Response to second round of revisions for “Physics-dynamics coupling with element-based high-order Galerkin methods: quasi equal-area physics grid”

In order to address a couple issues brought up by the reviewers, the aqua-planet simulations needed to be re-ran for additional analysis. The CAM4 physics package in the CESM2.1 code base is slightly modified from the original runs (most likely parameters in the Zhang-McFarlane deep convection scheme), and so the three aqua-planet plots look slightly different from the originals. The differences are minor and our conclusions are unchanged.

Reviewer 1

The revised version of the paper looks much better. I am glad to accept the paper with a minor change.

Line 364: For Andes "a lower latitudes" is within 10 to 20 degree from the equator but for Himalayas is within 20 to 30 degree.

The sentence has been modified at the reviewers request:
“At lower latitudes, over the Andes (between the equator and 20˚S) or the Himalayas (from 20˚N to 30˚N), there is a clear preference for extrema to occur at the element boundaries (Figure 10).”

Line 365: their -> there

Fixed.

Figure 12 caption: ne30np3 -> ne30pg3

Fixed.

Reviewer 2

I want to thank the authors for taking the time to address all of my comments on the first draft of the manuscript. This new version of the article is well written and I think a valuable addition to the field. I would recommend it for publication.

I did have a few comments after reading the revised manuscript:

1) Lines 279 and 281, I think you mean se_nsplit instead of rsplit here. If I recall correctly, in CAM rsplit is substepped within the se_nsplit, which is substepped within the dt_physics. So the remap timestep is dt_physics / se_nsplit.

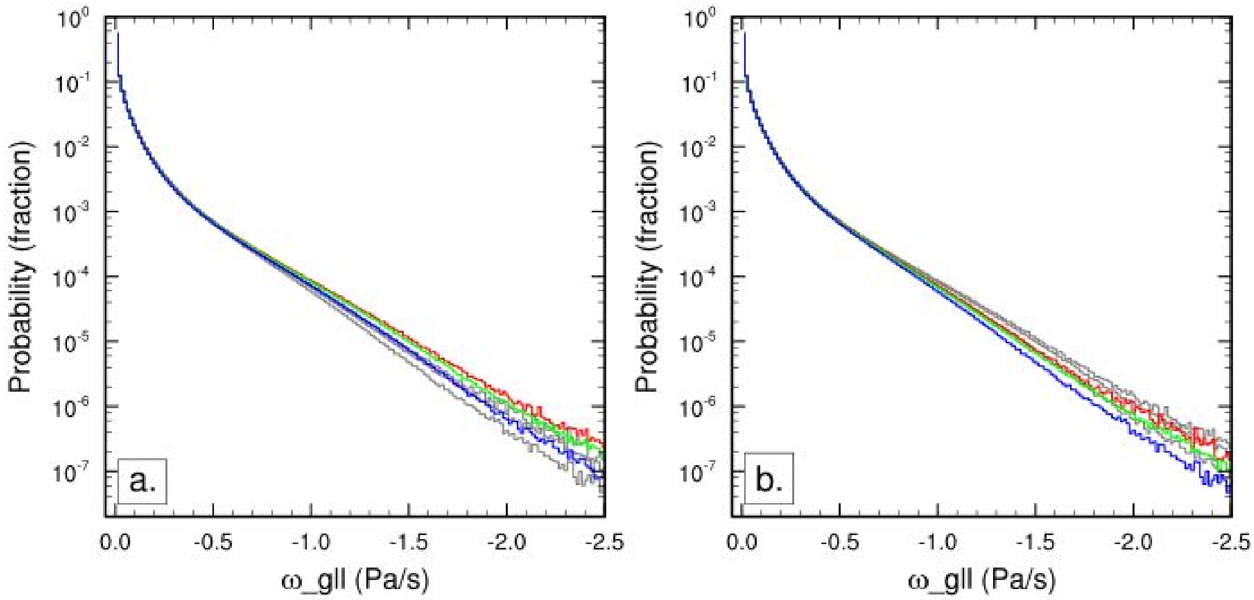
Yes, the reviewer is correct. The vertical remap is cycled nsplit times, and the text has been corrected.

2) Figure 9, The authors state that the np4 and pg3 solutions look the same in this simulation, but looking at the plots on the left-hand side there are clearly some differences in features of the solution, especially at within 30 degrees of the equator. For example the temperature tendency for np4 near 180/25N has a strong negative patch that isn't present in the pg3 solution. Similarly at around 100E/30S there are higher temp tendencies in the pg3 solution than the np4. Do the authors have an explanation for these differences? Could there be something having to do with the mapping of the tendencies from pg3 in physics to np4 in dynamics for the pg3 solution that is causing this? Are these related to topography, or another grid feature?

The reviewer is correct, that the pg3 simulation does not reach a peak mean low-level physics tendency like the one observed in the np4 simulation near 180/25N. The other peak the reviewer is referring to is a minimum; the red patch in the pg3 simulation near 100E/30S. We have added global means to the top of each plot in Figure 9, which effectively shows that the means and variances are slightly larger for np4 compared to pg3.

The plot suggests the physics tendencies may have larger magnitudes in np4 relative to pg3, albeit modestly. This is consistent with the mapping procedures in pg3. The tendencies are evaluated from the state on the pg3 grid, which are smoother due to integrating the basis functions over the pg3 control volumes. Therefore it follows that the tendencies, even before being mapped back to the GLL grid, are more damped relative the np4 runs. This effects the solution on the GLL grid, and drives the model accordingly.

An additional paragraph has been added to discuss the reviewers point (beginning at Line 347). That paragraph references a new version of Figure 3, and mentions that when plotting the pdf using the omega field on the GLL grid, in the pg3 run, the frequency of large magnitude omega is slightly lower, consistent with the reviewers observation. The plot of omega on GLL is provided for the reviewer:



Left is for the ne30np4 simulation, right is the ne30pg3 simulation. Colors are as in Figure 3a.

