Author’s response to reviewer’s comments

We have carefully reviewed the comments and have revised the manuscript accordingly. Our responses are given in a point-by-point manner below.

Responses to Reviewer 1

This manuscript presents an innovative analysis of recent North Atlantic hydrographic measurements along with ancillary data sets to quantify the overturning in the subpolar gyre.

I think this is a well-written and very interesting paper that significantly contributes to the understanding of water mass transformation in the subpolar North Atlantic. In my opinion, the analysis has two major weaknesses, namely (i) that the perimeter contour for large parts of the domain is oriented along the major boundary current system such that the cross-contour component of the flow is a small residual relative to the along-contour flow and (ii) that the perimeter contour has too coarse resolution to capture important components of the circulation, in particular the overflows through Denmark Strait and Faroe Bank Channel. I am not sure that these two weaknesses can be robustly addressed, but I think that at least a more extensive discussion of these two concerns is necessary. I also have a few other comments that I hope the authors will consider. Finally, I would like to emphasize that I think this manuscript ingeniously utilizes existing observations to address an important and challenging scientific question; I support the effort and encourage the authors to submit a revised version of the manuscript.

General comments:

Most of the major currents of the subpolar gyre boundary current system have substantial flow along the 1000 m isobath, such as the East Greenland (Le Bras et al., 2020) and West Greenland Currents (Pacini et al., 2020), and the dominant isopycnal slope along this depth contour is across the boundary current system. These currents are to varying extent subject to meanders and instabilities (e.g., Prater, 2002; Pacini and Pickart, 2022), which introduce substantial variability in the measurements. Quantifying the cross-slope geostrophic flow from the isopycnal slope along a depth contour that is characterized by substantial and vigorous dynamics is not optimal. Choosing a deeper isobath for the perimeter contour would likely alleviate the problem. While there are other good reasons for choosing the 1000 m depth contour, I think more robust estimates of cross-contour flow would be obtained along a deeper isobath. I will not advocate that the analysis is redone, but would like to see this issue more extensively discussed in the paper.

For the horizontal gridding along the perimeter contour, a resolution of 150 km was used. The coarse resolution may suffice for the large-scale, geostrophic interior-boundary exchange. However, very important contributions to this exchange occurs on much smaller spatial scales, in particular eddies and deep overflows from the Nordic Seas, but also currents such as the Deep Western Boundary Current are not resolved at this scale. While eddies may roughly balance in- and outward fluxes across the perimeter contour, the overflows and the water masses they entrain are crucial inflows into the subpolar gyre that will not have been properly accounted for. Downstream of Denmark Strait, the overflow plume has a spatial scale of much less than 100 km and rapidly descends beneath the 1000 m depth contour (Dickson and Brown, 1994; Girton and Sanford, 2003) – hence the Denmark Strait overflow cannot be the main source of inflow near Cape Farewell.

General response:

Thank-you for the broad and constructive review. We largely agree with the suggested changes and have implemented them where indicated in our responses.

Reviewer 1 highlighted two key concerns with the method. The first concern related to the choice of the 1000 m contour as the domain boundary, as in some regions the intense boundary currents bisect this contour. As correctly highlighted, the cross-contour component of the flow used in this study is small compared to the along contour component, and it is possible that choosing a deeper isobath would diminish this effect as currents may not be so strong.

The choice of the 1000 m isobath was motivated by several considerations, as outlined in the introduction. Beyond the requirement to include Rockall Bank in the SPG definition, the depth of maximum overturning was estimated to be around 1000 m by Hirschi et al. (2020) so this choice also infers the partitioning of upper and lower limb processes. One of the main regions where the boundary currents are offshore of the 1000 m contour is off southwest Greenland (the WGC). In this region the continental slope is very steep and a choice of 2000 m (the limit determined by standard Argo profile depth) or more as the reference isobath does not prevent the boundary current crossing the contour. In fact, when recreating the analysis in VIKING20X using the 2400 m isobath as the reference contour, we found the cross-boundary flows became more intense. This appears to be because the WGC crosses the 2400 m isobath more abruptly than it crosses the 1000 m isobath in VIKING20X. The intense boundary currents also remain offshore of the 2400 m isobath along the western Labrador Sea.

