

Reply to reviewers

We thank the reviewers for their comments and suggestions. Following their advice, we have revised the manuscript, which has improved in clarity as a result.

In response to both reviews, we changed our analysis to achieve the same length of tracks for all particles released. As a result, the total number of particles reduced. The whole quantitative analysis was adjusted to the new tracking time period of six months. Changing the analysis did not impact the main outcome of the paper.

Below we address the specific comments of the two reviewers with our replies.

Reviewer 1:

This is an interesting paper on an important topic: boundary-interior exchange in the subpolar North Atlantic and its impact on restratification after deep convection. The authors analyze simulated particle trajectories as they spread from the relatively warm Irminger Current that flows northeastward west of the Reykjanes Ridge, quantifying the fractions that (1) follow the cyclonic boundary current around the Irminger Sea, (2) spread into the interior Irminger Sea, and (3) spread specifically into the deep convection area of the interior Irminger Sea. They go on to investigate the relative buoyancy impact of waters from different depths and positions within the two-core Irminger Current.

The particle trajectories were calculated using a high-resolution (~2km; 216 vertical levels) configuration of the MIT/GCM covering the Irminger Sea, the Iceland Basin, and most of the Nordic Seas. This configuration has been around for a while and used in numerous studies. The authors provide convincing analyses that the model reasonably represents the primary circulation and hydrographic features of the area of interest (Irminger Sea). The particles are initiated in the Irminger Current west of the Reykjanes Ridge at depths from 0-1500m and tracked for up to 12 months (the simulation is only 12 Months long, covering the time period 9/1/2008 to 8/31/2009). The particles were initiated every three days for 9.5 months along a section crossing the Irminger Current at about 59N where moorings have been maintained since 2014 as part of the Overturning in the Subpolar North Atlantic Program (OSNAP). Total number of trajectories was 180,050, of varying lengths from 2.5-12 months.

I have two major comments about this work, and a few other comments.

First, I'm having difficulty interpreting the results since the simulated trajectories are of varying length but grouped all together in the analyses. The authors are up front about the varying trajectory lengths, but I'm concerned this leads to mis-leading results in some cases. For example, the fraction of particles that remains in the boundary current includes shorter trajectories that may have not had time to reach the interior. This statement, "For the BC particles we find a high particle density close to the top of the Reykjanes Ridge, mostly following the eastern IC core northward (Fig. 4d)" is technically correct, but the high concentration near the Reykjanes Ridge includes (I assume) many short trajectories that don't travel very far from their initiation position. So what does it mean that there is this high concentration near the Ridge? Similarly, consider the particles grouped as "DCA" (Deep Convection Area) and "non-DCA". According to the definitions, the former includes particles that enter the interior but

never reach the DCA (estimated as 32%), whereas the latter includes only particles that make it to the DCA (estimated as 29%). It is reasonable I think to assume that it takes longer for particles to reach the DCA, and they have to pass through the interior non-DCA region to get there. So the shorter trajectories may not be long enough to reach the DCA and therefore end up in the non-DCA group. If all the particles were 12 months long (the maximum), a much higher fraction may reach the DCA. In my opinion, the fractional results only make sense if all the trajectories are the same length.

I spent a little bit of time trying to think about how the authors could address this problem. They could just use the first 2.5 months of all the trajectories. This won't be very satisfying though, since the travel times to the most interesting destinations (e.g., the DCA and Cape Farewell) are on average longer than that. Another possibility would be to recycle the model output so trajectories of the same length could be calculated. I think this is the better option, unless the authors can come up with another idea.

We thank the reviewer for these important remarks. We decided to reduce the particles tracking time and only analyse particles tracks released between 1st of September 2007 and 13th of February 2008. All resulting particles tracks were only tracked for half a year to ensure the same particle track length. Our total particle number reduced to 108030 particles, but all have the same length of 6 months now. As a result, our quantitative analysis only slightly changed. The first selection between interior and boundary current particles changed to 39 % BC particles (38 % before) and 60 % interior particles (61% before). The second selection showed slightly more changes that are likely related to the shorter tracking time period. 26 % of the particles entered the DCA (29 % before) and 34 % stayed in the non-DCA area (32 %). All numbers were changed accordingly in the manuscript and also the summary figure was updated (Fig. 7). As adjusting the analysis did not change the major outcome of the paper, we decided against recycling the velocities fields. We also decided against recycling the velocity field as it would introduce a spurious jump in the velocity and tracer values. Rendering the trajectories after the jump would make it hard to interpret. Indeed, it would be a good idea when working with lower temporal resolution for example monthly means, but with our high temporal resolution of 6 hours the jump can be quite significant. In addition, we tested the particle positions after three and six months to ensure that they spread away from the release location within the tracking time period of six months. At three months particles have spread out through the Irminger Sea already which gave us confidence that particles have enough time to reach the DCA and for example Cape Farewell. The high concentration of particles around ridge is therefore not related to the particles not travelling away. Likely the strong exchange around the ridge also plays an important role in the high particle concentration.

