

This is my first review for this paper. The authors present the EC-Earth3P-VHR model configuration, a high-resolution global climate model developed for HighResMIP, featuring atmospheric resolution of about 16km and 8km oceanic resolution. The model shows improvements in key regions like the Gulf Stream and the Equator compared to lower resolution models, with reduced biases in some areas but increased biases in others, such as a larger warm bias over the Southern Ocean. The model also demonstrates better air-sea coupling in tropical regions. However, it tends to overestimate the oceanic influence on atmospheric variability at mid-latitudes. Overall, the EC-Earth3P-VHR configuration appears to offer enhanced opportunities to study climate variability and change on regional and local scales.

First of all, the paper is in my view well-written, understandable, and has basically no typos. The figures are all high quality and well done. A description of the EC-Earth configuration for HighResMIP is clearly within the scope of GMD.

We thank the Reviewer for the appreciation on our work. In the following, we answer each specific point (in blue).

My only minor comments are with respect to highlighting some key results better, and better embedding the study into previous work, also outside of Europe. I have provided some references below for that purpose that the authors can decide to include at their convenience, and also gave suggestions for potential additional figures that could make the study even stronger. Overall, the study in its present form is already very interesting, it lists key results that are encouraging for fellow coupled high-res modellers, and is worthy of prompt publication. I am providing line-by-line comments below that I'd suggest having included before the paper can be accepted.

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Line-by-line comments:

l.59-60 I would suggest to cite relevant papers for these projects, for example Hoffmann et al. 2023 (<https://doi.org/10.1016/j.cliser.2023.100394>) for Destination Earth or Hohenegger et al. 2023 (<https://doi.org/10.5194/gmd-16-779-2023>) and Rackow et al. 2024 (already cited elsewhere in the study) for nextGEMS. There should be something from MetOffice for PRIMAVERA as well

The references have been added to the reviewed manuscript.

l.64 have been “proven”

Modified, although both proved and proven can be used.

l.84 For single precision, there are other earlier examples, e.g. Váňa et al. 2017 (<https://doi.org/10.1175/MWR-D-16-0228.1>); as Destination Earth and nextGEMS were listed, there is also Sarmany et al. 2024 (<https://doi.org/10.1145/3659914.3659938>) for IO considerations

Both references have been added to the reviewed manuscript.

l.91 Another extreme example next to Gutjahr et al. 2019 is AWI's CMIP6 climate model (e.g. Rackow et al. 2019, <https://doi.org/10.5194/gmd-12-2635-2019>, Semmler et al. 2020

<https://doi.org/10.1029/2019MS002009>), where their ocean is locally finer than 10km and has been run with a 100km atmosphere. There might be other examples in CMIP
The references have been added to the reviewed manuscript.

l. 104 Regarding “High-resolution modelling usually relies on single-model component”:

I think there are several examples for relatively high resolution in both components, e.g. Chang et al. 2020 (<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020MS002298>), Small et al. 2014 (<https://opensky.ucar.edu/islandora/object/articles:14403>), the high-res studies from the South Korean group (<https://ibsclimate.org/research/ultra-high-resolution-climate-simulation-project/> and listed references there, e.g. <https://www.science.org/doi/10.1126/sciadv.abd5109>), and the DestinE, EERIE, PRIMAVERA and nextGEMS results of course as well

We agree with the Reviewer that there are indeed examples of high-resolution modeling in both the atmosphere and ocean. However, many of these examples are relatively recent (from 2020 on). The point the paragraph tries to make is that a part of the community has relied and still relies on forced single-model components (e.g., atmosphere-only and ocean-only models), and sometimes regional configurations, for their high-resolution studies, because either they do not have the computing resources, or because those configurations are more appropriate to address their research questions. Some of these studies are as recent as the ones listed by the Reviewer (e.g., Swingedouw et al., 2022) These approaches come to a cost, nonetheless, which is usually i) relying on boundary conditions generated by low-resolution models or observations, and ii) lacking a global perspective and climate model interactions. The introduction to the paragraph has been modified to make our intentions clearer.

References:

Swingedouw, D., Houssais, M.N., Herbaut, C., Blaizot, A.C., Devilliers, M. and Deshayes, J.: AMOC recent and future trends: a crucial role for oceanic resolution and Greenland melting?. *Frontiers in Climate*, 4, 838310, <https://doi.org/10.3389/fclim.2022.838310>, 2022.

l. 137 “of the” -> “of”

Modified.

l. 138 Is this OpenIFS or IFS?

As indicated in the manuscript, it is IFS in its 36r4 cycle.

l. 161 240 s and 720 s has large white spacing

Modified.

l. 198 What are the novel source code changes?

The section has been reformulated in the revised manuscript, with more details about the changes in the source code.

l. 200 Which model workflow software?