To exclude all the major boundary currents from the domain, we might consider offsetting the boundary contour a set distance offshore from an isobath. However, the convenient geostrophic constraint provided by a constant-depth curtain of data is then lost, and we exacerbate the problem of undiagnosed flow under the data curtain. As suggested by Reviewer 1, we have expanded the discussion of the choice of contour to include some of the points raised here [Mainly Line 403 onwards].

The second concern was with our choice of resolution for the climatology, and whether this resulted in the exclusion of small-scale but dynamically important features such as overflows. We did try different horizontal resolutions, and we thought the 150km horizontal grid size the best compromise between spatial resolution and the available profile density. In particular, we wanted a climatology which could be robustly split into 4 seasons without regions of poor coverage emerging. While the 150 km resolution was a good compromise for the boundary as a whole, Reviewer 1 highlights that some important small-scale features such as the overflows may be lost due to resolution and smooth scale.

To investigate the extent to which the resolution impacted our ability to resolve the overflows, we examined the raw Argo and CTD profiles in the dataset for evidence of the TS characteristics of overflow water at the expected locations of the Denmark Strait Overflow and Faroe Bank Channel Overflow. We found that very few (<10) profiles featured the expected TS properties. One reason for this finding appears to be the geometry of the 1000 m data cut-off relative to the seabed; the near-bed overflows only intersect with the data collection region close where it is in contact with the bed. Increasing the resolution would therefore not substantially increase the prominence of the overflows as they are not being captured in the raw profiles. See the geometry of the data collection region relative to the seabed in Fig. 12 in the manuscript. Note also that choosing a deeper reference contour would not alleviate this problem. We list several other factors which might limit our ability to properly sample the overflows using scattered CTD and Argo profiles in the manuscript.

Further, we argue that even if the overflows were perfectly resolved, a substantial portion of their flow is ageostrophic (as evidenced by the VIKING20X analysis) and so would not contribute to the volume transport estimates. We already stress that the omission of the overflows will have little impact on the overturning findings, as the overturning streamfunction is integrated from the surface downwards, and the overflows are too dense to undergo further transformation in the domain. However, we have expanded the discussion of the potential impact on the heat and freshwater estimates in the revised manuscript [Line 784 onwards], highlighted the role that gridding resolution might have [Line 889] and introduced the concept earlier in the manuscript. [Line 338 onwards]

Specific comments:

Line 25:

Is the minimum overturning in fall mainly comprised of the overflows from the Nordic Seas, which would form a steady baseline with minimal seasonal variability, while the maximum in spring also includes dense water formed within the subpolar gyre? Or is the surface Ekman component also an important source of variability? I think it would be good to specify the cause of this seasonal variability already in the abstract.

Surface Ekman appears to be the main driver of this seasonality. Added a sentence to clarify [Line 27].

Line 36: Including the estimate of 30526 TW across the Greenland-Scotland Ridge by Tsubouchi et al. (2021) would be another very relevant point of comparison here.

Thank-you for the suggestion. Added to text [Line 38].

Line 48: Another important component of the return flow from the Arctic Ocean and Nordic Seas is the surface outflow of Polar Water in the East Greenland Current (de Steur et al., 2017).

Good point, added to text [Line 52].

Line 60: The relatively low impact of water mass transformation in the Labrador Sea on the AMOC was known also prior to OSNAP (e.g., Pickart and Spall, 2007).

Modified text to include this point [Line 63].

Line 64: The importance of water mass transformation north of the Greenland-Scotland Ridge for supply of dense water to the lower limb of the AMOC should not be underestimated (e.g., Chafik and Rossby, 2019).

Added a note clarifying this point [Line 69].

Line 99: Another important dynamical consideration is that, apart from the overflows, most of the sinking occurs along the boundary (Spall and Pickart, 2000; Johnson et al., 2019)

This part of the introduction was removed as part of changes to better frame the results and discussion, as advised by Reviewer 2.

Line 148: If the minimal search radius is 150 km, equal to the distance between grid points, most of the profiles are probably used in more than one grid cell. Did you apply any weighting to emphasize the contributions of profiles closer to the grid point or the perimeter contour?

