## Public response to reviewers egusphere-2025-4944

The authors thank the editor and the reviewer for their constructive comments and suggestions. Please, see below our public responses.

## Response to the reviewer

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## Reviewer 2

**Reviewer Comment 2.1** — This paper examines how the Nordic Seas responded to a significant marine cold air outbreak in the winter of 2020. The authors use atmospheric and oceanic reanalysis data to examine this question. The authors quantify the contributions of the mean surface turbulent heat flux relative to the mean change in ocean heat content during the event. The authors find the air-sea fluxes dominant in the western part of the Nordic Seas, while finding lateral oceanic heat transport more important in the east. This is an interesting paper, looking in detail at a given atmospheric event. All possible processes are considered, and their relative roles (spatially) are considered and explored. The paper is generally well written. That said, there are some limitations to the study, and some places where the work could be expanded on. Thus, I would recommend moderate revisions. Specific comments are provided below.

**Reply**: We thank the reviewer for the comments and suggestions for improvement provided for the manuscript.

**Reviewer Comment 2.2** — The ocean fields used come from the GLORYSV12 reanalysis product. The authors reference other studies showing the quality of this product in the Nordic Seas. That said, it is a single product, with biases. Why did the authors not consider using several reanalyzes to determine how robust their analysis is?

**Reply**: The reviewer has a fair point and it would have been interesting to compare the response in GLORYS to other ocean reanalyses that provide output at a daily or higher temporal resolution. However, reanalysis such as ORAS5 and RARE only provide monthly and 5-day temporal resolution, respectively. GLORYS12 is thus superior in terms of resolution compared to most reanalysis products. Given that our study focuses on the oceanic response to a specific MCAO, the restriction to one reanalysis product is justified and a detailed reanalysis inter-comparison would be beyond the focus of our study and change the character of this manuscript from a physical discussion of the oceanic response to a discussion of a model inter-comparison.

**Reviewer Comment 2.3** — As a side point, at 1/12 degree resolution, the GLORSYSV12 product is not fully eddy-permitting. It resolves the largest eddies in the region, but not all. I believe the terminology eddy-rich would be more appropriate.

**Reply**: Thank you for pointing this out. We agree that not all the eddies are fully resolved in the Nordic Seas in GLORYS12 (e.g. size less than $\sim$ 5 km). However, the terminology "eddy-resolving" is used in the GLORYS12 overview paper (Lellouche et al., 2021) and we consistently adopted this terminology in our manuscript.

**Reviewer Comment 2.4** — The atmospheric product used is ERA5. It has known biases, including being too warm in polar regions, and having events with extreme wind anomalies. It would be good to confirm how those might impact the authors' study for the given MCAO.

**Reply**: Renfrew et al. (2021) compared ERA5 to observations for winter 2018 over the Iceland and Greenland Seas and found that ERA5 performs well over the ice-free ocean, with biases "significantly less than the observed standard deviations for all variables" (T2m, wind speed, wind direction, and turbulent heat fluxes). Moreover, the observed wind speed during the MCAO in the COMBLE campaign (see supplement in Geerts et al., 2022) is consistent with ERA5 on 2 and 4 February (max 15 m/s) at two different sites in the eastern Nordic Seas (Bear Island and Andenes). However, ERA5 has been shown to have some limitations over the marginal ice zone. We added this discussion in section 2.1 and section 3 (see also comment 2.7).

**Reviewer Comment 2.5** — The methods section requires some further details on the approaches used. The authors discuss using fields from ERA5. But what bulk formulae were used in the calculations? How were these computed in conjunction with GLORYSV12 – especially if the authors used daily outputs from the ocean reanalysis and higher frequency fields from ERA5? How were the fluxes computed over the model grid cells with sea-ice? Were all the calculations done at the location of the model T-grid cells? Or interpolated to them?