Please see the main parts of the adjusted text in the results section:

Lines 16-18: "Of those particles, 39% follow the boundary current circulation and 60% enter the interior Irminger Sea. Around 1% leaves the Irminger Sea through Denmark Strait and across the ridge to the Iceland Basin. From all particles released, about 26% reach the DCA, steered by mesoscale variability."

Lines 221-223: "We released particles in the first six months of the simulation, between 1st of September 2007 and 13th of February 2008 and track them for six months. Previous tests revealed that a tracking time period of six months is enough for particles to spread through entire Irminger Sea basin (not shown)."

Lines 242-245: “As we are interested in restratification processes in the Irminger Sea, we do not focus on the small number of particles leaving the Irminger Sea through DS (defined as particles that cross 22°W north of 66°N, black line in Fig. 4a; 0.2 %), and across the Reykjanes Ridge to the IB (defined as particles that cross the Reykjanes Ridge towards the east, dashed green line Fig. 4b; 1 %) within the considered 6-months tracking time period.”

Lines 279-280: “As we are interested in boundary-current-to-interior transport in the Irminger Sea, we focus our analysis on particles that either belong to the BC (39 %, Fig. 4c) or the iIS category (60 %, Fig. 4d).”

Lines 295-297: “From all particles released within the six months tracking time period, 26% reached the DCA (Fig. 4e) and 34% stayed outside the DCA in the interior Irminger Sea (Fig. 4f).”

My **second** major comment is related but regards the interesting analysis of the buoyancy contributions to the DCA from the various regions within the Irminger Current (section 4 of the paper). The authors find that both the eastern and western Irminger Current cores contribute similar density anomalies to the DCA, but that since 75% of the particles that reach the DCA are from the western Irminger Current core, it is concluded that the western core has more influence on restratification in the DCA. Again, I'm concerned about the variable length of the simulated trajectories. The eastern Irminger Current core is farther away from the DCA, so it will probably take longer for particles to get there. This may bias the contribution from the eastern core too low.

We thank the reviewer for this remark. As stated in our answer before, we changed the analysis, so now all particle tracks have the same time to get to the DCA. The new number of 65 % of the particles entering the DCA are released in the IC's western core is more robust considering that all particle tracks have the same length now and therefore the same chance to get to the DCA. See comment on the answer before for details.

Other comments are listed below, some of which relate to the major comments above.

Line 16: somewhere in the abstract it should be mentioned that the simulation is for just one year

We added this to the abstract. It now reads as follows: “*In this study, we investigate the potential impact of the IC on restratification of the Irminger Sea's deep convection area (DCA), using one-year output of a high-resolution regional model combined with Lagrangian particle tracking.*”

Line 26: Suggest to add a clarification here, something like “at least during the OSNAP observing period (2014-present)”

We added this to the introduction. It now reads as follows: “*Recent observations from the Overturning in the Subpolar North Atlantic Program (OSNAP) have shown that during the observational time period from 2014 - present deep convection in the Labrador Sea contributes much less to the strength of subpolar overturning than previously thought (Fu et al., 2023; Li et al., 2021; Lozier et al., 2019; Petit et al., 2020).*”

Line 56: Remove comma after “both”

Done.

Line 62: These areas were identified as regions of deep convection long before 2021. I suggest to re-phrase this sentence, perhaps adding more historical citations, to reflect that these areas have been known for some time as deep convection regions.

We agree with the reviewer and changed the sentence including more references.

Lines 63-66: “The main convection sites in the subpolar gyre are the Labrador Sea and the Irminger Sea connected by a region south of Cape Farewell (De Jong et al., 2012, 2018; Fröb et al., 2016; Pickart et al., 2003, 2008; Pickart and Spall, 2007; Piron et al., 2016, 2017; Rühls et al., 2021; Yashayaev, 2007; Zunino et al., 2020).”

Line 73: should some of the papers by Pickart et al. Be included?

We added Pickart et al. 2003 and 2008 (see line 78).

Line 75: Suggest to delete word “phase” at the end of the sentence.

Done.

Line 132: “much higher spatial resolution” compared to what? I think you mean compared to the observations—suggest to say that explicitly. The way it reads now, it sounds like you are saying this simulation has much higher spatial resolution than some other simulation.

