

11. I think Figure 4 should also include the radiosondes from EPIC that are mentioned previously. Looks like the data is available here:
https://atmos.washington.edu/~breth/EPIC/EPIC2001_Sc_ID/sc_integ_data_fr.htm

12. Around L465: This is a key conclusion of the paper, I think. If we think of this vertical regridding scheme as an attempt at some kind of "dynamic bias correction" to cloud cover, it doesn't really work. The initial cloud formation mechanisms are flawed, so a scheme that would just try to boost the cloud cover in the radiation is extremely limited in utility.

13. L502-4: I agree that one might expect better performance by also applying the

regridding to the the microphysics. That would be like an improved version of the SC-SUND scheme that would deal with phase partitioning and drop numbers better (and aerosol?). I would suggest that approach would also come up short, and that the argument then would be that the turbulent mixing isn't represented correctly because it doesn't know the correct "mixing height" because it is acting (probably, depends on the scheme) on full model layers. So if the microphysics and radiation were adjusted, the recommendation might be to extend the adjustment to the turbulence, too. At that point, the Grenier and Bretherton (and then Bretherton and Park / Park and Bretherton) schemes would seem like an attractive solution, harmonizing the shallow convection, gridscale cloud physics, and turbulence; the microphysics and radiation then get to come along for the ride, but would depend a bit on the implementation. I don't think that in NCAR-CAM5 (which uses the Bretherton/Parks schemes) the radiation has any information about the inversion height.

14. Another model that I thought about while reading this paper was the old UCLA GCM. The relevant idea there was that they used a well-mixed layer assumption to determine their lowest model level's height, which was synonymous with the "boundary layer". They did a relatively good job with stratocumulus because they had a level interface that was naturally at the inversion. (Now, where the mixed layer assumption didn't work well raised other important errors, but for stratocumulus it worked pretty well.) See Suarez et al. (1983) and Randall and Suarez (1984).

references

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