

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

Anonymous Referee #1

Received and published: 20 May 2016

This manuscript describes the proposed HiResMIP protocol which focused on the inter-comparison of “high” resolution AGCMs and CGCMs, defined as 25 to 50 km resolution for the atmosphere and eddy permitting for the ocean.

Main comments:

1. My main comment is that the current draft does not make a compelling case as why such a comparison is needed and what are the expected scientific benefits. Many are claimed but the current text does not justify them in a clear way.
2. For example a major claim is that such a MIP will help model improvements. But I could not find in practice what will this mean, i.e. how the knowledge obtain will inform model development. Increasing the resolution has always been a natural pathway for modelling groups and it is unclear how having this MIP or not will change the related

C1

priorities.

3. Another example is that only horizontal resolution is included in the protocol. This is a very serious shortcoming as many processes depend on both resolutions (horizontal and vertical) such as atmospheric convection or ocean mixing. As quickly alluded to, solving the diurnal cycle over the ocean requires 1m vertical resolution at the top level of the ocean and 2-3 hours coupling time step. I was surprised that this is not a requirement for this MIP.
4. The introduction suggests that modes of interannual variability can be analysed in these short simulations, which is not the case for ENSO for example (several centuries are needed). The details given near the end are careful not to include the modes prone to this sampling issue but the introduction should clarify these limits upfront.
5. The forcings are going to be different between the CTRL and the HiRes simulations for some model (e.g. aerosols). This is an issue that will prevent a clean comparison. Along the same lines, when changing the resolution one can never have “exactly the same parameters”. This limit also renders the comparison less informative.
6. The use of daily SSTs for the AMIP simulation is an issue I believe. As shown by several studies (Wu and Kirtman 2005, 2007, Cassou 2015), the mid-latitudes ocean is forced by the atmosphere, not the other way around. The classical use of smoothed monthly SSTs somewhat mitigates this problem. The use of daily SST requires a much better justification and an analysis that this will not have an impact on extremes over land (adverse impact was shown by Cassou 2015).
7. Finally the discussion on the benefits of increased resolution is not balanced, and mostly ignores the studies that don't show any impact of resolution, for example on model biases. The role of physical parameterization is not discussed even though it is central. Improving this balance would strengthen the manuscript which currently mostly appears as a manifesto of like-minded people.

C2

References:

Cassou C. (2015). Some critical technical choices for pacemaker experiments, Aspen DCPD Workshop June 2015. (<http://www.docfoc.com/aspen-dcpp-workshop-june-2015-some-critical-technical-choices-for-pacemaker>)

Wu, R., Ben P Kirtman, Pegion, K., Center for Ocean-Land-Atmosphere Studies. (2005). Local air-sea relationship in observations and model simulations.

Wu, R., & Kirtman, B. P. (2007). Regimes of seasonal air-sea interaction and implications for performance of forced simulations. *Climate Dynamics*, 29(4), 393–410. <http://doi.org/10.1>

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