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Geoscientific Model Development
Discussions

Interactive comment on "Description of the resolution hierarchy of the global coupled HadGEM3-GC3.1 model as used in CMIP6 HighResMIP experiments" *by* Malcolm J. Roberts et al.

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Reviewer 3: Justin Small Comments I think the main question is whether these simulations make robust controls against which transient simulations can be compared, and I think you do not address this directly (especially in the Conclusions and Abstract – the Abstract in fact does not clarify that you only look at the 1950 runs.) I would like a bit more discussion of this. Do you think it appropriate to identify climate change by subtracting the drift of your 1950 runs from the transient runs?

Response: The abstract has been modified to only mention the control simulations. Understanding whether we can identify a climate change signal with this experimental design is an open question but outside the scope of this work as indicated both by our questions (page 2 line 21) and on page 6, line 5. We have also added some discussion into the summary (page 16, line 19).

Page 3, line 6 – a little more detail on the atmosphere grid, e.g. how many levels in âĹij lowest 1km, how many levels in stratosphere. Line 8 – same for ocean, how deep does the 1m spacing go, # points in upper 100m and approximate spacing in main thermocline? This could go in the Table.

Response: New Table 1 now shows a selection of model levels and their associated height/depth.

Line 15. Presumably MACv2-SP scheme is used in both control and transient simulations? Line 20.

Response: Clarified on page 3, line 18.

For the unfamiliar – what is the "USSP launch factor"??

Response: Clarified on page 3, line 23. It is simply a model parameter used to adjust the QBO frequency.

Page 4, lines6-7 is a repeat re aerosols.

Response: The description has been unified on page 3, lines 13-17.

Line 11. Re solar cycle: do you expect the solar cycle to have a major impact, thus requiring your protocol of smoothing out the solar cycle?

Response: it may well make no difference, but this was not tested so we were just extra careful.

Table 1 – a curious point, why is CMIP6 nominal resolution for atmosphere âĹij 2*grid

spacing, but for ocean it is âĹij 1*grid spacing? Or do I misunderstand? Also, put a statement in the text that you use the word resolution to mean “grid spacing” if that is what you do (in common with most papers). Also, add to Table whether runs are spun up or initialized from another run, then add total run length.

Response: Done. Text on page 3, line 22 has clarified CMIP6 nominal resolution and defined model resolution. New Table 3 now included initial conditions and total length of simulations.

Page 5 lines 25-26. It is impressive that LL, MM and MH are run for extended long periods which helps put the 100 year results in context.

Response: It took a lot of time and CPU!

Page 6, line 14-16. Add units. Lines 13 to 16 could be usefully included in a Table and combined with the coupled model values.

Response: Done (now lines 21-22). Since the coupled values are shown in the Figure, and the atmosphere-only simulations are very different from the coupled model (given SST, sea-ice etc), I’m not sure that a table directly comparing these values would be informative.

Line 24. ML is repeated twice.

Response: Done (now line 30)

Line 29. Parentheses around “(beyond . . . model)”

Response: Done

Line 32. I’m not an expert on this, but I’ve heard that standard resolution PI controls are typically tuned so that the TOA imbalance Âń 0.1W/m2. Your values are somewhat larger – any comment?

Response: This experiment is for 1950’s conditions, and hence one would not expect

or want a near- zero TOA. In addition, as in Menary et al. (2018), the TOA in a PI control can be greater than 0.1 and still be judged reasonable if the trend is negligible. For the HighResMIP experiment, with no extra tuning, we are pretty happy how low the TOA is, and we do not claim that the models are in equilibrium.

Page 7. Line 5. Delete “in” before “near”

Response: Done

Line 11. I would say the reduction of SW CRF bias off North America is notable smaller than other regions.

Response: page 7 line 19 we have qualified the North America change

Fig. 7. There is a linear feature in Figs 7a-c in Southern Hemisphere at about the latitude of south- west tip of Australia. Is this an artefact of interpolation, or in original EN4 products?

