Reply to Referee comment #2:

The authors would like to thank the referee for their time, as well as their invaluable comments and suggestions. In the following each comment, suggestion or concern is replied in green font. Specific revisions of the text are in quotes, with the respective changes highlighted in bold.

Overall I found this to be a very relevant and useful study, with excellent figures and well-written text. Please see my minor comments below:

Introduction first paragraph: First sentence — there isn't really a scale separation between "underlying processes" and "changes in Earth's climate", there are changes and dynamics on a continuum and they're all linked/interact with each other.

Indeed ! We rephrased the first sentence in : "The vast range of spatial and temporal scales of Earth's climate system and the underlying processes involved, make numerical climate simulations a computationally costly endeavor : **it requires representing the effect of small scales (< 100km) in long simulations (> 500 yr).**"

Second sentence "their" isn't obviously grammatically related to "climate simulations".

Revised version : "Limited available computational resources therefore impose constraints on the horizontal resolution **of future climate projections.**"

Line 16: There's a difference between mesoscale eddies and geostrophic turbulence (the latter is a broader term); I'd say something like "mesoscale eddies are the most salient feature arising from geostrophic turbulence".

Thank you for the clarification. Here is the revised line : "These grid scales coincide with the horizontal scale of geostrophic turbulence (Chelton et al., 1998). **A prominent feature associated with turbulence at these scales is the formation of mesoscale eddies.**"

Introduction second paragraph: It's not just winds sustaining the PE reservoir but also heterogeneous buoyancy forcing.

Indeed, hence we revised in : "Winds inject kinetic energy at the surface, and, **together with heterogeneous buoyancy forcing,** they sustain a reservoir of potential energy (PE) at large scales."

Line 19: There's a cascade of energy into the first baroclinic mode as well, so it's an upscale and downscale cascade of energy (see Smith and Vallis 2001 Fig. 4 for example). In the barotropic mode there's an upscale transfer, but in the higher modes the energy transfers go both ways and funnel energy into the 1st baroclinic mode.

Thanks for this clarification. revised version "The thereby excited baroclinic modes and nonlinear interactions between them lead **to energy transfers across scales.**"

How valid are the parameter values chosen for the linear EOS when considering high-latitude vs. low-latitude behavior (where S vs. T are respectively more dominant in setting density)?

This was extensively explored in Caneill et al. (2022), where they tested this EOS across a range of parameters to investigate what sets the polar transition zone (transition from temperature controlled alpha ocean to salinity controlled beta ocean). We chose the EOS parameters for DINO after personal exchange with Romain Caneill and Fabien Roquet and refer to their studies for reference.

Can you explain more what is meant by the NW2 style bathymetry introducing an “undesirable separation into two basins with respect to dense water formation and overturning”? The real ocean does have this feature so I’m not sure where this hypothesis came from. We found that the ridge was important to setting some of the vertical structure properties of the eddies and potentially the broader circulation (Yankovsky, Zanna, Smith, 2022).

The DINO configuration as presented in the paper is the outcome of a very large number of sensitivity experiments, where we tested the bathymetry, amongst other characteristics. We started with the NW2 style bathymetry including a mid-Atlantic Ridge and found that it splits the subpolar gyre, leading to convection in the eastern half of the basin. The real ocean has a mid-Atlantic ridge indeed, but it is fractured and the Atlantic geometry allows for a coherent subpolar gyre across the basin with deep-convection in the western side of the gyre (the Labrador Sea). We tested opening the ridge in the north (and south, by the way) to mimic this, but concluded that it greatly complicates the interpretation of the results with little benefit for the purpose of testing mesoscale parameterizations. Hence we decided to pursue this study with no ridge. It remains optional in the namelist.

There is no mention of the dissipation scheme being used until Table 2, I recommend stating this in the model equations/setup. In NW2 we had to think at length about a viscosity scheme that could be applied in a consistent way across resolutions (ended up using biharmonic Smagorinsky). This isn’t being done here, the viscosity parameterizations in R1 are different in formulation than R4 and R16; could the authors speak more about the reasons and implications of this?

