

Reviewer 2

This manuscript can be viewed as a continuation of two previous papers by the same group Matthes et al. (2020) and Martin and Biastoch (2023). The main novelty lies in the use of a new atmospheric model, two different ocean resolutions, and an improved coupling strategy. The authors quantify the impact of ocean resolution (within a regional refinement) on the mean state, the AMOC, and the northward heat transport in the North Atlantic but the analyse of the atmospheric contribution and the explanation of the physical mechanisms is weak.

Overall, I believe that this work could be published in Geoscientific Model Development (GMD), but only after major revisions. My main comments are detailed below.

The authors sincerely thank the reviewer for the careful evaluation of our manuscript and for the constructive and insightful comments. The reviewer's critical questions and suggestions have been extremely valuable in improving both the quality and clarity of the manuscript. We believe that these revisions have substantially strengthened the manuscript, and we are grateful for the time and effort the reviewer invested in providing such thoughtful and detailed feedback. Point-by-point responses are given below.

A) Technical aspects

The use of a new atmospheric model together with a new coupling interface clearly falls within the scope of GMD. However, several important technical aspects require a more detailed description.

1) Model configurations

Although the OpenIFS and NEMO configurations have been described in previous publications, I find the description of configurations in the present manuscript far too brief.

- A summary of the main physical parameterizations used in the atmospheric model would be welcome.

Ans: Thanks for the suggestion. The below paragraph has been added to the revised manuscript.

"The OpenIFS model includes parametrisations for several physical processes including the ecRad radiation model (Hogan and Bozzo, 2018), bulk convection (Bechtold et al., 2008, 2014) , and cloud microphysics with prognostic cloud properties and precipitation (Tiedtke, 1993; Forbes and Tompkins, 2011). Surface fluxes of momentum, heat and freshwater are based on roughness length scales from Monin-Obukhov similarity theory (ECMWF, 2017)."

- For the ocean model, Eqs. (2) and (3) suggest that viscosity is represented using a biharmonic operator whereas diffusion uses a harmonic operator. This should be stated explicitly. It should also be specified whether these operators are applied horizontally or along isopycnal surfaces.

Ans: Thank you for the suggestion.

We totally agree with your point. This is now explicitly stated as:

"The horizontal viscosity is biharmonic and applied horizontally while tracer diffusion is harmonic and iso-pycnal on both the ORCA05 and VIKING10 grids."

- The mesh refinement factor within the AGRIF zoom is five, whereas the time-step refinement is only three (instead of five). How do the authors explain that such a large time step remains numerically stable? Have they observed any numerical noise within the refined region, for example near the ocean bottom or along the Gulf Stream?

Ans: Thank you for the question.

The time step is chosen to avoid CFL-violations and noise. We found neither of these when using a 3x time step refinement in VIKING10 so it seems that the refinements in the horizontal and temporal domains must not match at all times. A similar conclusion was found for INALT

grids (Schwarzkopf et al., 2019). The lower time refinement factor allows for computationally cheaper and hence longer simulations. We now mention this briefly in the main text.

- Matthes et al. (2020) report using a Gent-McWilliams coefficient of $2000 \text{ m}^2 \text{ s}^{-1}$, whereas the present manuscript states that $1000 \text{ m}^2 \text{ s}^{-1}$ was used (as in Martin and Biastoch, 2023). Which value was actually used in this study?

Ans: Thank you for your question.

We use a cap of $1000 \text{ m}^2 \text{ s}^{-1}$. After testing we found that the eddy-induced diffusivity very rarely exceeds $1000 \text{ m}^2 \text{ s}^{-1}$ so the choice has no impact on the results.

2) Coupling interface

The description of the coupling interface is also insufficient.

At the very least, the manuscript should list all variables exchanged between the atmosphere and the ocean, especially in the presence of sea ice. How is the coupling with sea ice performed? For example, is sea-ice thermodynamics computed within the ocean or the atmospheric model? Which strategy is used to ensure flux conservation in partially ice-covered regions where the ice fraction evolves during a coupling time step?

Ans: Thank you for pointing this out.

We agree that this should be explained better. A table listing all coupling fields and their methods is now included in the manuscript. The coupling with sea ice is also explained in more detail. We "circumvent" the simplistic ice thermodynamics in OpenIFS by replacing the ice surface temperature in OpenIFS by that from LIM2 (Arduini et al., 2022). A similar method is employed by ECMWF operationally.

Lines 161–162: "... fields to the OASIS coupler once per model time step and the fields are then sent from OASIS to both NEMO and AGRIF."

This wording is misleading and could suggest that OASIS is run as a standalone executable, as was the case for versions earlier than OASIS3-MCT. In reality, OpenIFS accumulates coupling fields at every model time step through the OASIS library without MPI communication. MPI exchanges between OpenIFS and NEMO, mediated by OASIS, only occur at the coupling frequency, not at every model time step.

Ans: Thank you for pointing this out.

We agree that this is indeed confusing and have now re-phrased the text to hopefully make this clearer. The manuscript is revised as:

"Coupling fields are sent from OpenIFS to NEMO and AGRIF via the OASIS coupler once per coupling time step. Hence, the surface fluxes sent from OpenIFS to NEMO and AGRIF are identical when sent but they are different when received due to differences in interpolation to the two different ocean grids. The global fluxes of heat and freshwater are globally conserved by OASIS, i.e. the interpolation may change a flux locally but the global integral remains unchanged. We do not apply conservation for fluxes over sea ice which means that e.g. solar heat flux over ocean and ice separately are not globally conserved while the total solar heat flux is. Note that fluxes sent to the AGRIF grid, VIKING10, are never conserved as it is not a global grid."

