

Response to comments from Reviewer #1

Reviewer's comments are shown in black, and the authors' responses are provided in blue below each comment.

We thank the reviewer for their thoughtful, thorough, and constructive assessment of our manuscript. We appreciate the detailed feedback and the opportunity to improve the paper. Below we address each comment point-by-point.

We wish to clarify that the scope of this study is to document and evaluate the performance of the existing TOPAZ5 high-resolution HYCOM-CICE configuration in free-running mode over 2009-2019, rather than to present a new model sensitivity study. The hindcast contains no data assimilation, and the reported results therefore describe the intrinsic behaviour and properties of the coupled HYCOM-CICE configuration. This scope guides our responses to many of the reviewer's suggestions.

Response to Major Comments

Comment 1: *"A model evaluation paper can, in principle, be used by two research communities. One is users of the given model/configuration, to be able to refer to in future studies to show that the model is a good tool for their specific paper. In general, the manuscript does this well (comments on potentially improving this aspect are given below). However, the other community that can benefit from a model evaluation paper are those setting up their own model simulations, who can learn from the development of this given system. And this aspect is completely missing here. Yet, I doubt the PIs pulled the model out of the box, and ran just one experiment to get the results presented here. I would suspect that they ran multiple sensitivity experiments, to get the set of ocean and sea-ice parameters that they use. At least a sub-section on the steps and development would allow others to learn from the development work done here and increase the value and utility of this manuscript."*

Response: We thank the reviewer for this constructive suggestion. We agree that documenting the model development process is valuable for the community, particularly given that TOPAZ5 represents a substantial evolution from its predecessor TOPAZ4. In response, we have added a new subsection to Section 2 (Methods) titled "Model development and parameter tuning" that summarizes the key development steps and parameter choices.

The TOPAZ5 configuration was developed through a series of sensitivity experiments and parameter tuning based on prior experiences with HYCOM coupled to sea ice in high-latitude regions, including TOPAZ4 (Sakov et al., 2012; Xie et al., 2017). In the following, we present the final/optimised configuration and summarize the key tuning steps underlying its development.

1. **Vertical coordinate adjustments:** Sensitivity experiments were conducted to determine the optimal target densities for the 40 hybrid layers (range: 24.05 to 28.12 σ units), refining the configuration to better represent the full density range relevant to

the North Atlantic, Nordic Seas, and Arctic system without being explicitly tuned to any single water mass.

2. We also experimented with different depths of the upper layer and the z-level stretching factor. The 10 upper layers are set to very low density values to ensure that they are always z-levels. The top layer at 1.2 m and a stretching factor of 1.1, ensuring high vertical resolution near the surface where air-sea exchange processes are most critical. The chosen configuration provided the best balance between near-surface stratification fidelity and isopycnal water mass tracking.
3. **Mixing parameterization:** We evaluated the K-Profile Parameterization (KPP) scheme (Large et al., 1994) with the primary objective of identifying the configuration that best represents Atlantic Water properties and their pathway/extension in the study region. Initial sensitivity experiments with and without the bottom boundary layer (BBL) option indicated that the configuration with activated BBL provided a more realistic overall representation of vertical mixing and resulted in a better representation of Atlantic Water properties over and along the continental slope. Background viscosity and diffusivity were kept at their default values of $3 \times 10^{-5} \text{ m}^2/\text{s}$ and $1 \times 10^{-5} \text{ m}^2/\text{s}$ respectively, with a maximum boundary layer depth limit of 1200 m to cap unrealistically deep convection. These parameter choices provided an improved representation of near-bottom water-mass properties, including those associated with Atlantic Water, while maintaining realistic stratification throughout the study region.
4. **Sea ice rheology and dynamics:** The elastic-viscous-plastic (EVP) dynamics were implemented with sensitivity tests on the number of subcycles (80, 100, and 120 iterations), with 120 subcycles chosen to improve numerical stability of ice drift under strong wind forcing. Ridging parameters followed standard CICE defaults ($C_f = 17.0$), with ice strength computed using the Rothrock (1975) formulation. Tests with alternative ice strength formulations showed that the Rothrock approach provided the most realistic thickness distribution in the central Arctic compared to CS2SMOS observations.
5. **Albedo and radiation parameterization:** The Delta-Eddington shortwave radiation scheme implemented in CICE v5.1 allowed for systematic tuning of albedo parameters. Sensitivity experiments varying visible and near-IR albedo values were conducted, with the final configuration adopting values of 0.78 (ice visible), 0.98 (snow visible), 0.36 (ice near-IR), and 0.70 (snow near-IR) following Briegleb and Light (2007). This tuning significantly reduced the positive sea ice thickness bias in the central Arctic that was evident in earlier configurations using the simpler Bitz and Lipscomb (1999) scheme.