Response: I tested several methods of interpolating EN4 and models to grids and this did not change. I think it is simply that the isotherms line up similar to latitudes in this region and the models slightly shift to give a cooling further north.

Page 8 line 8 – cold bias possibly due to “the experimental design of using EN4” initial conditions. Can you expand on this? I remember early versions of CESM2 also had a cold bias for some runs initialized from Levitus. Is there something about these models that lead the surface to cool when initialized from observations?

Response: I added to Page 8, line 18 that the TOA is negative in the first few decades which would lead to a surface cooling. Without further analysis I don’t have a better understanding of this.

Line 12-13. What about the typical warm bias of many degrees seen off the coast of N America or Japan due to western boundary current separation problems – do you see them in LL, and do they reduce at higher resolutions?

Response: I've added text to page 8 and the top of page 9 to mention these biases.

Line 28, "particularly in the ocean upwelling regions" – you could reference Gent et al 2010 (Clim. Dyn.), Small et al 2014 (JAMES), 2015 (J. Clim) who found consistent results in CCSM4,CESM1 regarding reduction of SST bias with atmosphere resolution.

Response: Done

Line 29-30. This is also consistent with CESM e.g Small et al 2019, Climate Dynamics (2019) 52:2067– 2089, their Fig. 9 – high resolution cools at the coast (reducing bias) but warms further offshore. In general are Figs. 7i-k consistent with Griffies et al 2015, von Storch et al 2016(Ocean Modelling, 108, 1-19)? See also later.

Response: I have added the Small et al. and Griffies et al. references. Von Storch et al does not have an SST bias plot, so I've left that for the vertical diffusion discussion (your point below).

Fig. 7d. The changes off Peru-Chile are smaller than I would expect from Figs 7a,b. Any thoughts? Does it relate to interpolating to a common, coarse grid?

Response: I'm not sure I agree. The warm bias at the coast in LL reduces strongly to MM and disappears by HH, primarily due to atmosphere resolution, in line with what I would expect.

Fig. 9. It seems that surface temperature over Greenland improves, but less dramatically than over other parts of Arctic. Is the bias over Greenland a true model problem, or lack of observational data? Is there an ice-sheet component to the model?

Response: There is no ice sheet model. It is difficult to say whether it is an observational or model problem. The different resolutions will represent the orography differently, and the representation of land ice is fairly simple.

Section 3.3 illustrates generally large changes with resolution. The depth scale in Figs 10, 11 is strange, probably stretched too much in upper ocean. Also, why not show

HH?

Response: In these figures I only show the simulations with the same resolution as those with a corresponding spinup-1950. The vertical scale is chosen for clarity, it is meant to enhance the near surface, both because the differences are generally larger here and smaller at deeper levels, and to be proportional to the model levels/depth spacing.

Griffies et al 2015 show some role for submesoscale (parameterization) in the heat budget. Does your model have such a parameterization? In Small et al 2014 we speculate that lack of submesoscale param. in the high-res model might explain some differences with the standard resolution model, which did contain the parameterization.

Response: I have added a paragraph (page 10, line 25) to include some of this discussion.

To complement Figs 10, 11, I think it is very useful to see spatial maps of temperature and salinity at say 500m or 1000m, at end of 100 year run, to look at regional detail. For example, do problems with Mediterranean Outflow, or Agulhas leakage, contribute to bias and drift?

Response: An additional new figure 12 has been included showing temperature and salinity biases at 950m, and text on page 10, line 20.

Page 10, line 30. I think this is a common problem with low resolution models, papers by e.g. I. Richter discuss this at length.

Response: I added a sentence to reflect this, page 11 line 21.

Figure 12. It is interesting that changes due to ocean resolution (Figs 12i,j) are comparable in magnitude to those due to atmosphere resolution.