We did not attempt any weighting in the along-contour direction, or in the distance from the perimeter contour. Added a note in the text to clarify this point [Line 167]. As property gradients are high in the across-contour direction, weighting by distance from the contour may result in unpredictable responses to data scatter. As noted in the next point, we feel that the uncertainty analysis provides a robust view of the errors which might result from the existing gridding approach.

Line 199: This is a great approach to estimate the statistical uncertainty inherent in the data set. Providing the measurement errors that are also inherent in the data set, such that the magnitudes of the statistical and measurement uncertainties can directly be compared, would also be good.

We now include the measurement errors associated with CTDs, Argo and satellite ADT. We found that the scatter in results which might be expected due to measurement error was negligible when compared with the statistical uncertainty [Line 231].

Line 210: Please provide some more details regarding the calculation of the surface Ekman transport. For example, what have you taken to be the depth of the Ekman layer?

For the flux and overturning calculations, the Ekman transports are added to velocities in the top 20 m cell. They therefore act on the corresponding top cells of the gridded temperature and salinity [Line 242].

Line 258: Important flows on relatively small scale such as the overflows from the Nordic Seas and the East Greenland Spill Jet (Pickart et al., 2005) are not properly resolved. Perhaps EN4 at 47◦N is not the sole cause of the imbalance, if these features, along with the Deep Western Boundary Current, substantially contribute to the imbalance?

As discussed later in the manuscript, we found a good qualitative agreement between the calculated geostrophic velocities and those diagnosed in VIKING20X, suggesting that the observations were capturing the important flows across the boundary above 1000 m. The 'remainder' term in the model (including most of the overflow transports) is flow that we would not be able to include using the geostrophic approach, even with perfect sampling.

Line 283: What are the length scales over which the satellite absolute dynamic topography product was smoothed? On line 183 it is stated that smoothing was applied to mimic the smoothing inherent in the hydrographic gridding process. Is the resulting length scale of the eddy kinetic energy consistent with eddies scaled by the Rossby radius, or were all eddies, to the extent that they were represented in the raw satellite record, removed by the smoothing procedure?

These instances reflect different treatments of the satellite ADT product for different purposes. There is no smoothing applied to the ADT prior to computing the eddy kinetic energy or diffusive fluxes. Added text to clarify this point [Line 314].

Line 308: Offshore fluxes of freshwater from the Greenland shelf into the interior Labrador Sea near Cape Farewell (Lin et al., 2018) and farther north where the West Greenland Current encounters steep topography and becomes unstable (e.g., Fratantoni, 2001; Prater, 2002) are likely major contributors to the cold, fresh low-density layer. Both of these processes are primarily eddy-driven, hence postulating that a portion of the West Greenland Current crosses into the interior supbolar gyre may not be necessary. These processes will not be resolved at 150 km horizontal resolution. Given the turbulent nature of these fluxes, it is also not obvious that there will be a consistent geostrophic flux across the perimeter contour.

This is a good point, added text to clarify the probable cause of the cold, fresh intrusion. As you say, these fluxes are primarily turbulent and may not be associated with a positive geostrophic flow so we still state that this may be due to the WGC moving into deeper water in this region [Line 343].

Lines 349 and 491: Most of the Atlantic Water inflow from the subpolar gyre to the Nordic Seas takes place east of Iceland, roughly evenly split on either side of the Faroe Islands (Østerhus et al., 2019). Given the course resolution of the perimeter contour, I think it is more appropriate to ascribe this flow to the Iceland-Scotland Ridge rather than the Wyville Thomson Ridge (note that Thomson is spelled without a p).

Agreed; updated text [Line 394].

Line 351: Note that there is also some flow of Atlantic Water northward through Denmark Strait (Jonsson and Valdimarsson, 2012; Semper et al., 2022).

Thank-you; added to text [Line 395].

Line 352: What is the magnitude of the retroflection of the East Greenland Current near Cape Farewell?

5.1 Sv flow from the EGC into the central Irminger basin (Holliday et al., 2007); added to text [Line 398].

Line 357: An export of 12 Sv from the subpolar gyre to the Labrador shelf is immense. Is this a realistic number? Could this be related to water sinking along the boundary (e.g., Johnson et al., 2019) or merge with the boundary current system (a substantial portion of which appears to be inshore of the 1000 m isobath, Zantopp et al., 2017)?