Value for the modeling community: We recognize that a comprehensive sensitivity analysis with full parameter exploration would be valuable, but such a study is beyond the scope of this paper, whose primary purpose is to present the TOPAZ5 configuration and evaluate its performance in a free-running mode, establishing a baseline for future data-assimilative applications. Nevertheless, we believe the documented sensitivity experiments and tuning steps

provide sufficient context for readers to understand the rationale behind the key design choices and to adapt the configuration for their own applications. The complete model configuration, workflow scripts, and parameter files are archived and publicly available (Othmani et al., 2026), enabling other researchers to conduct additional sensitivity analyses as needed.

We would like to clarify that TOPAZ5 is not a minor incremental update of TOPAZ4. The system incorporates substantial developments in model configuration, physical parameterizations, forcings, and overall system design. We recognize that the original manuscript did not sufficiently highlight these advances, and the revised version provides a much more comprehensive description of the configuration and its development.

Comment 2: *"The summary of the model and its parameters is also missing relevant information. There is nothing on mixing schemes, parameters for viscosity and diffusion, relevant sea-ice variables such as ice strength, albedo, drag coefficients, etc. If these are all the same as the previous version of TOPAZ, this needs to be stated. But at the very least a table with all the relevant information about the model specifics is needed. As well, does the model include tidal forcing? Icebergs? All these details are needed."*

Response: We thank the reviewer for this suggestion. We agree that a comprehensive summary of model parameters is essential for reproducibility. While some parameter details were present in the original manuscript, we have consolidated them into a single table following GMD standards. We have also clarified which parameters follow HYCOM-CICE defaults and which have been tuned for TOPAZ5.

In response, we have added Table 1: TOPAZ5 model configuration parameters to Section 2, covering ocean model, mixing, sea ice, and forcings. The table includes parameter values and references, with clear distinction between default and tuned parameters. Tidal forcing, icebergs, and Stokes drift are not included in the present configuration.

Table 1: TOPAZ5 model configuration parameters

Component	Parameter	Value	Note
Ocean Model	Model version	HYCOM v2.2.98	Bleck (2002)
	Vertical coordinate	Hybrid (z-isopycnal)	10 upper z-levels, 40 isopycnal
	Vertical layers	50	Top layer: 1.2 m
	Target density range	0.1 – 28.12 σ units	Tuned
	Horizontal resolution	6.25 km (polar stereographic)	Mesoscale-resolving

	Scalar advection	FCT2	Iskandarani et al. (2005); Zalesak (1979)
	Momentum advection	2nd order	HYCOM default
	Lateral boundary	No-slip	HYCOM default
	Laplacian viscosity factor	0.04	HYCOM default
	Laplacian momentum diffusivity	0.00286 m ² /s	Tuned coefficient (variable)
	Bi-harmonic thickness diffusion velocity	-0.01 m/s	Tuned coefficient (variable)
	Laplacian T/S diffusivity	0.005 m ² /s	HYCOM default
	Bottom friction	2×10 ⁻³	HYCOM default
	Vertical remapping	WENO piecewise parabolic	HYCOM default
Mixing	Scheme	KPP	Large et al. (1994)
	Background viscosity	3×10 ⁻⁵ m ² /s	HYCOM default
	Background diffusivity	1×10 ⁻⁵ m ² /s	HYCOM default
	Max boundary layer depth	1200 m	HYCOM default
	Bottom boundary layer	Enabled	Tuned
	Shortwave radiation	Jerlov water type	Kara et al. (2005)

Sea Ice	Model version	CICE v5.1	Hunke et al. (2015)
	Thickness categories	5	CICE default
	Dynamics	EVP, 120 subcycles	Hunke and Dukowicz (1997)
	Ice strength	Rothrock (1975)	CICE default
	Ridging parameter (Cf)	17.0	CICE default
	Ice-ocean drag	0.00536	CICE default
	Ice-atmosphere drag	0.00132	CICE default
	Thermodynamics	Bitz and Lipscomb (1999)	CICE default
	Brine drainage	Rapid + slow modes	CICE default
	Salinity	Simulated in ice	CICE default
	Shortwave radiation	Delta-Eddington	CICE default
	Visible albedo (ice/snow)	0.78 / 0.98	CICE default, tuned
	Near-IR albedo (ice/snow)	0.36 / 0.70	CICE default, tuned
	Melt ponds	Level ice scheme	CICE default