3) Interpolation schemes

The authors use interpolation schemes that are rather unusual, as they preserve neither integral quantities nor derivatives.

- As a consequence, heat and freshwater fluxes (evaporation minus precipitation) are not strictly conserved across the air-sea interface. Have the authors evaluated the resulting losses of mass

and energy through the coupling interface? For example, comparing maps of the fluxes computed on the atmospheric and ocean grids would allow the geographical distribution of positive and negative interpolation errors to be quantified.

Ans: Thank you for your comment.

We hope that the new table after comment 2 helps a bit here. Our interpolation schemes are similar to those used by the EC-Earth coupled model which also uses IFS + NEMO. In general, GAUSWGT proved the most stable for atm-to-ocean remapping and BILINEAR the most stable for ocean-to-atm remapping. We ensure conservation of global integrals of heat and freshwater fluxes in OASIS although this does not ensure local conservation or derivatives.

A technical report documents that using GAUSWGT with 15 neighbours and 0.1 variance was optimal for TCO319-VIKING10 remapping, see Maisonnave (2021): How to interpolate atmosphere fluxes to an AGRIF zoom? The ECHAM & OpenIFS test case (in French) , Technical Report, TR/CMGC/21/200, CECI, UMR CERFACS/CNRS No5318, France.

- Likewise, the divergence and, more importantly, the curl of the wind stress, which play a critical role in driving the ocean circulation, may be significantly degraded by the interpolation method used in this study. Have the authors compared the wind stress curl over the North Atlantic computed on the atmospheric and ocean grids?

Ans: Thank you very much for your suggestion.

We have computed wind stress curl on both the atmosphere and ocean grids and they are very similar. We now state this in the manuscript.

B) Scientific results

1) Atmospheric analysis

Most of the analyses focus on the ocean, although the simulations are fully coupled. The manuscript lacks a thorough analysis of the atmospheric response. For example lower atm temperature bias, e.g. at 850hPa compared to ERA5 could be added to Fig 2. The comparison between LH and HH should clearly identify the atmospheric processes responsible for the differences between these two simulations, which differ only in the resolution of their atmospheric component.

Ans: Thank you for the suggestion.

Figure 2 is modified by including the 850 hPa temperature bias compared to ERA5. The discussion related to the Figure has also been modified in the revised manuscript as:

"The spatial pattern of the atmospheric temperature bias at 850 hPa closely resembles that of the SST bias, except over the high-latitude oceans, where the SST is primarily influenced by the simulated sea-ice distribution. Increasing the ocean resolution reduces the cold bias in the lower-tropospheric temperature but leads to a more pronounced warm bias over the subpolar North Atlantic and the Arctic Ocean. When the higher-resolution atmosphere is combined with the higher-resolution ocean, the overall temperature bias is further reduced, resulting in the best agreement with the reference data among the three configurations."

On several occasions, the authors speculate about the underlying mechanisms even though the atmospheric model output could be used to test these hypotheses directly:

- Line 234: the differences are "likely" attributed to cloud radiative forcing. Why not simply show the cloud radiative forcing? Alternatively, analyze separately the upward and downward radiative components, or the shortwave and longwave fluxes at the top and bottom of the

atmosphere.

Ans: Thank you for the suggestion. We have analyzed the cloud radiative forcing and found no evidence to prove our hypothesis. The manuscript has been revised as:

"We initially hypothesized that the noticeably cooler atmospheric temperatures in the HH configuration (Fig. 3b) were associated with differences in cloud radiative forcing. However, after carrying out additional analyses to test this hypothesis, we did not find sufficient evidence to support this explanation. The underlying cause of the cooler atmospheric temperatures in the HH configuration therefore remains unresolved and needs further investigation."

- Line 241: the authors state that they "speculate". Why not plot the polar jet and verify whether this hypothesis is correct?

Ans: Thank you for the suggestion.

We apologize for not having phrased this sentences carefully. The main message is that temperature changes are due to a displacement of the tropopause. We speculate about the positioning of the tropopause, because it is not part of the diagnostics saved. Changes in the polar jet strength are a logical consequence thereof and we observe a weaker jet at high altitudes in HH compared to LH. The respective sentence has been rephrased accordingly. The manuscript revised as:

"This upper tropospheric warming north of 50°N and at about 200 hPa is very likely caused by a lowering of the tropopause resulting from a vastly colder troposphere in HH, which has implications on the strength of the polar jet being weaker in HH than in LH at high altitudes."

- Figure 9: the net surface heat flux combines several different physical processes into a single quantity. It would be much more informative to present the turbulent heat fluxes and the net radiative fluxes separately. Furthermore, separating shortwave and longwave radiation would greatly help the interpretation, particularly over sea-ice regions where albedo changes play an important role.

Ans: Thank you for the suggestion.

We fully agree with the reviewer that separating the turbulent and radiative components of the surface heat flux provides valuable additional insight. Accordingly, we have prepared the suggested analysis, and the corresponding figure is included in our response.