The description stating that ~12 Sv flowed onto the shelf was misleading. Inference of on-shelf flow in fact stops at ~9200 km, at which point the flow is not onto the shelf but through and over the Flemish Cap. Whilst the core of the boundary current is inshore of the 1000 m isobath, at 53 N it extends 75-100 km offshore of the 1000 m isobath (e.g. Fig. 8 in Zantopp et al., 2017). If we assume this region averages 10 cm s^{-1} (referring again to Zantopp et al., 2017) this suggests several Sv of the boundary current could be within the boundary contour (and above 1000 m) at this latitude. The remainder is gained just south of the OSNAP crossing. The volume flowing south through the Flemish Pass could account for 6-10 Sv (Petrie and Buckley 1996). Added the above transport estimates to the manuscript [Line 405, as part of modified paragraph].

Line 450: Good discussion of missing contributions. While the model has a much higher resolution, how confident can you be that it is able to realistically capture these features?

These small-scale baroclinic features are always going to be challenging for a basin-scale model to accurately represent, but their contributions to the volume budget of the SPG do appear to be reasonably consistent with observation campaigns (e.g. Biastoch et al., 2021, Harden et al., 2016; Jochumsen et al., 2017).

Lines 493, 631, and 696: It is not obvious why there should be a lower layer inflow in the vicinity of Cape Farewell. This is too far south of Denmark Strait to be ascribed to the overflow (Dickson and Brown, 1994; Girton and Sanford, 2003), but perhaps the East Greenland Spill Jet (Pickart et al., 2005) contributes? Please elaborate.

We should probably reiterate that in the context of the Irminger and Labrador Basins, our definition of the lower layer (> 27.54 kg/m3) is still quite light. Examining Figs. 3c and d, it's clear that this density class accounts for all transport below ~150 dbar. Given the water properties in this region are those of the EGC (Holliday et al., 2007), it seems likely that this inflow is at least in part due to the ~5.1 Sv retroflection of the EGC into the Irminger Sea observed by Holiday et al. (2007). As we note in the manuscript, another factor could be the tendency of the EGC to track deeper isobaths west of Cape Farewell as the shelf edge steepens. Added notes to this effect in the manuscript [Line 705, 783].

Line 525: This estimate of advective flux across the Iceland-Scotland Ridge appears to be in reasonably good agreement with the estimate of Tsubouchi et al. (2021).

Thank-you for pointing this out, noted in text [Line 589].

Lines 572 and 726: There is substantial discussion of the high EKE west of Greenland in the literature (e.g., Fratantoni, 2001; Prater, 2002).

Thank-you; added to text [Line 636].

Line 599: The overturning across the Greenland-Scotland Ridge would be another vital point of comparison (e.g., Østerhus et al., 2019; Tsubouchi et al., 2021).

Added to text [Line 665].

Line 618: How does the density surface of 27.30 kg/m3 for maximum overturning compare to similar results from Lozier et al. (2019) and Petit et al. (2020)?

27.30 kg/m3 is substantially lighter than the density of maximum overturning found by Lozier et al. (2019) (27.66 kg/m3) and Petit et al. (2020) (27.55 kg/m3). Added to text [Line 686].

Line 623: This is an important result, which substantially modifies the conclusions of Petit et al. (2020). Without velocity measurements, they considered this water mass transformation part of the overturning in the subpolar gyre, and concluded that more deep-water formation occurs in the subpolar gyre than in the Nordic Seas. You have demonstrated that a substantial portion of this intermediate-density water continues to the north, into the Nordic Seas, where it is further transformed. As such, densification in the subpolar gyre preconditions further water mass transformation in the Nordic Seas and is thereby important for the North Atlantic overturning, but it is not appropriate to ascribe that part of the water mass transformation to overturning in the subpolar gyre, since the water proceeds into the Nordic Seas in the upper layer rather than returning to the south at depth.

Thank-you for highlighting this comparison. We interpret the conclusions of Petit et al. (2020) to be that a similar volume of water is transformed north of the GSR (6.6 Sv) to the region bounded by OSNAP-East and the GSR (7 Sv). These estimates do not seem incompatible with our results. We have modified the text to clarify the role of pre-conditioning but have stated that this is more or less aligned with the findings of Petit et al. (2020) [Line 692].