	Freezing temperature	Linear salt formulation	CICE default
Forcing			
	Atmosphere	ERA5, 6-hourly	Hersbach et al. (2020)
	Flux algorithm	COARE 3.0	Fairall et al. (2003)
	Greenland melt	Mass loss estimates	Groh et al. (2019)

We have added the following text to accompany the table:

"The key configuration parameters are summarized in Table 1. Most parameters follow HYCOM and CICE defaults; values indicated as 'tuned' resulted from sensitivity experiments. Tidal forcing, icebergs, and Stokes drift are not included in the present configuration. Iceberg freshwater flux is not represented separately from river runoff and Greenland Ice Sheet mass loss contributions."

Comment 3: *"In terms of looking at the exchange out of the Arctic, the authors only look at Fram Strait. Yet, one question for models is how well they partition the Arctic transport each side of Greenland, which can significantly impact the sub-polar North Atlantic. I think it would be good to add a comparison section west of Greenland, such as Davis Strait."*

Response: We agree that Davis Strait is an important gateway for Arctic freshwater and volume exchange, complementing the Fram Strait. We have added an analysis of the Davis Strait to the transports section (Section 3.6). This includes:

- Net volume transport through Davis Strait (67°N, 50-64°W)
- Freshwater transport estimates (relative to 34.8 reference salinity)
- Comparison to observational estimates (Curry et al., 2014; Rabe et al., 2014)

Comment 4: *"I think the statement about good representation of deep water formation based on the MLDs is a bit simplified. It looks like the model has too deep deep convection, occurring over too broad a region – which is common for models of this resolution. Some more discussion of the implications of those biases would help. Plotting maximum MLD might also help with*

the evaluation and show if the model has convection to the bottom in some regions like the Labrador Sea."

Response: We thank the reviewer for this important point. We have revised Section 3.5 to provide a more nuanced discussion of MLD biases and their implications. Specifically:

1. We have modified the figure showing maximum MLD.
2. We have quantified the over deepening and discussed implications:
 - Too-deep convection may artificially ventilate the intermediate/deep ocean, affecting water mass properties and potentially biasing the representation of Labrador Sea Water formation.
 - Overly broad convection regions could bias the AMOC representation by overestimating the volume of dense water formed in the subpolar gyre. This is a known common bias in non-eddy-resolving models.
 - The KPP mixing scheme with maximum boundary layer depth of 1200 m may be contributing to overdeepening.
3. We have noted that the model produces MLDs exceeding 1000 m in the Irminger Sea and 500-600 m in the Greenland Sea, whereas observations suggest maximum MLDs of approximately 500-700 m in the Irminger and 400-500 m in the Greenland Sea.
4. The discussion now acknowledges that while the spatial pattern is credible, the intensity and extent of convection need improvement – a limitation that will be addressed in future development through refined vertical mixing parameterizations and potentially enhanced horizontal resolution.

Comment 5: Freshwater transport

"Why not freshwater transport as well as volume transport to help evaluate the properties of the water carried through the studied sections? As above, would like to see a section west of Greenland. Also, for Bering Strait, the authors imply a close comparison with the 0.8 Sv of Woodgate et al. Yet the Woodgate et al. papers show an increasing trend that isn't discussed."

Response: We agree that freshwater transport is an informative diagnostic. We have therefore added this analysis and expanded the discussion of freshwater exchanges in the revised manuscript. For the Bering Strait, we also expanded the discussion to compare the simulated trend with the observed trends reported in Woodgate and Peralta-Ferriz (2021).

Comment 6: *"The authors state credible AMOC dynamics. Yet no estimates of the model AMOC is presented. It would be good to see the modelling overturning in depth and density space along the OSNAP section, to compare with those estimates."*

Response: We agree that this statement should be supported more explicitly. However, we respectfully note that a comprehensive AMOC analysis along the OSNAP section would constitute a substantial additional study on its own. To avoid overstating our conclusions, we have revised the corresponding text and moderated the statement regarding "credible AMOC dynamics." The revised text reads:

"The model simulates deep convection in the Labrador, Irminger and Greenland Seas, suggesting that the physical processes associated with dense water formation, surface cooling, wind-driven mixing and lateral exchange, are represented at a credible level. However, the overdeepening of the mixed layer in some regions indicates that the precise intensity and spatial extent of convection require further refinement. Full AMOC diagnostics are beyond the scope of this evaluation."