The results show that the spatial patterns of the surface net heat flux and its differences closely resemble those of the turbulent heat flux, indicating that the turbulent heat flux is the dominant contributor to the net surface heat flux changes. In contrast, the radiative component contributes comparatively little to the overall signal. Accordingly, we have replaced the net surface heat flux with the turbulent heat flux in Fig. 9 and moved the original net heat flux figure to the Appendix of the revised manuscript. The corresponding text has also been revised to reflect this change. The decomposed heat flux analysis is provided here for completeness.

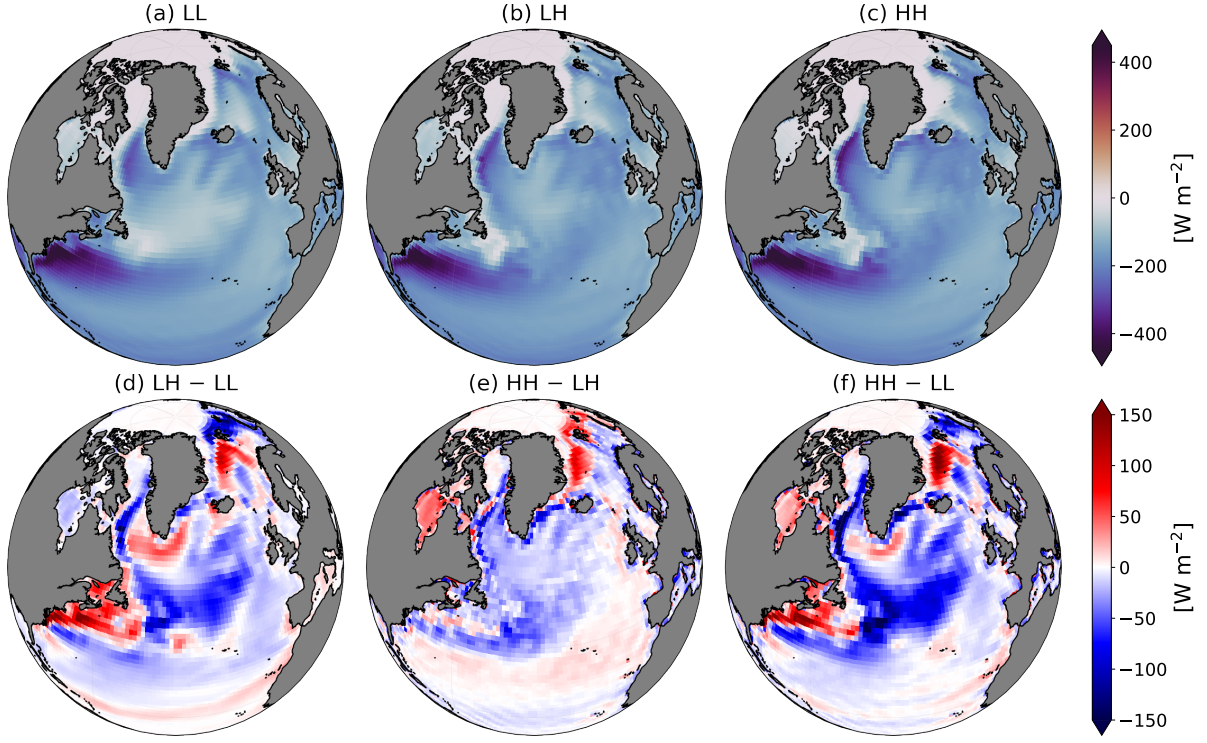


Figure 1: (a–c) DJF mean turbulent heat flux (W/m^2) (negative is ocean heat loss), (d–f) difference in DJF turbulent heat flux (W/m^2). The DJF means are computed over simulations years 51–100.