Line 639: Overflow waters from the Nordic Seas become lighter as they mix with and entrain ambient water masses while descending to the abyss of the subpolar North Atlantic. I do not think there are other processes that can make the overflow waters significantly lighter. In general, the overflow waters are located too deep in the Labrador Sea, where the deepest convection occurs, to be accessed during convection in winter (Yashayaev, 2007). More importantly, for the overflow waters to be modified by convective mixing, the mixed layer would have to be sufficiently deep that it extends into the overflow layer. Since the ocean is stably stratified, the density of the mixed layer would then have to be at least the same as the density of the overflow layer. For this reason, deep convection would not make the overflow water lighter.

Thank-you for pointing this out. Modified text to state that this is most likely due to mixing and entrainment [Line 713].

Line 642: The deepest overflows are generally considered denser than sq = 27:8 kg/m3 (Dickson and Brown, 1994).

Modified threshold and added reference to text [Line 717].

Line 649: This is a remarkably swift export of newly formed dense water, in particular considering that most of the transformation takes place within cyclonic gyres (e.g. Lavender et al., 2000; Straneo et al., 2003).

Yes, it seems a more likely cause of the springtime overturning maximum is that winter surface Ekman forcing acts to suppress overturning, shifting the peak to the spring, in a similar manner to that seen in OSNAP (Li et al., 2021a; Petit et al., 2020; Petit et al., 2021). If we remove surface Ekman forcing from the volume budget, the overturning peak occurs in winter instead. Changed text to reflect this [Line 725].

Line 656: It is unclear to me how virtually all of the subpolar mode water is exported before undergoing further transformation to dense water, in particular considering that the residence time within the cyclonic gyres where most of the water mass transformation takes place may be on the order of years (Straneo et al., 2003). Please elaborate.

Only half the SPMW is exported. This sentence was confusing and has been rephrased [Line 736].

Line 664: Dense-water formation is not considered a "driver" of the AMOC (Kuhlbrodt et al., 2007).

Changed to "important source of dense water masses for the lower limb of the AMOC" [Line 745].

Lines 673, 681, and elsewhere: Adding uncertainties to these estimates would be good.

Added error estimates where suggested, and at other appropriate locations [e.g. Line 755, 759, 764].

Line 714: Most high-latitude currents with substantial barotropic components closely follow density contours (e.g., Nøst and Isachsen, 2003). Instabilities in the West Greenland Current and formation of Irminger Rings may be a more likely source of this signal (Fratantoni, 2001; Prater, 2002).

Agreed, changed text and added references to reflect this [Line 800].

Line 727: While deep convection at the boundary of the Labrador Sea may not have taken place in the 2000s, the boundary current system was ventilated during the more severe winters of the early- and mid-1990s (Pickart et al., 1997).

Thank-you for highlighting this. Added to text [Line 817].

Line 758: I think it would be great to relate the overturning in depth space to the corresponding results obtained for the density space calculations. That would also integrate this section better within the rest of the manuscript.

The depth of density contours varies widely around the SPG above 1000 m so it is hard to generalise the overturning in depth space for the entire SPG boundary. We have enhanced the visibility of key overturning isopycnals in Fig. 3d (geostrophic velocity) and added them to Fig. 3c (density). We have also added a label to the overturning stream function for the 47N transect (Fig. 9c) to indicate that the 27.7 contour is at approximately 1000 m.

Line 774: This is the first proper discussion of the unresolved overflows. This is a major drawback of the coarse perimeter contour and likely has a substantial impact on the results. I think this discussion needs to be introduced much earlier in the manuscript.

As previously discussed, the inability to resolve the overflows is primarily a sampling problem and not a resolution problem. We have increased signposting to this section throughout the manuscript [e.g. Line 126, 784] and have also noted the absence of the expected overflows in the Hydrography section of the results [Line 338]. There is also an introduction to the missing overflows and other ageostrophic processes in the VIKING20XS analysis (Section 3.3).

