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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Received and published: 21 July 2019

The paper describes a major contribution to the HighResMIP experiments, namely a very comprehensive suite of resolution comparisons by the UK Met. Office group with their latest climate model. The HighResMIP protocol includes a 1950 spin-up ($\sim$30 years), a 1950 control (100 years) and transient simulations of 1950-2050. (These choices were governed by necessity: it is too expensive to run high-resolution simulations over many 100s of years as in main CMIP6 protocol.)

This initial paper focusses on the first two components with an aim to determine whether the protocol is effective. For example, is the spin-up too short, is the upper ocean in control still drifting rapidly after 100 years (not ideal for comparisons with transient), is 1950 a good base point? Most of these questions are comprehensively addressed except for the latter (appropriateness of 1950) which could be addressed in future work on transient simulations.

The paper is thorough: the set of experiments is outstanding in its breadth of resolutions for climate models: and the paper should be a good reference for the community and HighResMIP users in particular. I only have minor comments before, in my opinion, it should be published.

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?

Page 3, line 6 – a little more detail on the atmosphere grid, e.g. how many levels in $\sim$ 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.

Line 15. Presumably MACv2-SP scheme is used in both control and transient simulations? Line 20. For the unfamiliar – what is the "USSP launch factor"??

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

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?

Table 1 – a curious point, why is CMIP6 nominal resolution for atmosphere $\sim 2^*$grid spacing, but for ocean it is $\sim 1^*$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.

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.

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.

Line 24. ML is repeated twice.

Line 29. Parentheses around "(beyond . . . model)"

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 « 0.1W/m2. Your values are somewhat larger – any comment?

Page 7. Line 5. Delete "in" before "near"

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

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?

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?

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?

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.

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.

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?

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?

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?

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.

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?

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.

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

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)?

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?

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.

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

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?"

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.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-148, 2019.