We refer to the Auto-EC-Earth model workflow, which is used to run simulations at BSC. This was added to the revised manuscript.

l. 204/205 Is there a parallel version of this available now? Is this linked as part of your document?

The coupler model version, OASIS-MCT 3.0, did not have parallelization in weight generation at that time. More recent versions of OASIS3 (such as the one in EC-Earth4) support OpenMP work sharing. This speeds up the process and allows EC-Earth4 to couple meshes on the fly. This is now indicated in the revised manuscript.

l. 209 Can you write technical details of “the network” here? Otherwise this does not tell much.

Added

l. 279-281 This inconsistency could trigger a bigger adjustment potentially. From your experience, does this lead to a cooling or warming initially that gets levelled out during the spinup?

The difference between temperature potential temperature and conservative temperature is very close to zero for the range of salinity values in the ocean (see Figure 2 in McDougall et al., 2021). Practical salinity can take lower values than absolute salinity (~0.1 psu as in Pawlowicz, 2013). The adjustment to this difference, however, will depend on the preferred state of the model. In regions that tend to show negative salinity biases, initializing from a lower salinity values might lead to a smaller adjustment. Regions with positive salinity biases might, by contrast, would show stronger adjustments. The global effect is in the end difficult to assess. Nonetheless, and as shown for the second Reviewer, the drift in the globally averaged ocean salinity is no larger than 0.00005 psu/yr in the last 50 years of the control simulation in the three configurations, being the smallest in VHR. This result suggests that any potential adjustment due to the incorrect initial conditions is soon levelled out during the spin-up phase.

References

McDougall, T. J., Barker, P. M., Holmes, R. M., Pawlowicz, R., Griffies, S. M. and Durack, P. J.: The interpretation of temperature and salinity variables in numerical ocean model output and the calculation of heat fluxes and heat content. *Geoscientific Model Development*, 14(10), 6445–6466, <https://doi.org/10.5194/gmd-14-6445-2021>, 2021.

Pawlowicz, R.: Key physical variables in the ocean: temperature, salinity, and density. *Nature Education Knowledge*, 4(4), 13, 2013.

l. 296 “but for” -> “except for”

Modified.

l. 310 “will enjoy” please rephrase

Done.

l. 314/315 This is a very encouraging result; in principle, this is what modelling groups have been hoping for to see with higher resolution. This point could maybe be highlighted, potentially with a figure, and be included in the abstract?

These lines describe the results of Figure 3; we therefore do not think an additional figure is needed. Nonetheless, the result has been mentioned in the Abstract, as suggested.

l. 389 delete “bias” in this line?

Removed.

l. 401 40 deg N seems far away from polar influence, are you sure about this statement?

The linkage is suggested between the warm subsurface bias and the sea ice anomaly at subpolar latitudes in the Labrador Sea, between 45–60 °N. The connection is therefore less remote. As explained in the manuscript in the lines following that, the excess of sea ice weakens the surface oceanic circulation (subpolar gyre), leading to a heat accumulation to the south near the surface in the intergyre region (at around 40 °N). Also, the excess of sea ice and the associated weak oceanic deep mixing in the Labrador Sea would reduce the vertical exchange between surface waters, which are cooled down by the atmosphere, and the subsurface, leading to an anomalous warming as well.

Figure 13: Hard to see anything on those maps. Could you try with other colors or try a different (shorter) range?

The Figure has been modified to increase clarity.

l. 459/460 The plus/minus refers to what, standard deviation of monthly values?

Clarified.

l. 493-495 This seems like another key result that is very encouraging and not covered with a separate figure.

This result refers to Figure 4, which is now indicated in the revised manuscript. Although we could add another figure, zooming in over certain mountain regions, it would not add additional information to that in Figure 4. Therefore, and for the sake of conciseness, we have decided to refrain from doing so. This result, nonetheless, has been highlighted in the Abstract and Conclusions.

l. 535 Sections -> sections

Changed.

l. 561 The lack of ocean current feedback comes a bit as a surprise here and could be covered earlier in the model description as to how the coupling is implemented

Added to the reviewed manuscript.

l. 580/581 Maybe give another example here if you know it (e.g. US or South Korean references mentioned above if matching), but this statement might indeed be correct

References have been added for completeness.

l. 585 excessive whitespace after “performed”

Changed.

l. 619/620 Another study in this direction would be Sein et al. 2017 (<https://doi.org/10.1002/2017MS001099>). The authors argue that resolution over the shelf areas northward of the Gulf Stream front is key, an area where the cold Labrador water from the north meets the warm Gulf Stream water.

The reference has been included, and the text updated.

l. 687/688 “and VHR does it faster and appears more stable after 100 years than HR and LR” Another key result, see above

We agree. This result is now highlighted in the Abstract.