We agree with the reviewer and changed the sentence accordingly:

Lines 169-170: “Slight differences between this simulation and the observations can be expected due to the much higher spatial resolution in this model simulation compared to the OSNAP mooring array.”

Line 154: Should these Fried citations be labeled 2024a and 2024b?

Thank you. We labelled the two citations accordingly.

Line 182: Suggest to add “particle” before the word “release”.

Done.

Line 199: Did you consider to recycle the velocity fields used to integrate the trajectories so as to get trajectories that are all the same length? See major comments.

Thank you for the suggestion. Instead of recycling the velocity field we decided to only use tracks in this simulation with a length of 6 months. See answer on major comment. We decided against recycling the velocity field as it would introduce a spurious jump in the velocity and tracer values. Rendering the trajectories after the jump would make it hard to interpret. Indeed, it would be a good idea when working with lower temporal resolution for example monthly means, but with our high temporal resolution of 6 hours the jump can be quite significant.

Line 225: Need discussion on how these maps may be affected by the differing lengths of the trajectories. See major comments.

See answer on major comments. All tracks have the same length of six months now. We added this information to the sentence:

Lines 265-267: “Within the half-year tracking time period these particles spread into the interior Iceland Basin, but the majority stay near the ridge north of the release location and partly also south of the release line.”

Line 233: There may be a bias here related to the fact that some trajectories are much shorter than others. It could be that all of the particles that cross the RR would spread into the interior Iceland Basin if they were all 1 year in duration.

See answer on major comments. All tracks have the same length of six months now. This did indeed slightly change the numbers for the Iceland Basin. The number slightly increased from 0.8 % to 1 %. Indeed, before some shorter particle tracks might have not passed the 40-day criterion. As all have the same length now, the number is more robust.

Line 251: What is the meaning of 6% when some of the trajectories are not long enough to reach Cape Farewell?

Thank you for that comment. We decided that this side information is not relevant for the main outcome of the paper and deleted it entirely to avoid confusion.

Line 289: Add figure number before panel letter for clarity.

Done.

Line 353: I think “of” should be “or”.

Done.

Line 355: I found the text from this point to the end of the paragraph somewhat confusing. Some statements appear to be contradictory. I suggest to rewrite for clarity.

Thanks for your comment. We rewrote the paragraph as follows.

Lines 357-366: “To understand whether there is a connection between the location where a particle is released (vertically and horizontally) and its final categorisation, we next split these volume transports into those that enter the interior and those that stay in the boundary current (Fig. 5b, c). As the DS and IB particles also follow the BC circulation and are only a very small number, we combined those with the BC particles. Note that red colors correspond to northward and blue to southward transport. The only small difference between the iIS and BC particles is that particles released in the eastern part of the section (around 31.5° W) follow the BC and do not enter the iIS (Fig. 5c). For particles entering the iIS we find slightly higher volume transports in the western IC core around 34° W, suggesting that particles released here are more likely to enter the iIS (Fig. 5b). The further split of the iIS particles into DCA and non-DCA particles reveals that more non-DCA particles originate from the eastern IC core (Fig. 5d-e). This implies that the seeding location is relevant when considering whether a particle enters the iIS (and DCA or non-DCA) or stays in the BC.”

Line 389: Add comma after “convection area”

Done.

Line 390: I found this sentence somewhat confusing. If the particles lose heat and salt, it is not obvious whether they become more buoyant or less. That will depend on the relative impact of

cooling and freshening. So I don't think it's obvious that these particles will add to the stratification. Maybe I'm missing something, but some clarification might be needed here. Maybe the authors mean "contribute to changes in stratification" rather than increase stratification, which is what I interpreted "add to the stratification" to mean.

Thanks for your comment. We changed the sentence to: *"Next to losing heat to the overlying atmosphere, we find that on their way towards the convection area, particles lose heat and salt to the surrounding waters and with that likely contribute to changes in the stratification outside of the main convection area (Fig. 6a)." (Lines 396-398)*

Line 436: This statement (including the 99% value) could be mis-leading since some of the trajectories are very short and therefore don't have enough time to leave the basin. Suggest to rephrase this sentence.

See answer on major comments. All tracks have the same length now. Nevertheless, we added the tracking time period to make this clearer. It now reads as follows: *"Within the 6-month advection period, the overwhelming majority of particles (~99 %) circulated within the Irminger Sea where they can be divided into two main groups based on their circulation pattern." (Lines 449-450)*

Line 487: Add "the" before "basin".

Done.