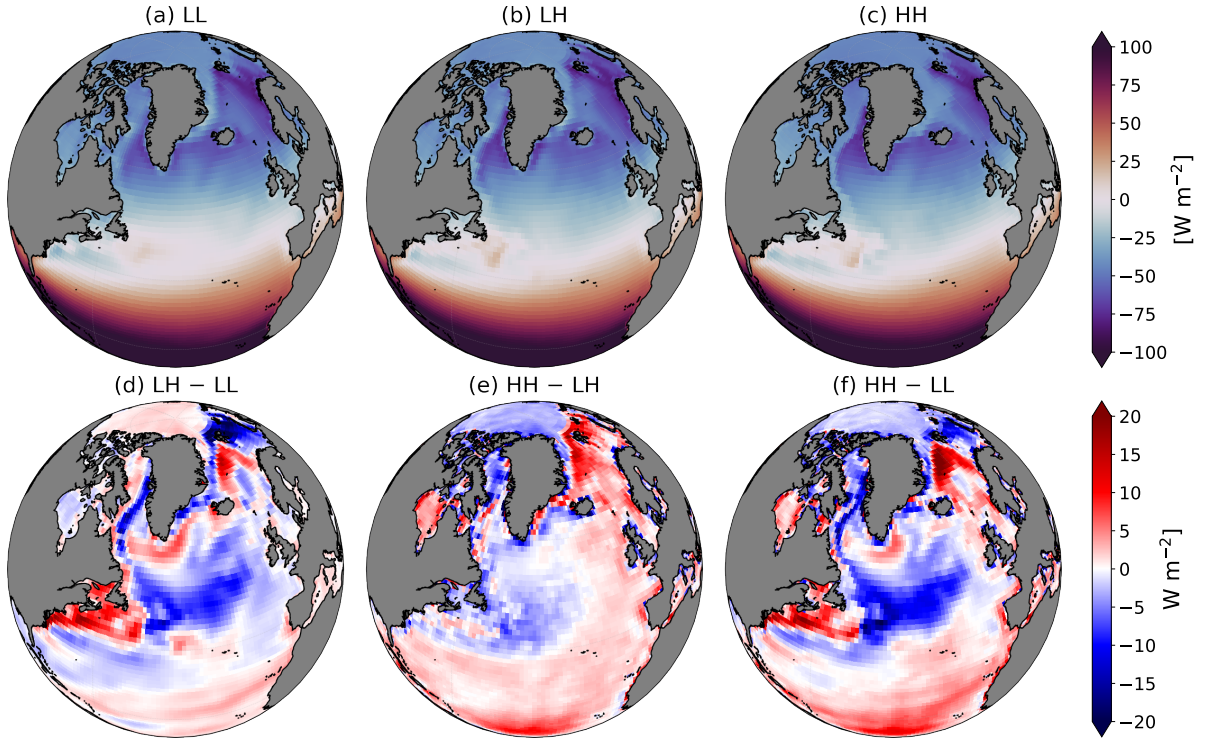


Figure 2: (a–c) DJF mean radiative heat flux (W/m^2) (negative is ocean heat loss), (d–f) difference in DJF radiative heat flux (W/m^2). The DJF means are computed over simulations years 51–100.

2) Initialization and model drift

Unless I missed it, I could not find any description of how the ocean initial state was generated. Was it taken from a long coupled simulation, as in Martin and Biastoch (2023)? From the simulations of Kjellsson et al. (2020)? Or from an observational climatology such as the World Ocean Atlas? In any case, the simulations presented here are probably too short to robustly evaluate the deep ocean circulation. The model is most likely still undergoing a significant adjustment phase (see, for example, Griffies et al., 2025). I fully understand that the authors may not have had sufficient computational resources to perform a much longer spin-up. However, this issue cannot simply be dismissed by the brief statement given in lines 195–197.

Ans: We agree that this was not well explained in the manuscript and hope that it is now clearer. All experiments start from observations or reanalysis in January 1950, i.e. EN4 for the ocean state and ERA-5 for the atmosphere. Our simulations follow the HighResMIP protocol where high-resolution models start from initial conditions representative for 1950 and use a spin up period of a few decades, usually 30-50 years. These points are now added to the "Simulations" section.

As stated by the authors, no retuning was performed for the HH simulation, while the top-of-atmosphere radiative balance differs from the other simulations (line 236). This strongly suggests that the HH experiment is not in radiative equilibrium and is therefore drifting, potentially in a different manner from LL and LH. Under these circumstances, it is difficult to support the statement that there is a "common drift in all simulations" (line 197).

At the very least, the manuscript should present the top-of-atmosphere radiation budget for each simulation together with time series of several key diagnostics, for example the mean atmospheric temperature between 500 and 100 hPa north of 60°N, or the mean ocean temperature between 500 and 1000 m south of 40°N.

Ans: Thank you for pointing out a very important issue.

We agree with the fact that the deep ocean circulation may be still undergoing an adjustment and it can not be stabilised within a 100 year model simulation. To support our statement about the common drift in all the simulations, we have prepared a time series figure with several key diagnostics such as TOA radiation budget, the mean atmospheric temperature between 500 and 1000 hPa north of 60°N, the mean ocean temperature between 500 and 1000 m south of 40°N and the sea-ice area and sea-ice volume. These analyses indicate that the long-term model drift is comparable among the three configurations over years 51–100.

The HH configuration exhibits a sudden change in the TOA radiation budget. This discontinuity resulted from a technical issue encountered during the simulation. To evaluate its potential impact on our results, we repeated the key analyses after excluding the final 22 years of the HH simulation. The results remain qualitatively and quantitatively very similar to those obtained using the full 50-year analysis period.

Furthermore, we find that this discontinuity does not affect the evolution of the atmospheric and ocean temperatures, sea-ice distribution, and the AMOC which evolve smoothly across the period of the TOA radiation change, with no discernible shift associated with the technical issue.

Taken together, these sensitivity analyses demonstrate that our main conclusions are robust and are not affected by the technical discontinuity. We are therefore confident that using the complete 50-year analysis period for the intercomparison is justified. To support this conclusion, we have included the relevant figures in the response.