Line 782: More recent estimates of the Denmark Strait Overflow Water transport converge at values around 3.2- 3.5 Sv (Harden et al., 2016; Jochumsen et al., 2017). This transport across the sill may then approximately double by entrainment as the dense water descends toward the abyss (Dickson and Brown, 1994). As such, VIKING20X may not be overestimating the overflow, although even in a relatively high-resolution model the overflows are probably not simulated very realistically.

Good point; added to text [Line 875].

Line 788: More recent papers have made significant progress improving our understanding of the variability in Denmark Strait (Spall et al., 2019; Lin et al., 2020).

Thank-you; added references [Line 884].

Line 792: All of the reasons discussed in this paragraph may contribute, but the main cause of the poorly represented overflow water must be the low horizontal resolution along the perimeter contour.

Whilst we partially agree, even In the raw CTD data, relatively few profiles capture true DSO water. Presumably unless the profile is very close to the boundary contour the overflow passes below the 1000 m curtain in our analysis. We could increase the proportion of 'overflow' profiles by reducing the offshore search area, but this would have a detrimental effect on the data density. But we have added a note in the text to acknowledge this point [Line 889].

Line 802: Perhaps specify here that the dense water exiting the subpolar gyre in the North Atlantic Current continues to the north, into the Nordic Seas. As previously stated, this is an important result that demonstrates the importance of the subpolar gyre in preconditioning overturning in the Nordic Seas and thus modifies the conclusions of Petit et al. (2020).

Thank-you, modified text to clarify [Line 901].

Line 812: The net sinking that occurs along the boundary (Spall and Pickart, 2000; Johnson et al., 2019) may be another such process that is important to better understand, but difficult to address using this approach.

Good point; added to text [Line 911].

Detailed comments:

Lines 9, 105, 659, and elsewhere: Oceans and Basins should be capitalized, also in plural.

Comment addressed [e.g. Line 9, 117].

Lines 16, 299, and 485: Biscay is a province of Spain, I think Bay of Biscay would be more appropriate.

Comment addressed [Line 291, 499].

Line 28: The acronym NAC should be defined at first usage.

Comment addressed [Line 25].

Line 48: "Arctic" by itself is an ill-defined term. Arctic Ocean would be better.

Comment addressed [Line 51].

Lines 66, 134, 135, 138, 423, 460, 489, 492, and elsewhere: I would have added at least one "the" to these lines.

Thank-you for highlighting these omissions [e.g. Line 71, 148, 149].

Line 93: "Deep mixing" is ambiguous. Do you mean convection=deep vertical mixing?

Text removed in response to Reviewer 2 comments.

Line 149: A search radius cannot be negative.

Agreed. Text amended [Line 166].

Line 198: Is not an integral by definition cumulative?

Agreed. Text amended [Line 218].

Line 210: Data are typically considered plural.

Text amended [Line 238].

Line 223: It should be: “...for an improved representation...”

Comment addressed [Line 253].

Line 225: It should be: “...show that it realistically...”

Text amended [Line 255].

Line 253: It should be: “...surface Ekman transports capture...”

Text amended [Line 283].

Line 257: The Deep Western Boundary Current should be capitalized.

Text amended [Line 287].

Line 275: It should be: “...Using ERA5 monthly means...”

Text amended [Line 305].

Line 290 and elsewhere Scale-dependent is a compound modifier that should be hyphenated.

Text amended [e.g. Line 320].

Line 316: The comma should be removed.

Text amended [Line 359].

Line 319: The Labrador Current should be capitalized.

Text amended [Line 362].

Lines 366 and 393: Transport should not be capitalized.

Text amended [Line 423, 451].

Line 382: The unit Sv is missing.

Text amended [Line 439].

Line 463: A comma is missing.

Text amended [Line 522].

Line 592: It should be: “...water mass transformation...”

Text amended [Line 659].

S4: Gulf Stream should be capitalized.

Text amended.

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Responses to Reviewer 2

This paper creates a novel climatology of the subpolar North Atlantic around the 1000 m isobath and across 47N and discusses properties and fluxes across and with this region. The techniques used are interesting and the climatology looks great. I have reservations about the use of EN4 at 47 N that I think could be investigated further. I think the discussion and observations are good and interesting.