Reviewer 2:

Review of « On the fate of the Irminger Current water and its impact on the convection region in the Irminger Sea - A Lagrangian model study » by Nora Fried et al. submitted to Ocean Science

This manuscript addresses the important topic of the role of the Irminger Current in the restratification of the convection zone in the Irminger Sea. To do so, the authors conduct a series of experiments in which particles are released into the Irminger Current within a high-resolution (2 km) numerical simulation run for one year (September 2007 to August 2008). Tracking the trajectories and evolution of the particles' properties allows them to identify which particles affect the deep convection region in the Irminger Sea and to assess their impact on the ambient stratification. The authors conclude that a significant number of particles reach the convection zone and contribute buoyancy there. The topic is important, the idea is interesting, and the study falls within the scope of Ocean Science. However, the model, methodology, and manuscript suffer from shortcomings that weaken the conclusions, particularly their quantitative aspects.

Major Comment 1

The authors should be commended for proposing a model evaluation by comparing it with observations. They conclude that there is generally good agreement with observations. I agree that the velocity structure of the Irminger Current aligns with observations. However, this is not the case for the deep structure of the temperature and salinity fields. The salinity maximum associated with ISOW and the temperature minimum associated with DSOW are absent from the model. This is important. Indeed, the consequence is that the deep density gradient created by these water masses at the interface with LSW is missing. This gradient vertically limits the extent of deep convection. In its absence, convection reaches the bottom. I believe this is what happens in the model. The stratification is unrealistic, and convection nearly reaches the bottom, which is not observed in reality. The authors must acknowledge these limitations and attempt to convince the reader that, even with a model featuring flawed stratification, it still makes sense to study the impact of Irminger Current water masses on stratification in the convection zone. It should be noted that the presentation of the mixed layer (Fig. 5) comes too late in the manuscript and should be included during the model evaluation.

We thank the reviewer for this very constructive feedback.

We agree that the model has shortcomings in the T and S fields related to the representation of ISOW and DSOW. We tried to make this clearer by explicitly mentioning this in the text alongside with the corresponding literature. We also want to stress here that we are not focussing on the actual convection process but on the pathways of waters towards the convection region. We also added to the text that more than 95% of the particles actually enter the DCA within the upper 1500m, so above the part that is not so well represented in this model (*“Notably, we find that more than 95% of the particles reaching the DCA within six months arrive in the upper 1500 m and hence enter the part of the water column relevant for convection in winter (see DCA MLD definition in Fig. 2c, 3d).” Lines 408-410*). We agree that the evaluation of the mixed layer came too late in the manuscript. We decided to split this figure up and added the MLD map to the surface evaluation (now Fig. 2) and the section of the MLD to the section comparison (now Fig. 3). For a better story line, we decided to switch Figure 2 and 3.

Lines 191-204: “We note here that the model has shortcomings in correctly reproducing deep overflows along the OSNAP East line. Both the deep salinity maximum associated with the Iceland Scotland Overflow Water (Fogelqvist et al., 2003; Fu et al., 2023; Lozier et al., 2019; Våge et al., 2011) and the temperature minimum associated with the Denmark Strait Overflow Water (Dickson and Brown, 1994; Våge et al., 2011) are less pronounced in this model simulation compared to observations.

Lastly, we show a section of potential density with the corresponding mixed layer depth (white line) in February, the month with the deepest mixed layers in this simulation (Fig. 3d). As already shown with the spatial extent of the MLD (Fig. 2c), the deepest mixed layers occur between 41.5° and 39.5°W in the vicinity of the Irminger Gyre. There, isopycnals are outcropping and water convects down to around 2000 m. Even though the location is comparable to observations we want to note here that the model overestimates the MLD according to observations (De Jong and De Steur, 2016; Pickart et al., 2003; Rühls et al., 2021; Våge et al., 2011), maybe related to the deviations in representing the overflow waters. As our following analysis focusses on the upper part of the water column to study the pathways of the main velocity core of the IC and not the convection itself, we consider the location of the DCA more important than the actual depth of the mixed layer. Nevertheless, it should be kept in mind, when interpreting the results.”

Major Comment 2

I lack information about the particle tracking algorithm. Can the authors confirm that it is 3D? More details are needed. I am also surprised that the authors did not discuss the depth at which the particles arrive. Is it reasonable? What concerns me most is the use of different integration times (I understand this is due to the simulation being limited to one year). Particles seeded in September are integrated over nearly a year, while those seeded at the end of the simulation are only integrated over a few weeks. This will bias the statistics. It is reasonable to assume that particles integrated over a few weeks are less likely to reach the DCA or exit the region toward DS or IB than those integrated over a year. This bothers me, especially since the article strongly emphasizes the quantitative aspect of particle distribution (e.g., 61% of particles enter the interior of the Irminger Sea). Can the authors argue that this bias is negligible? I also think that the seeding strategy only allows for the study of a portion of the water masses entering the DCA. As mentioned by the authors, vigorous exchanges between the Iceland Basin and the Irminger Sea north of the OSNAP line are not accounted for in the experimental setup. Why not conduct an experiment integrating particles backward in time (starting in August 2008) to identify the geographic origin of the particles and determine whether the Irminger Current at the OSNAP line plays a major role as a source for the DCA, as postulated by the study?