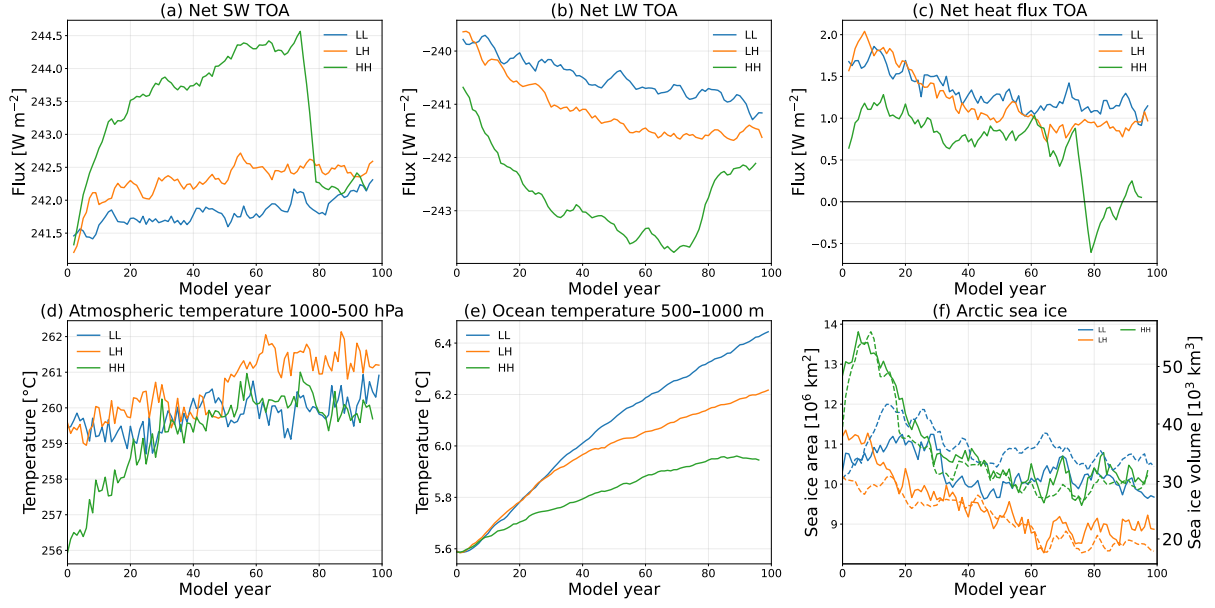


Figure 3: Time series of globally averaged (a) Top-of-atmosphere shortwave radiation (W/m^2), (b) Top-of-atmosphere longwave radiation (W/m^2), (c) Top-of-atmosphere net heat flux (W/m^2), (d) mean atmospheric temperature between 500 and 1000 hPa north of 60°N , (e) mean ocean temperature between 500 and 1000 m south of 40°N , (f) Arctic sea ice area (10^6 km^2) (solid lines) and sea ice volumes (10^3 km^3) (dashed line).

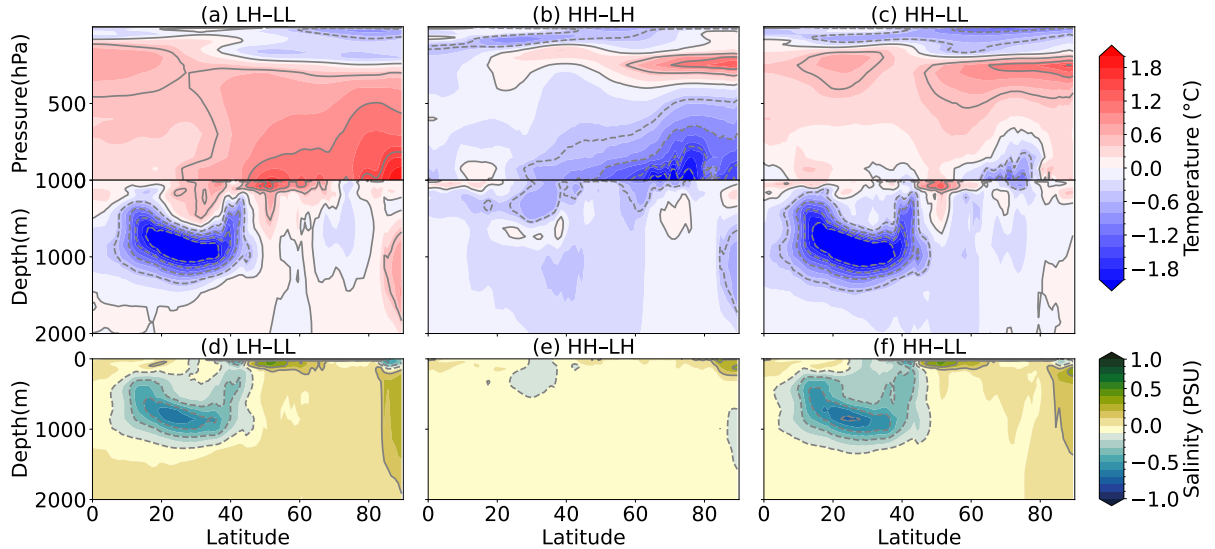


Figure 4: Annual-mean differences in North Atlantic region zonal mean (a-c) air temperature ($^{\circ}\text{C}$) as well as ocean potential temperature and (d-f) salinity (PSU) between the three model configurations. In the atmosphere the region is limited to 60°W – 0° . The differences are computed over simulations years 47–76.

