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Geoscientific Model Development Discussions

Interactive comment on "Climate model configurations of the ECMWF Integrated Forecast System (ECMWF-IFS cycle 43r1) for HighResMIP" *by* Christopher D. Roberts et al.

Anonymous Referee #2

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Review of "Climate model configurations of the ECMWF Integrated Forecast System (ECMWF-IFS cycle 43r1) for HighResMIP" by Roberts, Senan, Molteni, Boussetta, Mayer and Keeley, submitted to Geosci. Model Dev.

This paper looks at the climate modelling skill of the ECMWF Integrated Forecast System. (ECMWF-IFS). ECMWF-IFS is typically only used for forecasts up to a year ahead. There is an interest to see if it can also perform climate-length simulations. The experiments follow the HighResMIP protocol and are based on the PRIMAVERA project.

The paper is very thorough and realistic about the skill of the ECMWF-IFS and its

sensitivity to resolution. A 1deg. ocean and 0.25deg. ocean are considered, and the atmosphere is at either 50km or 25km. As well as the high and low resolution configurations, a mixed resolution run with low-res atmosphere and high-res ocean is employed. I abbreviate the runs to LR, HR and MR.

This is the first paper that I am aware of that arose from HighResMIP, and I think it is a very valuable contribution to the literature, being honest about improvements and degradations due to resolution. This is useful for the community in the choosing between e.g. larger ensembles or higher resolution, to get a more reliable indication of the climate system characteristics.

Comments General comment: I think some reference to earlier papers discussing sensitivity of climate models to resolution should be made, both in the Introduction, and their findings may be useful for the main results section. See e.g. (but not limited to) McClean et al 2011, Delworth et al. 2012, Bacmeister et al. 2014, Small et al. 2014, Griffies et al. 2015.

Page 3, Line 29. I think this should be ECMWF-IFS-HR (uses Tco399 grid)

Page 5, Line 16. It would be useful to know why the run-off from land is not coupled. Is this not important for short-term forecasts i.e. the main use of the model?

Page 9, lines 5-10. The results for the adjusted 0-700m ocean heat content change are indeed quite impressive (after removing the drift in CTL). Was such a good fit (to ORAS4) expected? This positive result should be mentioned in Conclusions and Abstract.

Figs 6, 9. It is interesting that the coupled model biases in SST in eastern boundary regions do not seem to largely affect the CRF or net surface shortwave. (Compare uncoupled and coupled results)

Page 12, line 20. You could add here the warm SST bias at eastern boundaries is common to all resolutions but with slightly differing magnitude (see Small et al. 2015

for some discussion of sensitivity to resolution.)

Page 12, lines 25-30. In Fig. 12a,b, is the eddy part for LR got from the GM parameterization? (Please clarify in text.) Is the eddy part for MR shown in Figs 12a-b ? Finally, in the text please point out the latitudes of the improvements in the eddy heat transport. For the Atlantic it seems that eddy transport may contribute to differences between models at around 40deg. N, but does not contribute much between 10deg.N and 30deg.N. So presumably the mean flow is quite different between models in the latter latitude range.

Fig. 3,13. In the N. Atlantic, LR may have a slightly larger SST bias in year 1 (Fig. 13), which may be amplified under coupled feedbacks in the long term (Fig. 3) whereas for MR and HR the coupled feedbacks may be less important ? You might want to add an additional figure showing close-up of N. Atlantic for the SST bias in year 1 and long-term.

Also for Fig. 3, 13, I was a bit surprised about the lack of sensitivity of MLD to ocean resolution, following the work of Lee et al. 2011. Firstly you might want to use an updated MLD climatology such as Holte et al 2017, Roemmich et al 2009 using the vastly better sampling of ARGO. Please show a close up of the S. Ocean, showing MLD for LR-ARGO, MR-ARGO and HR-ARGO, all for the key winter season, July-August-September.

I'm not sure if this is mentioned in the text, but it is very notable from Figs 3,13 that the biases in SST, SSS, SSH in the S. Ocean are a slow process (but MLD biases appear immediately). Any thoughts on this?

It might be worth mentioning that MR and HR have excessive deep mixed layers in the Labrador Sea (but again see comment on using a more recent observed product.)

Regarding the very weak ACC in MR and HR (half as much as observed) is this simply due to the reduced temperature gradient between the poles and subtropics (from the

warm SST bias at high latitudes), and a consequence of geostrophy/thermal wind, or is it something more complicated?

Fig. 16b, c – it looks like the model has some events longer than any observed (e.g. 42-48 months). Perhaps this is just due to the short observed record, such that longer events may be possible but not recently observed.

Page 18, lines 12-13 and then lines 19-21 are worded very similarly so that I was confused as to whether this was a sentence erroneously repeated, or a real result. Can they be reworded ?

Page 19, lines 16-19. I was confused by these sentences. Can they be clarified and explained in more detail?

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