**Reply**: We did not use any bulk formulae, as we used the fluxes provided by ERA5 for our analysis (3-hourly surface sensible and latent heat fluxes, cf. section 2.1). We used ERA5, as GLORYS12 is forced by ERA5 after 2019 (cf. section 2.1). In GLORYS12, momentum and heat turbulent surface fluxes are computed from the Large and Yeager (2009) bulk formulae, where atmospheric quantities were sampled at a 3-hour resolution and the calculations were done on the model Arakawa C-grid. See Lellouche et al. (2021) for details on production of the GLORYS12 reanalysis.

**Reviewer Comment 2.6** — I don't understand why the heat budget is computed over the total ocean depth (i.e. using Dbot). The lower layers are not going to be impacted by the atmospheric forcing. Thus, the size of the signals shown will be impacted by the ocean depth, which doesn't seem relevant to the authors' questions. Especially for the ratio calculation, such as in figure 5. Yes, not using the bottom depth does mean looking at vertical heat fluxes, but it shouldn't be especially hard to compute. As well, that approach could mean that the authors could look at the very fluxes into and out of given watermass layers, such as the Atlantic Water layer.

**Reply**: Thank you for raising this concern. As mentioned in the response to reviewer 1 (cf. reply to comment 1.6, Fig. R1-1 and Fig. R1-2 which is the same as Fig. R2-1 below), integrating the ocean heat content down to the bottom or to near the MLD does not really affect the amplitudes of the change in the western Nordic Seas, as we analyse total heat change, not specific (mass-normalised) heat exchange. Integrating to the MLD would imply that we would integrate to a spatially and temporally varying depth across the domain of interest, requiring further terms in the heat budget equation, such as entrainment and heat content changes due to the change of the MLD. To truthfully resolve these effects, one would ideally implement an online calculation within the model. As indicated by the reviewer, when integrating down to a fixed level close to the deepest MLD (e.g., 763 m, Fig. R1-2c), the residual would also include contributions from the vertical heat flux convergence, which we, however, cannot calculate as GLORYS does not provide vertical velocities. Furthermore, as mentioned in the reply to comment 1.2, it is not feasible to provide reasonable estimates of the lateral ocean heat flux convergence. To justify our choice, we show that the depth chosen for the integration does not significantly affect our results when integrating down to a fixed level close to the deepest MLD or to the bottom (compare Fig. R2-1c and Fig. R2-1d below). However, when the vertical integral stops within the ML (109 m), the ratio, e.g., in the Greenland Sea, can be greater than 1, meaning that the atmospheric forcing overestimates the changes in the ocean heat content (Fig. R2-1a), while the ratio still features strong variability in the easter Nordic Seas. Already when integrating to 266 m depth (Fig. R2-1b), the results are qualitatively very similar to chosing 763 m or the bottom, respectively (Fig. R2-1c,d).