This was done deliberately, as subgrid parameterizations and their numerical schemes should ultimately be the users choice, depending on the aim of the study. Here we provide reference experiments for the DINO configuration, with the associated parameter choices. For R1 we use a less scale selective, more dissipative Laplacian viscosity operator for numerical stability and the GM parameterization, as is commonly done in models of similar horizontal resolution. NW2 is not presented for such coarse resolutions, presumably for the same reasons.

Lines 171-179: Is GM by default added to the higher resolution simulations as well, just with a lower coefficient? I would be more explicit about this. This is in itself a “parameterization” choice that may conflict with other choices the users make on top of that to test other eddy parameterization schemes. For example, in my work on backscatter parameterizations, I found that backscatter can replace the need for GM in eddy permitting simulations (and using the two simultaneously is problematic, see Yankovsky et al. 2024).

GM is deactivated entirely in R4 and R16, for the same reasons you mentioned. This should be clear from the text and we propose to clarify it in the revised version of the paper : “In eddy-permitting and eddy-resolving horizontal resolution, we assume this process to be at least partially resolved **and consequently omit the GM parameterization**.”

Line 186: Can you show a figure verifying that the tracers have reached a quasi-equilibrated state? This can be incorporated as a panel into one of the first several figures. I’m curious what is meant by “quasi” here, are the tracers in the deep ocean still evolving? How far out of equilibrium are the higher-resolution simulations? Would be helpful to visualize this in a figure as well. In the higher-resolution simulations, it would be helpful to have more discussion of what the lack of equilibration can introduce error-wise into the analysis.

We have prepared a figure for the revised version that illustrates equilibration of thermodynamic properties : a time-series of the volume of water masses defined by $\sigma_2 < 27$ and $27 < \sigma_2 < 26$. This figure is added in appendix, to illustrate what is meant by “quasi-equilibrium” : surface energetics and main ocean currents are equilibrated, yet there remains a drift in the thermodynamic properties at depth.

How do you propose accounting for the unresolved submesoscale dynamics? Is there any parameterization for those effects implemented, and how might that conflict with the mesoscale parameterization?

We did not include submesoscale parameterizations in DINO, as we aim to assess parameterizations targeting the eddy-permitting regime primarily. NEMO has a parameterization for mixed-layer eddies implemented (Fox-Kemper et al. 2008), which could be activated in the future. As this is outside the scope of this paper, we do not include dedicated experiments. We agree that this choice is arguable : submesoscale parameterizations may indeed interfere with mesoscale parameterizations. We added a sentence about this in the discussion section of the revised paper.

Might be interesting to consider referencing some of the recent work being done on the Oceananigans model in light of the more traditional modeling efforts/studies addressed here. One can make the argument that rather than layering more complex parameterization schemes on top of each other, we should instead focus on developing modeling frameworks that are able to resolve down to submesoscales through GPU-based architectures. See Silvestri et al. 2025 (**https://doi.org/10.1029/2024MS004465**)

We agree with the reviewer, and for this reason we approached Simone Silvestri and the Oceananigans team about 1 year ago. We contributed to implementing a prototype of DINO in Oceananigans (https://github.com/simone-silvestri/WenoNeverworld.jl/blob/f8539306879e357d3527fbeda51b14cf2c126c67/dino/WenoDINO.ipynb). However, these developments are not ready yet for this publication.

References:

Caneill, R., Roquet, F., Madec, G., & Nycander, J. (2022). The polar transition from alpha to beta regions set by a surface buoyancy flux inversion. *Journal of Physical Oceanography*, *52*(8), 1887-1902.

Fox-Kemper, B., Ferrari, R., & Hallberg, R. (2008). Parameterization of mixed layer eddies. Part I: Theory and diagnosis. *Journal of Physical Oceanography*, *38*(6), 1145-1165.