We thank the reviewer for the thoughts and suggestions. The particle tracking algorithm is indeed 3D. We added that to the text (“To find out whether waters from the IC enter the interior Irminger Sea and potentially the convection area, we evaluated numerical particle trajectories in the MITgcm simulation calculated using the 3D-particle tracking algorithm presented in Koszalka et al. (2013) and Gelderloos et al. (2016).”).

We did add a sentence on the depth at which the particles arrive in the DCA as we agree with the reviewer that this is an interesting point. The text reads as follows: “Notably, we find that

more than 95% of the particles reaching the DCA within six months arrive in the upper 1500 m and hence enter the part of the water column relevant for convection in winter (see DCA MLD definition in Fig. 2c, 3d).” (Lines 408-410)

In line with the comment of reviewer 1, we decided to reduce the particles tracking time and only analyse particles tracks released between 1st of September 2007 and 13th of February 2008. All resulting particles tracks were only tracked for half a year to ensure the same particle track length. Our total particle number reduced to 108030 particles, but all have the same length of 6 months now. As a result, our quantitative analysis hardly changed. The first selection between interior and boundary current particles changed to 39 % BC particles (38 % before) and 60 % interior particles (61% before). The second selection showed slightly more changes that are likely related to the shorter tracking time period. 26 % of the particles entered the DCA (29 % before) and 34 % stayed in the non-DCA area (32 %). All numbers were changed accordingly in the manuscript and also the summary figure was updated (Fig. 7). As adjusting the analysis did not change the major outcome of the paper, we decided against recycling the velocities fields. We decided against recycling the velocity field as it would introduce a spurious jump in the velocity and tracer values. Rendering the trajectories after the jump would make it hard to interpret. Indeed, it would be a good idea when working with lower temporal resolution for example monthly means, but with our high temporal resolution of 6 hours the jump can be quite significant.

Please see the main parts of the adjusted text in the results section:

Lines 17-19: “Of those particles, 39% follow the boundary current circulation and 60% enter the interior Irminger Sea. Around one percent leaves the Irminger Sea through Denmark Strait and across the ridge to the Iceland Basin. Of those entering the interior, about 26% reach the DCA, steered by mesoscale variability.”

Lines 221-222: “We released particles in the first six months of the simulation, between 1st of September 2007 and 13th of February 2008 and track them for six months.”

Lines 241-244: “As we are interested in restratification processes in the Irminger Sea, we do not focus on the small number of particles leaving the Irminger Sea through DS (defined as particles that cross 22°W north of 66°N, black line in Fig. 4a; 0.2 %), and across the Reykjanes Ridge to the IB (defined as particles that cross the Reykjanes Ridge towards the east, dashed green line Fig. 4b; 1 %) within the considered 6-months tracking time period.”

Lines 278-280:” As we are interested in boundary-current-to-interior transport in the Irminger Sea, we focus our analysis on particles that either belong to the BC (39 %, Fig. 4c) or the iIS category (60 %, Fig. 4d).”

Lines 294-296: “From all particles released within the six months tracking time period, 26% reached the DCA (Fig. 4e) and 34% stayed outside the DCA in the interior Irminger Sea (Fig. 4f).”

Lastly, we agree with the reviewer that our study is not a full representation of all waters that reach the DCA. But we want to emphasize that was not the purpose of the study. The goal of our study, as stated in the text, was to analyse where the waters from the IC moorings go to and whether/how much they reach the DCA. Therefore, we think that our seeding strategy is correct to answer this question.

Major Comment 3

The authors adequately cite previous work but could be more comprehensive. For example, [Tooth et al., 2023], [Mercier et al., 2024] identified the Irminger Sea as a key area for MOC variability (lines 30–31); [Perez et al., 2018] demonstrated the role of deep convection in the Irminger Sea in the burial of anthropogenic carbon (line 34); [Piron et al., 2016] studied the extent of convection in the Irminger Sea (lines 65–66); [Piron et al., 2017] examined deep convection in 2015 (lines 69–70), etc..

Thank you, we added the suggested citations to the introduction.