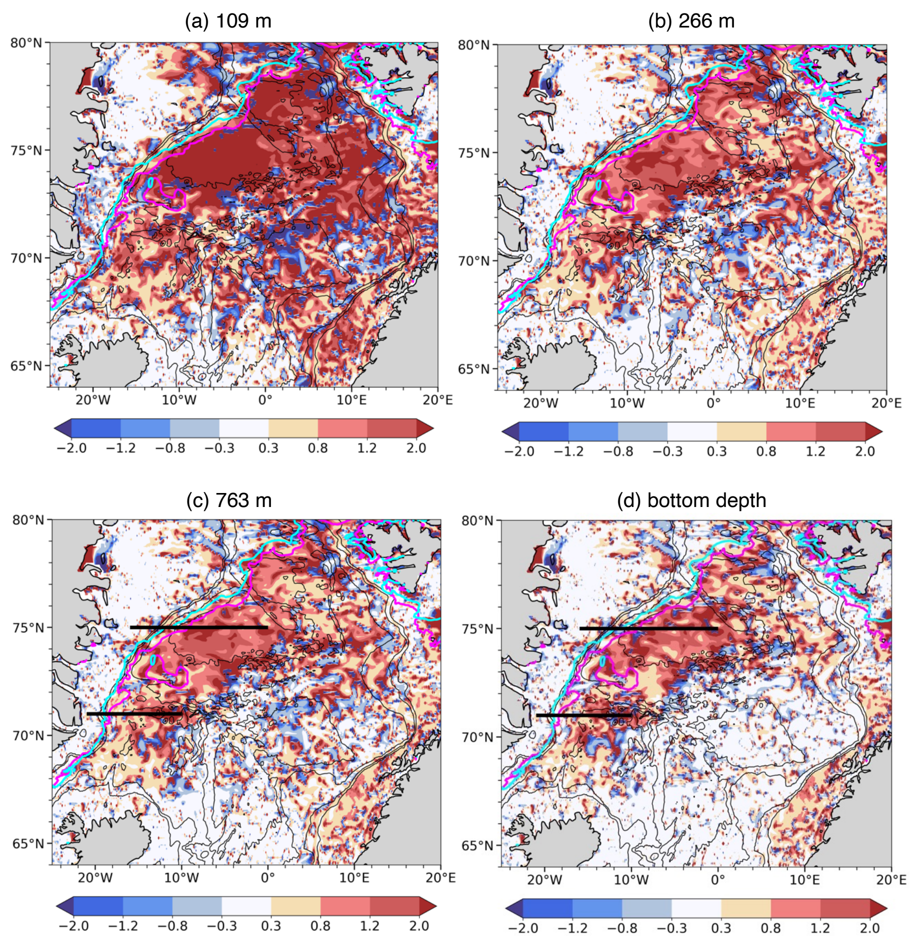


Figure R2-1: Ratio between mean downward surface turbulent heat flux and mean ocean heat content tendency (W·m$^{-2}$) during the MCAO (02/02/2020-06/02/2020), at 4 different model depths: (a) 763 m, (b) 902 m, (c) 1452 m, and (d) bottom depth (as in Fig. 5d of the manuscript).

**Reviewer Comment 2.7** — Given the importance of sea-ice in modifying the air-sea fluxes, I would have liked to seen more discussed about it in the paper (rather than just seeing the ice edge on figures). Since the authors discuss this at line 140 (for example), could the authors' show sea-ice growth rates, thickness changes and/or advection during the MCAO event, as the sea-ice is likely also responded to it.

**Reply**: Thank you for raising this interesting question about the sea ice response. Unfortunately, GLORYS12 does neither provide sea ice growth rates nor sea ice advection. However, we refer to the sea ice volume changes during the MCAO in line 138 and refer to Fig. S1 and added "likely indicating a combination of south-eastward sea ice advection and sea ice formation at the edge."

**Reviewer Comment 2.8** — In section 4.3.1, the authors' discuss how both Polar Surface Water and Atlantic origin Water were transported southward. It would be nice to quantify this, with timeseries, to help see how the transport changes during the MCAO.

**Reply**: Thank you for the suggestion. While we do not quantify the actual volume or heat transport (nor heat flux convergence in general, see our reply to comment 1.2 to reviewer 1), the strength of the current is visible in Fig. S3b,e (figure cited in the beginning of sections 4.3.1 and 4.3.2). When comparing the sea surface velocity before and at the end of the MCAO, we see that there is only a small change in the 75°N section in the Greenland Sea (cf. Fig. R2-2 below), and the Polar Surface Water and Atlantic-Origin Water velocities within the EGC during the MCAO are therefore relatively constant. In the Iceland Sea at the 71°N section (cf. Fig. R2-3), a distinct change in velocity is visible, though this change is related to an eddy, as mentioned in the manuscript (section 4.3.2. Please see also comment 1.9). In our framework, any change in transport is taken into account as a change in ocean heat flux convergence (accounted for as a residual), and we do not assess how it might have been altered due to the MCAO, as this would require a detailed momentum analysis, which is beyond the scope of this manuscript.