Comment 7: *"Something more on the deeper circulation, such as Atlantic Water circulation within the Arctic Ocean, and/or the overflows from the Nordic Seas would be useful evaluation."*

Response: We agree that these are important components of Arctic Ocean dynamics. We have added T and S maps at 300 m against the WOA climatology to further assess the model's ability to represent subsurface properties of the Arctic.

Comment 8: *"Is it more appropriate to discuss this paper as a model validation, or a model evaluation? I feel this manuscript is more focused on an observational evaluation, rather than a detailed validation of the code and numerical schemes."*

Response: We agree that "model evaluation" is more appropriate and have consistently used this term throughout the revised manuscript. We have clarified in the introduction that this is an evaluation of the model's physical performance, not a technical code validation.

Comment 9: *"L35: Not sure what 'use in their majority advanced...' is trying to say."*

Response: We have rephrased this sentence for clarity. The revised text reads:

"Several regional models have been validated for the Nordic Seas and Arctic, with most employing advanced z-level vertical coordinate models such as the MITgcm (Nguyen et al., 2021), NEMO (Hordoir et al., 2019) and FESOM (Wang et al., 2018). However, fewer studies have evaluated the performance of isopycnic and hybrid vertical coordinate models in these regions. The use of isopycnic coordinates is advantageous for ensemble data assimilation (Counillon et al., 2016), which again favours coupled ice-ocean data assimilation (Sakov et al., 2012) and coupled physical-biogeochemical modelling, both important features of Arctic Ocean forecast and reanalysis products."

Comment 10: *"Given the comment 'horizontal resolution varying from 6.4 km at the Pacific boundary to 10.2 km at the Atlantic boundary', might it be worth adding a panel to figure 1 showing a spatial map of the model resolution?"*

Response: This is a useful suggestion. We have added a panel to Figure 1 showing the horizontal grid spacing across the model domain.

Comment 11: *"The model uses ERA5 forcing. Yet ERA5 is known to have a warm bias in the Arctic as well as having issues like occasional extremely strong and unrealistic wind events. Are any corrections applied to the ERA5 forcing?"*

Response: No corrections were applied to the ERA5 forcing in this free-running simulation. The model uses ERA5 fields directly as provided by ECMWF. We have expanded the discussion of the known Arctic biases in ERA5.

Comment 12: *"L97: The authors mention estuaries along the Greenland coast. Do they mean fjords? In any case, I think it would be good to provide more detail about how this freshwater is distributed? For example, is any of it moved to the shelf for fjord systems that are not well resolved?"*

Response: We thank the reviewer for noting this ambiguity. We have clarified that "estuaries" refers to the points of freshwater discharge along the Greenland coast, which are primarily fjords. The freshwater is indeed distributed into an elliptic area that stretches outside of the fjords.

Comment 13: Spin-up length justification

"Why was the length of the spin-up decided to be the first 17 years? Any objective measure used to decide on that length?"

Response: No objective measure was used to decide this length, but from experience with various HYCOM configurations in this region, when initialized from climatology the model takes about this time to stabilize. The length of the spin up was decided based on visual inspection rather than objective measures.

Comment 14: SST bias and sea ice

"Is the summer SST bias related to sea-ice biases?"

Response: We thank the reviewer for this important question. Our analysis does not indicate a direct relationship between the summer SST bias and sea-ice biases. The summer SST bias is located far from the sea-ice region, and its spatial pattern appears more consistent with mixed-layer depth biases (Ali et al., 2019). While we cannot exclude the possibility of indirect

connections, the available evidence does not support sea-ice bias as the primary driver of the summer SST bias. We have therefore framed this discussion in terms of consistency rather than attribution.

Regionally, the warm SST bias in the Labrador Sea and Norwegian Sea occurs in ice-free regions and appears primarily related to atmospheric forcing and mixing errors. In contrast, the cold bias around the marginal ice zone (MIZ) appears to be associated with sea-ice extent errors: where the model has excessive ice cover, cold SST biases of up to -2°C are simulated. In the Barents Sea, the warm SST bias ($0.5\text{--}2^{\circ}\text{C}$) may partly reflect an underrepresentation of ice-edge cooling due to delayed ice advance or early retreat. This warm bias is likely influenced by a combination of factors, including the ERA5 warm bias in the Arctic (Graham et al., 2019), an underestimation of sea-ice extent (allowing greater solar radiation absorption), and insufficient vertical mixing bringing cold water to the surface. The model's sea-ice thermodynamics (Bitz and Lipscomb, 1999) with the Delta-Eddington radiation scheme may also affect these biases through the albedo parameterization and melt pond representation. We have added these points to the revised manuscript.