Response: I have noted that (page 11, line 28)

Section 3.6. High resolution CESM also had a weaker ACC transport than standard resolution CESM (Small et al. 2014). Any thoughts why HH, MH has weaker ACC than LL (in addition to your explanation for MM)?

Response: I have added some text – page 13, line 28.

Fig. 17. All the power spectra look quite sensible, but then I noticed the log scale ordinate. If plotted with linear ordinate would it be easier to see model differences and model biases?

Response: With a linear scale the plots become extremely noisy, as can be seen from the subsets of 50 year chunks. In addition I have removed the shorter simulations (responding to Reviewer 2).

Section 3.5. I think you should emphasize more how good the high-resolution models (MH, HH) are in the deep ocean in terms of AMOC mean profile. Put this in the context of what the AMOC actually represents in terms of major ocean currents.

Response: In have included an indication of this on Page 13, line 4.

Fig. 18, 19. Consider adding contours of Sea Level Pressure for the composites.

Response: Done.

Section 3.7. Also, consider the paper: Deser et al 2017, J. Clim. "The Northern Hemisphere Extratropical Atmospheric Circulation Response to ENSO: How Well Do We Know It and How Do We Evaluate Models Accordingly?"

Response: I have added a comment to this effect (page 15, line 19).

Finally, there has been a recent paper published (which unfortunately I cannot find now, but I think was published in 2019) that showed the slightly surprising result that although a high-resolution ocean model gave much reduced SST bias in the N. Atlantic in the first 50 years of the run, compared to low-res, the biases looked much more similar (between resolution) at the end of a multi-century integration. (In other words, the high-res bias increased substantially with time). Their paper used forced ocean-ice

models. I wonder if this has relevance for your paper which only looks at 100 years of high-res. Perhaps the results will differ between coupled and forced simulations.

Response: Unfortunately we could not work out to which paper you were referring.

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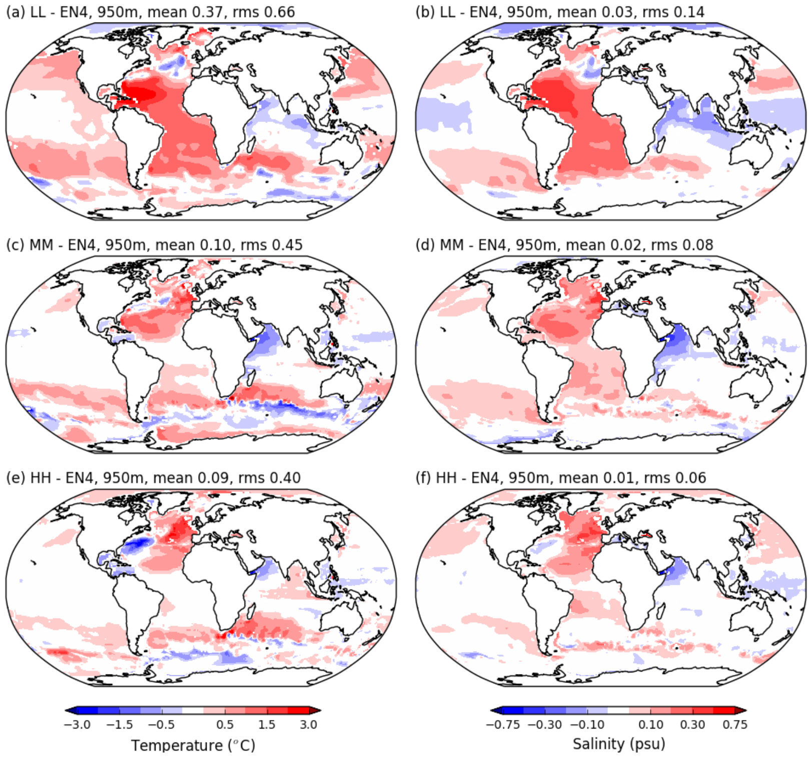

Fig. 1. Ocean biases at 970m depth