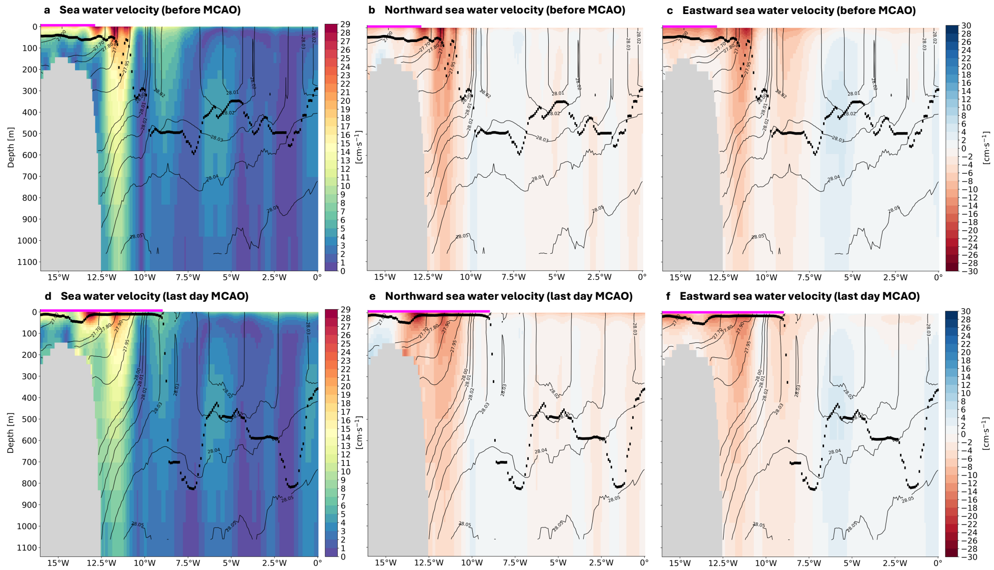


Figure R2-2: Vertical cross-sections of mean sea water speed ($cm{\cdot}s^{-1}$) at 75°N in the Greenland Sea, before the MCAO (31/01/2020, first row) and at the end of the MCAO (06/02/2020). The northward components are shown in (b) and (e), and the eastward components are shown in (c) and (f). The thick black lines indicate the mixed-layer depth. The magenta lines indicate the sea ice extent. The thin black contours correspond to the density contours.