I thank the authors for their considered response to previous reviews. The majority of my previous points have been addressed, but I still have a few more comments. I am particularly keen that the last of these should be addressed.

• In my previous comment 4, I was suggesting clarification of 'linear correlation preservation' would be useful for the reader, not for my benefit! For readers that are not familiar with this, I still think it would be useful to say something on this after the list of requirements at line 249.

We have modified requirement 4 (line 202) to:
"linear correlation preservation, i.e., if field A is a linear function of B, this relationship is still preserved (see, e.g, equation 5 in Lauritzen and Thuburn 2012)"

• In figure 7, is there any need to include the plot of the constant field – surely it would suffice to say this in the text?

We have chosen to keep the second plot of the constant field, since it is illustrative of our point as a stand alone figure (if someone is just 'browsing' the manuscript).

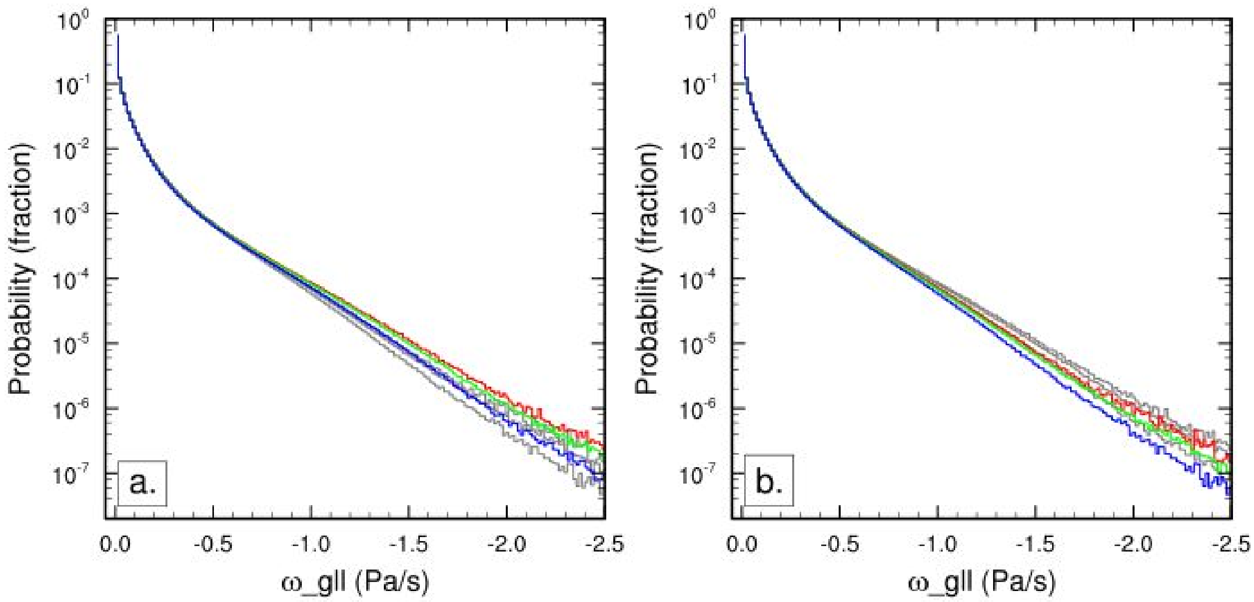
• Regarding Figure 10 (previously Fig7), I don't particularly find the circles beneficial. I would still rather see a difference plot (as you have done in Fig11) to make it easier to pick out and quantify the differences.

We have modified Figure 10 to have the same layout as Figure 11. The prior middle plot (the pg3 simulation on the GLL grid) was removed, and the difference plot was added, as the reviewer suggests. We thank the reviewer for being persistent on this point, since it nicely isolates grid imprinting in np4, and the improvement in grid imprinting in pg3. An additional line of text was necessary to explain the figure (Line 390).

• In my comment 7, I noted the change in the fractional values of large omega and suggested that overlaying the two plots would be helpful to see this. In your response you argue that this particular difference was due to the sampling differences and, having now changed the sampling method, the two figures are now more similar. While I agree, the curves have changed, I think there is still a significant difference that it would be good to highlight and could do with some explanation. See below for my poor man's attempt at overlaying the curves – the gray curves in Fig 3a represent the data from Fig 3b and vice versa. I would like to see a plot like this and – if my quick attempt is not far from the truth – a comment on the fact that there seems to be a systematic increase in large omega values with the new method. (This is counter to my intuition which would lead me to expect systematically more frequent large omega values with the old anisotropic method).

We also thank the reviewer for his/her persistence on this point. The 'poor man's' attempt is fairly close to the truth. It turns out, that this was an artifact of not using the exact same bin widths between all six curves - we let ncl determine the bin widths. This of course was not wise, and we have re-done the figure using the same bin-widths (see new Figure 3). In addition, as the reviewer suggested, panel (a)'s curves are overlain on panel (b) in grey, and vice versa. The result is much more consistent with the reviewers and our thinking - the larger magnitude vertical velocities are systematically less frequent in pg3.

An additional paragraph was added (beginning at Line 347) in response to another reviewers observation of Figure 9, that the mean and variance of the physics tendencies on the GLL grid are slightly lower in pg3 relative to np4. We therefore did another plot, like FIgure 3a that looks at the pdf of omega on the GLL grid, in the pg3 simulation. We do not show this in the text, but it is provided here for the reviewer:



Left is for the ne30np4 simulation, right is the ne30pg3 simulation. Colors and grey lines are as in Figure 3a.