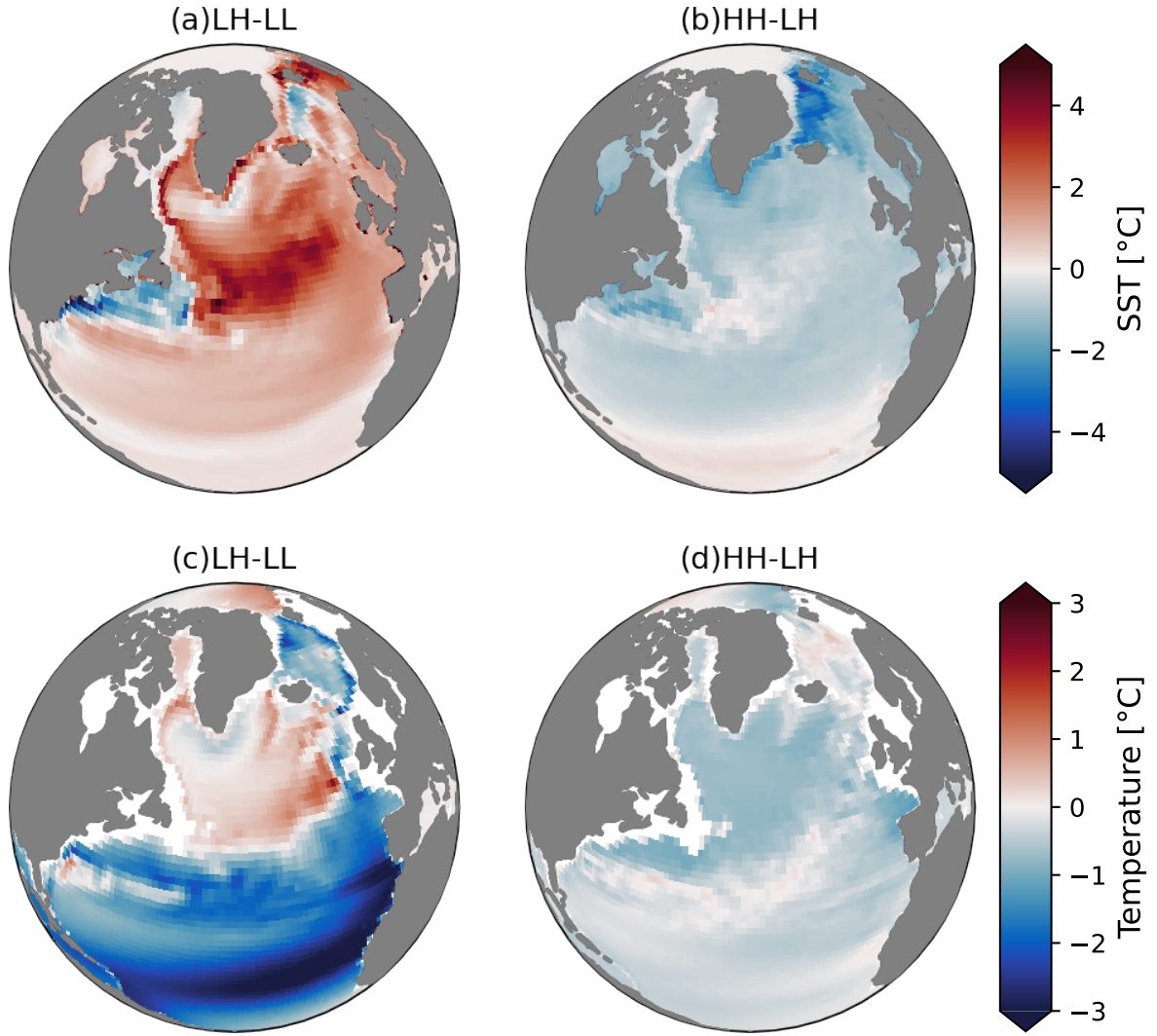


Figure 5: Annual mean differences in (a-b) SST ($^{\circ}\text{C}$), and (c-d) 500–1000 m mean potential temperatures ($^{\circ}\text{C}$) between the model configurations, (a, c) LH-LL and (b, d) HH-LH. The differences are computed over simulations years 47–76.