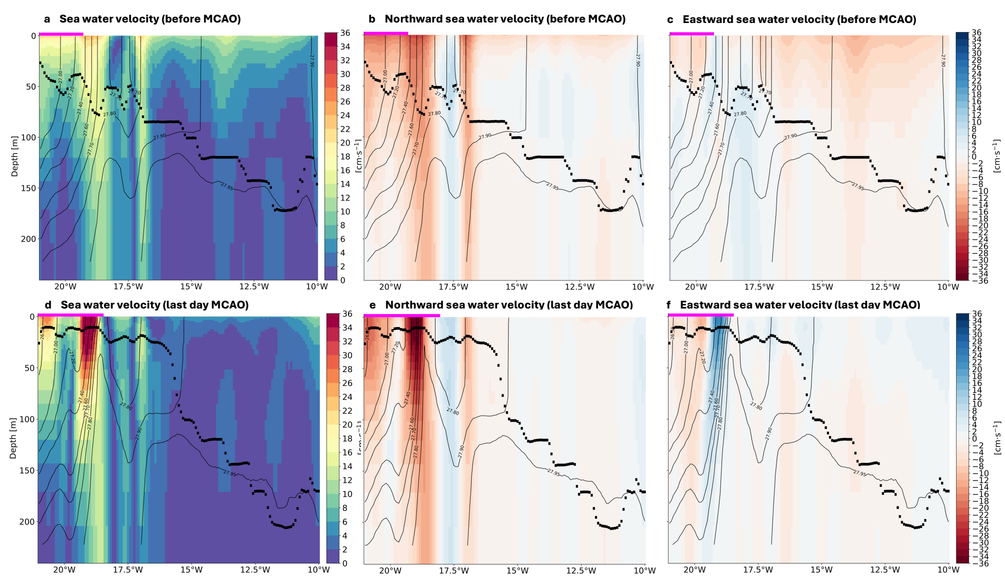


Figure R2-3: Vertical cross-sections of mean sea water speed ($cm{\cdot}s^{-1}$) at 71°N in the Iceland Sea, before the MCAO (31/01/2020, first row) and at the end of the MCAO (06/02/2020). The northward components are shown in (b) and (e), and the eastward components are shown in (c) and (f). The thick black lines indicate the mixed-layer depth. The magenta lines indicate the sea ice extent. The thin black contours correspond to the density contours.

**Reviewer Comment 2.9** — In the conclusion, the authors start by stating they use the high-resolution GLORYSV12 reanalysis. Beyond my above point related to eddy-permitting vs eddy-rich, I don't feel like the authors really take advantage of the resolution of the product. It would be good to know where the given resolution is most helpful for the study. Or, even better, can the authors examine the role of the eddies and mesoscale in their results, for example, their role in the lateral oceanic heat transport in the eastern part of the domain.

**Reply**: This is a very interesting and relevant question. However, to quantify the role of the mesoscale requires comparing our GLORYS analysis to another dataset at coarser resolution that does not resolve the scales inherent to GLORYS, with the disadvantage that difference will be due to both resolution as well as model version. Ideally, one would need a dataset driven by the same forcing using the same model at different resolution to prove the relevance of scales. Alternatively, one could perform a filtering, removing certain scales from the data, but such an analysis would also suffer from certain shortcomings, as all results will mainly be a result of the choice of filtering. Most likely, the main difference when using different ocean resolutions would be the representation of the currents and eddies. From our analysis, it is evident that the strength and presence of currents and eddies significantly alters the visibility of the oceanic response to the atmospheric forcing. In coarser resolution ocean models, eddies would be absent and currents would be significantly weaker in strength, which would then most likely yield weaker variability across the Nordic Seas in the response to the atmospheric forcing. This discussion is already implicit in our manuscript, but we tried to further clarify these aspects in the conclusions.

**Reviewer Comment 2.10** — L124: Fram Strait

**Reply**: Corrected, thank you.

**Reviewer Comment 2.11** — Figure 4 caption: The authors state they are comparing surface ocean conditions pre and post the MCAO. Yet, if the MCAO covered Feb 2-6, while Jan 31 is pre-MCAO, Feb 6 is still during the MCAO. Shouldn't the plots then use Feb 7? And why not Feb 1, instead of two days before for the pre-MCAO panels?

**Reply**: Thank you for noting this. We corrected "the post-MCAO (06/02/2020)" to "end-MCAO" as it is written for the sea ice edges in the end of the caption. We also chose to show February 6th in the manuscript and not Feburary 7th, as the effects on the ocean are the same (for example regarding the Conservative Temperature differences, compare Fig. R2-4a and b below). In this way, the figures are consistent with Fig. 2 (last row), showing the atmospheric conditions throughout the MCAO. Similarly, using January 31st or Feburary 1st does not really affect the result here (Fig. R4-2).

