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 responses.

Second response to the reviewer

Reviewer 1

I am mostly good with authors' response and revision.
I have some points regarding the authurs response, and a few comments:

Reviewer Comment 1.1 — Violating CFL criterion would cause the model to stop, not the reason for budget closure. Data Assimilation "artificially correct" some variables, though it could be minor.

Reply: We agree that a model would most likely crash when violating the CFL criteria. However, a similar issue arises when trying to diagnostically calculate advective tendencies from these data. The CFL criteria basically assesses if the speed of signals in the data can be resolved numerically given the grid and time spacing. Within the strong currents along the boundaries, data with daily resolution will not suffice to reasonably resolve advection within the currents given the high spatial resolution of the GLORYS data. Hence, the violation of the CFL criteria when calculating advective terms will be equally relevant and will cause problems with the data analysis. Of course, data assimilation will correct data at every time step, but that does not help when trying to calculate budgets of advective terms using the reanalysis data at one time step.

Reviewer Comment 1.2 — To be rigid in equeation (2), I would use "approximately equal symbol ≈" or add a "Residual", ESPECIALLY givien that volume is not conserved and it is reanalaysis. And you can find there is a diffusion term in many other heat budget studies.

Reply: Thank you for the suggestion. We added a residual term RES in the equation, corresponding to a residual contribution from horizontal diffusive processes and inaccuracies due to data assimilation (L. 107).

Reviewer Comment 1.3 — I would make it clear that sea ice pack formation is GLORYS behavior.

Reply: We modified the sentence L.144 "A large sea ice protrusion formed around 73°N–11°W (Fig. 2d,g), and was present from 2 to 9 February (not shown)." to "A large sea ice protrusion formed around 73°N–11°W (Fig. 2d,g), and was present from 2 to 9 February **in GLORYS12** (**not present in observations,** not shown)."

Reviewer Comment 1.4 — "total surface heat flux"in fig2, "Downward turbulent heat flux" in Fig5, "upward net surface turbulent flux" in Methods, positive flux in Fig2 then Negative flux in Fig5 are used interchangeably and confuses me. Intead of mentioning sub-components Q_rad, Q_thf, downward or upward, using the "net surface heat fluxes" is succinct.

Reply: The reviewer has a point about the presenting surface fluxes once as upward and another time as downward. However, the choice is consistent with respect to the community of interest, i.e., figure 2 presents the atmospheric perspective, where fluxes are commonly presented as upward, i.e., positive implies a contribution to the atmosphere. For figure 5, however, the context is the ocean and its response to the surface forcing. Hence, a downward component is more directly interpretable for the budget of the ocean, where negative values imply a loss for the ocean. The choice for figure 5 is then also consistent with the other diagnostic of the ocean heat content change. Hence, we prefer to keep the figures as they are and envision that the reader will be able to understand the respective context. Regarding the mentioning of the radiative and turbulent components: we think that it is valuable to remind the reader about this composition of the total surface flux, especially given the fact that we then often limit our analysis on the turbulent part, due to our focus on the atmospheric impact on the ocean. Hence, we prefer to keep this explanation.

Reviewer Comment 1.5 — Fig5 and equation (2) and related response, the authors still consider ocean heat flux convergence CONV term as the residual relative to surface heat flux term, which is fine. Matimatically it does make sense, you choose one and the rest is the residual. But in terms of quantity, full-depth CONV term is much larger. Using colorbar range on the scale of thousand (-3000 to 3000 W/m2) for heat flux on range of hundrads (700 to 100 W/m2) of course makes it less evident. Neverthess, if not to add up more work on CONV, authours may be able to find some literature to support their consideration and convince readers. Either surface CONV or Mixed-layer CONV, or full-depth CONV using model data has been done elsewhere (e.g, Isachsen et al., 2012, Fig6 in Årthun & Eldevik, 2016)

Reply: As shown in our previous response, calculating the ocean heat content change over different depths did not qualitatively change our arguments. Furthermore, the CONV term will only be much larger for specific regions, mostly those with deeper currents, though for many other regions the differences when integrating over only a fraction or the full depth of the ocean did not make a significant difference (see response to last review with assessment for different choices of depth). We also have added the reference to Årthun & Eldevik, 2016 (L. 101), as the equation used is equivalent and the integration includes the whole water column as we did.

Reviewer Comment 1.6 — Authors mentioned "ocean currents and eddies" 6 times and pointed to fig5b,5c,3e. It is obscure and no argumental supports. "regions with strong currents or eddies (fig x)", then somewhere with brackets "strong currents and associated eddies (Norwegian Sea and West Spitsbergen Current", then somewhere "In the boundary current region, xxx strong currents and eddies". I guess that authors wanted to point to the eastern Nordic Seas but note that EGC is also a strong current with eddies around, and under influence of CAO.

Reply: It is not clear what the reviewer means by obscure in this context. The mentioned figures clearly support the respective claims about the presence of ocean currents and/or eddies in specific regions dominating the ocean heat content signal compared to the surface forcing associated with the MCAO. Furthermore, in the conclusions, we wrote: "In regions of the Nordic seas where strong ocean currents and eddies are resolved in the reanalysis, however, lateral oceanic heat transport dominated the mixed-layer response. This effect is observed in the eastern Nordic Seas over the Norwegian Atlantic

Current and over parts of the EGC that are located offshore of the sea ice edge in the Iceland Sea." Hence, we mention both the eastern Nordic Seas and EGC. We also do not mean that the currents/eddies are not affected by the MCAO, and we were not focusing on this causal link in the study, but on how both the MCAO and currents/eddies together act in modifying the ocean mixed layer properties. We added "with respect to lateral oceanic heat transport" in the conclusions (L. 290) for better clarity.

Reviewer Comment 1.7 — GLORYS is not eddy-resolving in the Nordic Seas, though is is described as global eddy-resolving reanalysis.

Reply: We added "global" in the abstract (L. 4) and changed "eddy-resolving" to "eddy-rich" in the introduction (L. 53) when referring more specifically to the Nordic Seas.

Reviewer Comment 1.8 — Line 289, "the ocean surface in the Nordic Seas becomes deeper"?

Reply: Thank you, corrected to "the ocean mixed layer".

Nevertheless, it is publishable subject to minor/technical correction.

Reference:

Isachsen, P. E., Koszalka, I., & LaCasce, J. H. (2012). Observed and modeled surface eddy heat fluxes in the eastern Nordic Seas. Journal of Geophysical Research: Oceans, 117(C8).

Årthun, M., & Eldevik, T. (2016). On anomalous Ocean heat transport toward the Arctic and associated climate predictability. Journal of Climate, 29(2), 689–704.