My major comment on the paper is that it could be much more focused. The introduction covers much more material than the results address. The question that the climatology and calculations are addressing could be framed much more succinctly. Likewise in the discussion and conclusions, there needs to be a closing of the loop back. E.g. the discussion around Fig. 12 was very interesting but I wasn't sure what question this was addressing.

I have a long list but these are minor comments, the only major comment is a tightening up of the framing of the results.

General response:

Thank-you for the review. In response to the main concerns of Reviewer 2, we have shortened and restructured the abstract [e.g. Line 15-21] and introduction [e.g. Line 49-60, 85-112], improved the framing of the key findings and increased the signposting to the main points raised in the discussion [e.g. Line 126, 180, 338]. Some of these changes also incorporate suggestions by Reviewer 1. With regards to linking Fig. 12 back to the introduction, in tightening the introduction we have hopefully set the scene more effectively for this part of the discussion. As our method was quite novel for the region and some of the findings could not be anticipated, we felt that it was reasonable to extend the discussion beyond the questions set by the introduction. We largely agree with the minor comments and have implemented the suggested changes where indicated.

Minor comments:

Why were Argo velocities not used? The dataset seems dominated by Argo Fig 2b

For the main investigation (across-boundary transports) we investigated the ANDRO dataset but found that the interpolation scheme was unsuitable for the boundary. The relatively high along-slope velocities also meant that any errors in inferred velocity from Argo trajectories would be magnified when the small across-slope component was investigated. Similarly, for the estimate of bottom Ekman flow we found that ANDRO was not suitable for the analysis of boundary currents due to the proximity to the continental slope.

I think the abstract is too long and could be shortened to 2 paragraphs. Too much intro material in paragraph 3 of the abstract especially.

We have condensed the abstract down to one paragraph.

L32. This definition of the AMOC is not correct: the AMOC (uniquely) transports heat across the tropics from the South Atlantic

Agreed, modified text to clarify [Line 35].

L53, no need to complicate with the drifter results

Agreed, this point has been removed from the restructured introduction.

L58, canonical -> generally accepted

Text amended [Line 61].

L64, to the mean what? This line throws the paragraph out. If you're considering processes north of GSR, then your first sentence should consider these also i.e. GSR overflows + entrainment in addition to Lab Sea processes are fundamental to AMOC functioning.

Text amended also in response to a similar comment by Reviewer 1 [Line 69].

L67, 'they' is ambiguous here. I presume you mean Lab Sea density anomalies?

Text amended [Line 72].

L70, don't see why you're bringing in subpolar mode water

Restructured paragraph for clarity [Line 74-78].

L78, add 'in the eastern basin'

Sentence removed [Line 85-89].

The introduction is very general. It should be more focused to frame this study rather than a general subpolar gyre introduction.

As discussed in the general response, we have shortened and restructured the introduction to better frame the research questions.

Fig. 2. > Radon transform for analysis of propagation speeds in Fig. 2.

As for the ANDRO product, the proximity of the boundary makes the analysis of float propagation speeds problematic. For the bottom Ekman order of magnitude discussion we only require a crude estimate, and the radon transform approach would only serve to narrow the uncertainties around the supplied figure.

> Not much data prior to 2008.

Argo data is sparser before 2008, but the regular CTD transects which bisect the dataset provide reasonable continued coverage. As Argo was still being populated in the 2000s, there will inevitably be some temporal bias towards later years, but we feel that the early 2000s still makes a valuable contribution to the climatology.

> Higher propagation speeds upstream of FSC.

We don't see much evidence of higher propagation speeds upstream of the FSC. Floats in the Rockall Trough are rarely entrained in the European slope current as it is too narrow and shallow for a typical Argo drift profile.

Propagation speeds are only relevant for the Argo data, not the CTD data (unless you're telling us about the speed of the ship). Can ship CTD data be removed from Fig. 2b.

We have now coloured the ship CTD data distinguish it from the Argo profiles in Fig. 2a and b.

L144. What is the justification for using a much longer search radius in the along bathymetry direction than cross bathymetry, limited to 75 km?

Cross-bathymetry property gradients are much greater than along-bathymetry gradients. As we were treating the dataset as a nominal transect along the 1000 m isobath (to enable geostrophic constraints and volume continuity), we wanted to minimise unnecessary distance from the contour. Added a sentence in the text to justify this choice [Line 161].