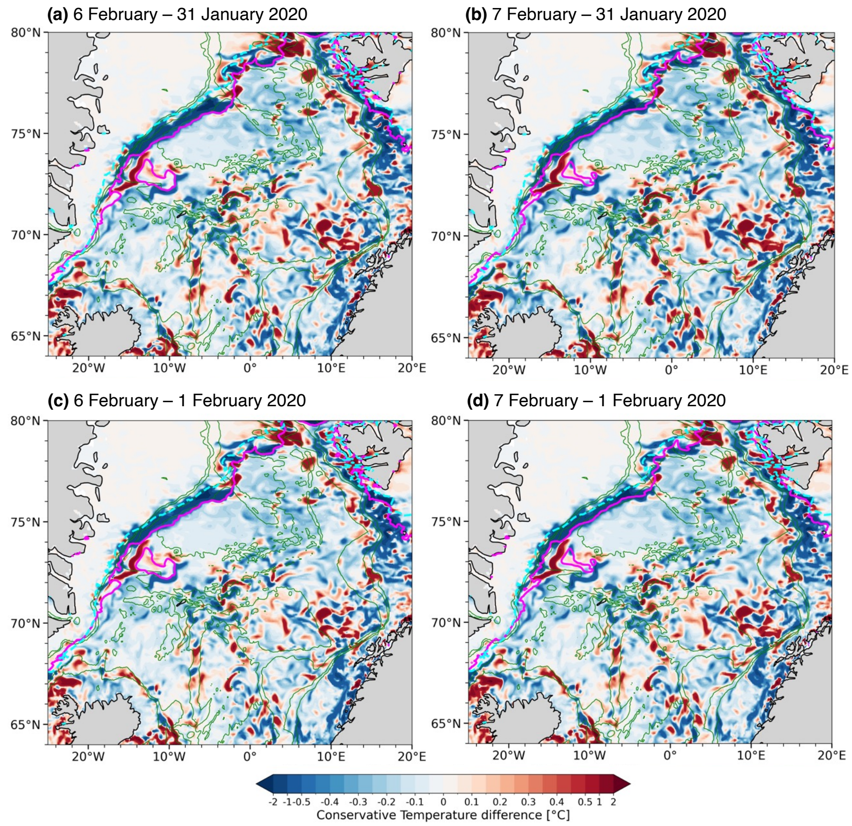


Figure R2-4: Conservative Temperature difference at different starting and end dates. (a) 06/02/2020 - 31/01/2020, as in Fig. 4a in the manuscript. (b) 07/02/2020 - 31/01/2020. (c) 06/02/2020 - 01/02/2020. (d) 07/02/2020 - 01/02/2020

**Reviewer Comment 2.12** — Figures 6 and 7: The thick black, blue and red lines to show the mixed layer depth are discontinuous, and not easy to follow in places where the depth changes rapidly. The plotting of these lines could be improved (with a continuous line).

**Reply**: Thank you for the suggestion. We changed the lines representing the mixed-layer depths into continuous lines.

# References

Geerts, B., Giangrande, S. E., McFarquhar, G. M., Xue, L., Abel, S. J., Comstock, J. M., Crewell, S., DeMott, P. J., Ebell, K., Field, P., et al.: The COMBLE Campaign: A study of marine boundary layer clouds in Arctic cold-air outbreaks, Bulletin of the American Meteorological Society, 103, E1371–E1389, https://doi.org/10.1175/BAMS-D-21-0044.1, 2022.

Large, W. and Yeager, S.: The global climatology of an interannually varying air–sea flux data set, Climate dynamics, 33, 341–364, 2009.

Lellouche, J.-M., Greiner, E., Bourdallé-Badie, R., Garric, G., Melet, A., Drévillon, M., Bricaud, C., Hamon, M., Le Galloudec, O., Regnier, C., et al.: The Copernicus Global 1/12° Oceanic and Sea Ice GLORYS12 Reanalysis, Frontiers In Earth Science, 9, https://doi.org/10.3389/feart.2021.698876, 2021.

Renfrew, I. A., Barrell, C., Elvidge, A., Brooke, J., Duscha, C., King, J., Kristiansen, J., Cope, T. L., Moore, G. W. K., Pickart, R. S., et al.: An evaluation of surface meteorology and fluxes over the Iceland and Greenland Seas in ERA5 reanalysis: The impact of sea ice distribution, Quarterly Journal of the Royal Meteorological Society, 147, 691–712, https://doi.org/0.1002/qj.3941, 2021.