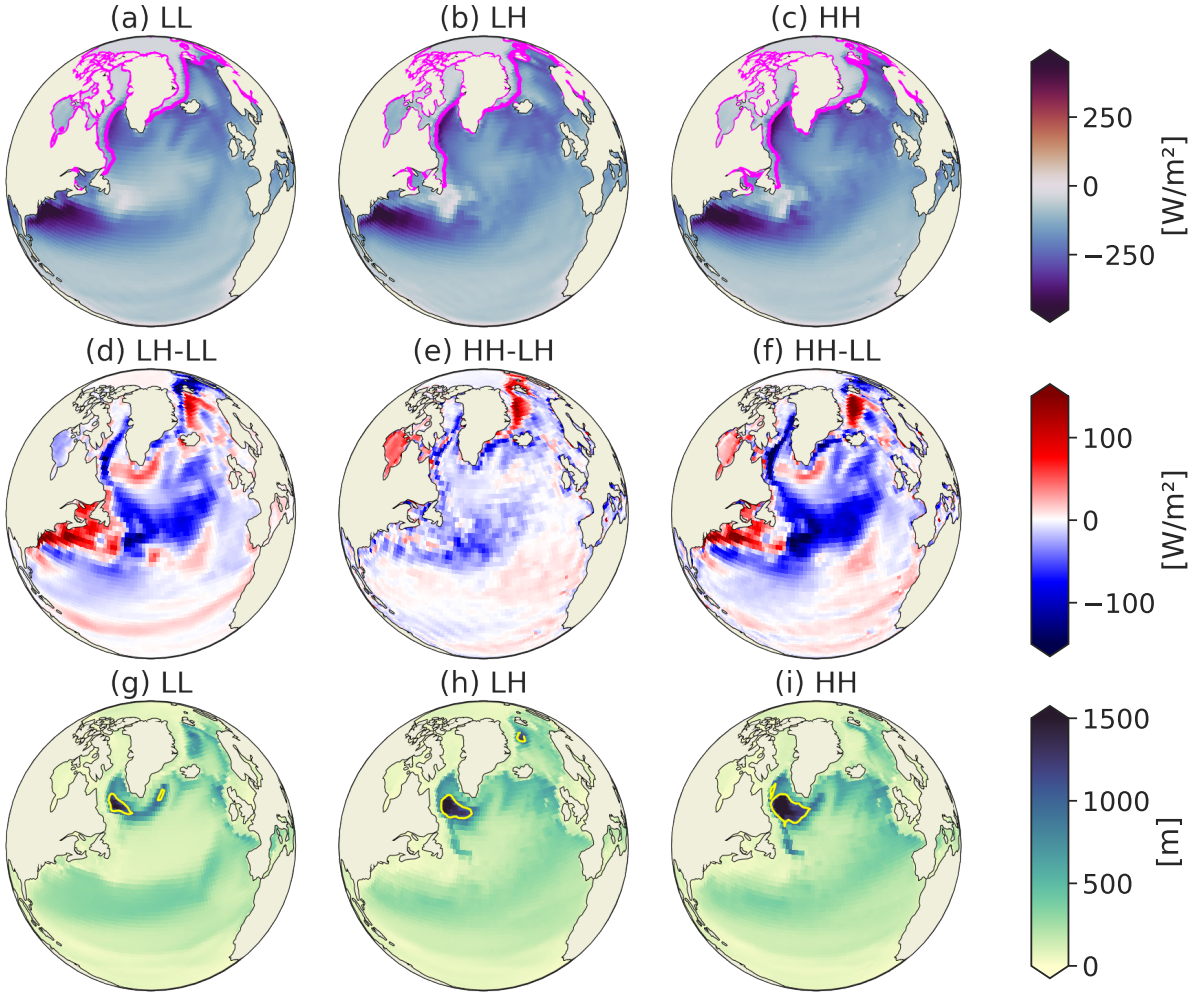


Figure 6: (a–c) DJF mean turbulent heat flux (W/m^2) (negative is ocean heat loss), (d–f) difference in DJF turbulent heat flux (W/m^2), (g–i) March mean MLD (m). The magenta contours show the 15% sea-ice concentration and yellow line shows the center of deep convection where mean MLD exceed 1000 m. The DJF means are computed over simulations years 47–76.

3) Mediterranean Outflow

As stated in the manuscript, the width of the Gibraltar Strait in ORCA05 is fixed to the observed value (20 km). Figure 5 appears to suggest that the Gibraltar Strait is unrealistically deep in ORCA05. This is not correct. Its depth (284 m, if I am not mistaken) is also constrained to the observed value thanks to partial bottom cells, meaning that the geometry of the strait is already realistic even in ORCA05. Although the LH bathymetry is more detailed, the effective cross-sectional area of the strait—which primarily controls the volume transport—should remain very similar in LL and LH. Moreover, the lateral friction coefficient can also be adjusted to reproduce the observed Mediterranean outflow. Therefore, the lower resolution of ORCA05 alone cannot explain the "unrealistically large outflow." The results shown here appear to reflect model (wrong) tuning at a given resolution rather than a genuine resolution effect.

Ans: Thank you so much for your comment. We have reconsidered our argument and rephrased the respective paragraph to now read as below. We hope this clarifies the misunderstanding of strait geometry addressed in your question.

The discussion related to the Mediterranean Outflow Water is modified in the revised manuscript as:

"Temperature maps at 800 m depth (Fig. 5a–c)—about the core depth of the deep ocean temperature difference in Figure 3—reveal a clearly warmer water mass in LL compared to LH (and HH) with a center in front of the Strait of Gibraltar. This strongly hints at crucial changes in the Mediterranean outflow with introduction of the nest, which includes the strait. Mediterranean an outflow water (MOW) is a prominent warm and saline intermediate-depth water mass at this latitude in the North Atlantic. A zonal cross-section of temperature through the Gibraltar Strait shows that LH and HH are colder than LL in the area where MOW is found (Fig. 5e–g). Zooming in on the Strait region (Fig. A6), shows that just by refining the horizontal resolution (but leaving the vertical axis unchanged) and adjusting the diffusion parameters accordingly we achieve an actual overflow of the MOW, which stays attached to the steep bathymetry down to 600–700 m. A MOW layer is established at around 600 m further offshore in LH and HH, which is absent in LL. Typical for low resolution models, LL depicts a large, diffusive MOW core, connecting to and partly destroying the upper ocean (0–500 m) stratification resolution, which also enables near-surface heat to access the deeper layers. The warm temperature and the weaker stratification of LL strongly disagree with observations (cf. Fig. 5d, h). Below the MOW, the vertical temperature gradient intensifies between 1000 and 1500 m. This gradient is too strong in all three simulations but mostly in LL. LH and HH show a structure similar to the observational data but with MOW at a too shallow depth; the observed MOW core being located at 1000–1100 m. However, the upper ocean stratification is intact in LH and even better represented in HH.

The Strait of Gibraltar is included in the VIKING10 nest region and thus its bathymetry is more realistically represented in LH and HH though width and depth of the Strait are constrained to observed values in LL using partial cells. While this constrains the MOW volume flux, it does not help with the larger (numerical) diffusion in LL. Since the vertical resolution and discretisation is the same across all model versions here, all are subject to the common overflow issue in z-layer models, especially with insufficient vertical resolution, causing considerable biases in MOW spreading (Legg et al., 2009; Behr et al., 2022). We thus conclude that the more realistic—though still imperfect—simulation of MOW spreading in LH and HH explains the mid-depth, sub-tropical cold and fresh difference to LL. "

Further, we included a statement on the sill depth in the caption of Figure 5:

"Note, the sill depth in ORCA05 and VIKING10 grids is roughly the same, only in panel (a) topography appears deeper due to the plotting technique."

