

Interactive comment on “High Resolution Model Intercomparison Project (HighResMIP)” by R. J. Haarsma et al.

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Response to anonymous referee #3

We thank the reviewer for his/her critical remarks and the positive attitude towards the manuscript.

"My only concern is the lack of a coupled experiment that is likely to have a large signal to noise ratio. An example might be a quadrupling of CO₂ at some point during the 1950s control. A similar experiment is proposed in CMIP6 (albeit from pre-industrial conditions). The role of high ocean resolution in heat uptake could provide some results that may be difficult to tease out of the tier 2 simulations. The computation costs could be relatively modest. For instance, Kuhlbrodt et al. (2015) showed useful results from a 20-year experiment."

This type of experiment was already proposed and discussed within the HighResMIP community. In our original protocol it was omitted mainly because to limit the number of experiments. In response to your remark we have discussed this option again and we have now explicitly included this experiment as an additional Tier. We agree with you that it has significant added scientific value and that the computational costs are relatively modest. We have added in 3.4 an additional targeted experiment with an abrupt 4xCO₂ increase in the coupled climate model.

"Minor comments"

"Abstract: define the acronym MIP"

Done

"Introduction: A few more acronyms to define, e.g. ITCZ, MJO, QBO."

Done.

"Section 2.1: Give a proper reference to the CMIP6 publication in this same special issue."

The reference Eyring et al. 2016, that describes the DECK simulations in this special issue, has been added.

"Figure 1: Provide a more detailed caption."

We have provided a more detailed caption. We hope that the content of the figure is clear now.

"Section 3.1.1: As noted we do not have high resolution data for the entire historical period."

We have added a sentence mentioning this.

"Are there any issues with blending the pre and post satellite era data? Whatever methods are used to produce 1/4 degree SSTs, the raw observations simply are not

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there. For instance the process outlined for producing future (2015-2050) SSTs relies upon the variability being unchanged in a changing climate. Please comment."

It is true that high resolution observations of SST were not available before the satellite era. The ability to produce 0.25 degree fields of actual SST comes from our use of a satellite-era climatology. The analysis of residuals from that climatology (aka anomalies) which we do on a 5-day timescale is performed on a 1 degree grid. On that spatial and temporal scale, that analysis is perfectly achievable with the data available and is done both for pre-satellite data and satellite-era data alike. Information on covariances between anomalies in different locations gained from the satellite (and in situ) data enables the analysis to be done at the same resolution throughout. The ensemble captures the uncertainty in this process and its spread is wider prior to the satellite era. The Rayner et al manuscript (in prep.) and a technical note for the HadISST (Kennedy, J. et al. in prep.) will describe more in detail the process of generating these data for the past.

For the future, of course you are right that it is a large assumption that the variability will not change. However, we have no other satisfactory way of generating the future forcing, and since the ForcedAtmos future simulations are more useful to contrast different model responses to the same future warming (which is not possible in the coupled models), they are in no way a future projection, so this assumption is not so important.

"Section 4.2, page 10, line 34: insert a space somewhere in andreanalysis."

Done

"Section 6, page 12, last line: No need to define ToE as acronym not used."

Done

"Section 6, Ocean model biases: insert the word "coastal" before upwelling as the equatorial upwelling zone bias is often different to this."

Done

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"Section 7.1, page 14, 2nd to last sentence: Is there any evidence that sea ice simulations might improve with increasing resolution. If so give a reference. The project will be moving towards the limit of where the continuum hypothesis is reasonable, which may be an issue."

The reviewer correctly points out that the continuum hypothesis used to model sea ice dynamics may become an issue as we move to high ($\sim 1/4^\circ$) and very high ($\sim 1/12^\circ$) resolutions. However, there is to our knowledge no scientific evidence that the viscous-plastic or elastic-viscous-plastic rheologies are not adapted (numerically or in terms of results) to such high resolutions. Preliminary tests conducted at $1/4$ and $1/12^\circ$ with the NEMO-LIM3 ocean-sea ice model indicate not only stable results, but also realistic heterogeneities and intermittency behaviors in the sea ice cover. HighResMIP will be the perfect testbed to assess whether these increases in resolution have to be conducted in conjunction with development in model physics (rheology in this case), or if the two can be done separately. We have added a remark about this in the manuscript.

"Section 7.1, page 14, last sentence: This doesn't sound quite right. Maybe Differences would be better than Difference."

Done

"Section 7.1, last sentence: replace "such as" with "outlined by" or something similar."

Done

"Section 7.4, page 16, paragraph 2: Too many acronyms reduce the readability of manuscripts. AR seems a bit unnecessary as it replaces just two words and only used twice. TC (a bit later on) also seems a bit unnecessary, but as it is used a few more times I could live with it."

The acronym AR for atmospheric rivers has been removed.

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-66, 2016.