Comment 15: *"Looking at the large SSS biases along the Arctic shelves, it looks as if there might be a river runoff issue?"*

Response: We thank the reviewer for this observation. We agree that river runoff is a possible contributing factor to the large SSS biases along the Arctic shelves, although it is likely not the only one. In our simulations, river discharge is prescribed from a climatology, which does not capture interannual variability, timing, or fine-scale spatial distribution; this may contribute to the biases. We plan to replace this forcing with GloFASv4 (Grimaldi et al., 2026) in future work. However, our assessment suggests that the surface salinity issue may be more strongly related to the model, possibly associated with mixing and stratification errors. We have expanded the discussion of the salinity biases along the Arctic shelves in the revised manuscript, considering both river runoff and model mixing/stratification as plausible contributors. We cannot currently quantify their relative contributions.

Comment 16: *"For the section figures, they use discrete color intervals, which I feel enhances the figure and makes them easier to follow. Yet the spatial contour plots use continuous color intervals, which blend together and are harder to read. I would suggest changing all of them to discrete color intervals as well."*

Response: We appreciate the suggestion and have revised the figures to improve consistency and readability. All spatial contour plots now use discrete color intervals consistent with the section figures, making the features more clearly distinguishable and facilitating comparison across panels.

Comment 17: *"Figure 4: The fonts are too small (comparing with figure 8 for example), and the lines not thick enough."*

Response: We have increased font sizes and line thicknesses in Figure 4 to improve readability and ensure consistency with Figure 8.

Comment 18: *"L296: The text states 'large-scale altimetry pattern with high fidelity'. Is that a surprise if GLORYS is assimilating altimetric fields?"*

Response: This is a fair point. We have revised the text to clarify the interpretation:

"GLORYS reproduces the altimetry pattern, as expected given its assimilation of SLA observations. TOPAZ5, without such constraints, still captures the large-scale structure but underestimates the magnitude of positive trends, particularly in the Lofoten Basin and Barents Sea. This demonstrates the model's intrinsic skill (from dynamics alone) versus the benefit of assimilation."

This revision avoids overstating the significance of agreement with an assimilative product and better characterizes the comparison.

Comment 19: *"Figure 8: Font size is good but lines could be thicker."*

Response: We have increased line thicknesses in Figure 8.

Comment 20: *"L344: Given the comment about the limitations with small scale eddies - did the PIs want to then consider some eddy parameterization?"*

Response: This is an excellent point. We have expanded the discussion of unresolved mesoscale variability and potential future developments:

- TOPAZ5 does not currently use explicit eddy parameterization (e.g., Gent-McWilliams)
- Given the 6-10 km resolution, the model resolves the largest mesoscale eddies but misses smaller scales, particularly in the Lofoten Basin where the Rossby radius is approximately 4-6 km
- In the Arctic region, the first baroclinic Rossby radius of deformation is generally less than 5 km, so the 6-10 km resolution is eddy-permitting but not fully eddy-resolving
- We note that the operational version benefits from data assimilation, which helps correct eddy-related biases
- The developed TOPAZ6 version has a finer spatial resolution specifically to better resolve these processes.

- A version of HYCOM includes a 4th order momentum advection scheme, which is expected to improve the effective model resolution even when not fully eddy-resolving (Backeberg et al 2009).

We thank the reviewer again for the thoughtful and constructive comments. We believe the proposed revisions will substantially strengthen the manuscript and improve its usefulness to both TOPAZ users and the wider modelling community.

References (added to the revised manuscript):

Ali, A., Christensen, K. H., Breivik, Ø., Malila, M., Raj, R. P., Bertino, L., Chassignet, E. P., and Bakhoday-Paskyabi, M.: A comparison of Langmuir turbulence parameterizations and key wave effects in a numerical model of the North Atlantic and Arctic Oceans, *Ocean Model.*, 137, 76–97, <https://doi.org/10.1016/j.ocemod.2019.02.005>, 2019.

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Grimaldi, S., Salamon, P., Disperati, J., Zsoter, E., Russo, C., Ramos, A., Carton, C., Barnard, C., Hansford, E., Gomes, G., and Prudhomme, C.: GloFAS v4.0 hydrological reanalysis, European Commission, Joint Research Centre [dataset], <https://doi.org/10.2905/f96b7a19-0133-4105-a879-0536991ca9c5>, 2026.

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