4) Absence of current feedback

As explained in lines 108–109, the coupling interface does not account for the current feedback. This is surprising given that the objective of the study is precisely to investigate the impact of ocean mesoscale resolution on the coupled system.

From a technical point of view, including ocean currents in the wind-stress calculation is straightforward to implement and computationally inexpensive. Numerous studies have demonstrated the importance of this process, making its omission increasingly difficult to justify, particularly in a study focusing on the effects of high ocean resolution.

Ocean modelers often dislike this process because it damps mesoscale eddies, whose explicit simulation is computationally expensive. However, can one justify neglecting a physically important process simply because doing so produces a compensating error and artificially maintains stronger eddy activity?

Ans: We thank the reviewer for spotting this mistake and we agree that the absence of current feedback is a limitation. However, it was a mistake from our side to describe the wind stress as absolute when in fact all our model experiments use relative wind stress. The manuscript now clearly states that we use relative wind stress, we have corrected the reference, and a table with all coupling fields now clearly state how ocean currents are coupled.

Finally, a minor but important point raises concerns about the care taken in preparing the manuscript. The citation to Zhai et al. (2010) in line 109 is incorrect. That paper does not discuss the eddy-killing effect associated with including ocean currents in the computation of wind stress.

Ans: Thanks a lot for pointing this out.

The citation has been corrected.

5) Relation to previous studies

Several previous studies are closely related to the present work. The authors cite Matthes et al. (2020) and Martin and Biastoch (2023), but they should also cite Taillandier et al. (2014), which was a pioneering study on this topic, albeit using forced rather than coupled simulations.

Many of the differences found between LL and LH, particularly regarding the AMOC and northward heat transport, had already been reported in Taillandier et al. (2014) and Matthes et al. (2020) for example. The authors should therefore discuss much more explicitly what is genuinely new in the present study. Are these simulations simply confirming previous findings, or do they provide new insights? More specifically, what scientific advantages does OpenIFS provide compared with ECHAM6?

Ans: Thank you for your suggestion.

We agree that our findings share similarities with those of Talandier et al. (2014). However, as the reviewer correctly noted, their study was based on an ocean general circulation model (OGCM), whereas all simulations in the present study were performed using a fully coupled climate model. We have therefore revised the manuscript to include a more detailed comparison between our results and those of Talandier et al. (2014), while also highlighting the novel aspects of our study. In particular, we emphasize the effects of coupling, the separate contributions of ocean and atmospheric resolution, and their combined influence on the simulated climate. We have also expanded the discussion to explain the scientific advantages of OpenIFS over ECHAM6 in the context of high-resolution coupled simulations. The following text has been added to the revised manuscript:

"Improvements in ocean circulation and the associated changes in volume transport resulting from the use of a nested ocean configuration have previously been reported by Talandier et al.

(2014). In contrast, the present study provides a comprehensive assessment of both volume and meridional heat transport in the subtropical and subpolar North Atlantic and examines their implications for subpolar deep convection and the associated air–sea coupled feedbacks. A key novelty of this work is the explicit separation of the respective impacts of increasing ocean and atmospheric resolution in a fully coupled climate model. This approach allows us to quantify not only the individual contributions of the two model components but also the additional benefits arising from a better match between a high-resolution atmosphere and a high-resolution ocean. Using the FOCI modeling framework, Matthes et al. (2020) previously demonstrated substantial improvements in the mean climate state using a nested ocean configuration coupled to the ECHAM6 atmospheric model (Stevens et al., 2013). In the present study, ECHAM6 is replaced by OpenIFS, whose capability to operate at higher atmospheric resolutions allows a more balanced coupling with the eddy-rich nested ocean configuration (Kjellsson et al., 2020), enabling a direct assessment of the combined effects of increased ocean and atmospheric resolution."

The discussion would benefit from explicit comparisons with equivalent figures in previous studies, for example:

- Figure 2 with Figure 7 of Taillandier et al. (2014) and Figure 3 of Martin and Biastoch (2023).
- Figure 8 with Figure 6 of Taillandier et al. (2014).

Ans: Thank you for the comment.

The comparison has been included in the discussion section of the revised manuscript as:

"Talandier et al. (2014) showed that the nested high-resolution configuration enhances the transport of dense water in the Deep Western Boundary Current (DWBC) and the Denmark Strait overflow, leading to a stronger AMOC in the subpolar North Atlantic. They also attributed the enhanced AMOC intensity near 35°N to interactions between the surface and deep currents. This mechanism is consistent with our findings, which similarly indicate an increase in AMOC strength associated with the improved representation of ocean circulation in the high-resolution configuration."

- Figure 6 with Figures 15 and 16 of Taillandier et al. (2014).

Ans: Thank you for your suggestion.

The comparison has been included in the discussion section of the revised manuscript as:

"Talandier et al. (2014) showed that the nested high-resolution configuration enhances the transport of dense water in the Deep Western Boundary Current (DWBC) and the Denmark Strait overflow, leading to a stronger AMOC in the subpolar North Atlantic. They also attributed the enhanced AMOC intensity near 35°N to interactions between the surface and deep currents. This mechanism is consistent with our findings, which similarly indicate an increase in AMOC strength associated with the improved representation of ocean circulation in the high-resolution configuration."

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