L160. It's not so surprising that EN4 and Argo agree closely as the Argo profiles are in EN4. Did you compare with a ship hydrographic section? Are the (complex) fronts and current meanders across this section captured in EN4?

There are no regular hydrographic sections across 47 N but as discussed in Section 2.4.5 we did compare with equivalent observations and model transects and found that the sub-1000 m geostrophic velocities calculated from EN4 data overestimated the strength of the Gulf Stream at depth and underestimated the Deep Western Boundary Current and other southward flows across 47 N. This was the main reason for requiring a conservation constraint to close the volume budget. We have improved signposting to Section 2.4.5 to clarify our treatment of the EN4 data [Line 180].

L174. A sensible constraint. What was the reference velocity and how much transport does it amount to in total? Please state in the paper.

We state the reference velocity and transport in Section 2.4.5 where this constraint is described in more detail. Improved signposting to this section [Line 180].

L178. The ADT requires an estimate of the geoid, which can be uncertain in the open ocean. How much do your results depend on the mean dynamic topography?

The ADT accounts for about 60 % of the variance for the heat and freshwater fluxes, but only 30 % of the variance for the overturning results. Added this information in the 'estimation of uncertainties' paragraph, Line 227. In general, we would hope that our results would be robust to local inaccuracies in the geoid because we accumulate flows over large horizontal scales.

L190, could I suggest using l or s instead of x for your along contour co-ordinate. X is very frequently used to mean zonal direction.

We have more explicitly stated the meaning of x in this context [Line 211]. As this coordinate system is used throughout the study (rather than switching back and forth with the zonal definition of x) we feel that it is reasonable to keep this notation.

L195, define Q, v in equation. Suggest using Qv to match later equations.

V is defined on Line 208. Defined Q as suggested [Line 213]. The use of Q for volume flux is often used in similar studies and seems compatible with our notation in later equations, so have not changed to Qv.

L230, did the volume conservation constraint applied in the observations work in the Viking model?

As discussed in Section 2.4.5, the constraint was necessary in the observations because sub-1000 m geostrophic velocities calculated from EN4 data overestimated the strength of the Gulf Stream at depth and underestimated the Deep Western Boundary Current and other southward flows across 47 N when compared to dedicated observation campaigns and model studies. This appears to be primarily due to data coverage and resolution limitations of EN4. While model geostrophic velocities mimicked our methods by referencing the model sea surface, the corresponding property gradients at depth did not suffer from the same resolution problems. The correction velocity required to balance the model geostrophic flows would therefore be smaller than that necessary for the observations.

L282, I don't find the overbar helpful notation

The overbar notation is commonly used to depict the time averaging necessary for EKE computation.

L295, counter-clockwise -> cyclonic

Text amended [Line 324].

Fig4: fabulous figure. Please add colorbars.

Thank-you. Added colorbars to Fig. 4.

L297. I think ‘negative’ deserves more explanation: it means going to a higher density in a cyclonic direction?

Modified text to clarify [Line 328].

Fig 5a. I’m not sure about arrows here. The arrows don’t point in the direction of the current. They’re constrained to be perpendicular to your section.

We feel that the arrows are helpful for contextualising the 2D transport figures but agree that they could give an incorrect impression of direction. Added the following text to relevant figures: “Quiver arrows show magnitude of geostrophic transport perpendicular to the section.” [Line 424, 452]

L346. Do you mean Goban Spur or the Porcupine Bank? It looks bigger than GS to me.

Agreed, changed to “Porcupine Bank”. [Line 391]

L357. I’m struggling with export and a negative number in one line. ‘Export of 12 Sv’ or ‘transport of -12Sv’?

Very unhelpful double negative: thank-you, amended [Line 408].

Fig. 6a is hard to read the arrows. Really interesting breakdown of Ekman component. Why not the same colours for the geostrophic? Fig 5a?

Reduced the line width on the arrows on Figs. 5a and 6a to hopefully improve clarity. The Ekman component (Fig. 6a) has clear seasonality, so the magnitude of the arrows is distinct. By contrast we found that the geostrophic component (Fig. 5a) had little seasonality, and four arrows for each grid cell cluttered the plot without providing much insight.

Fig. 7. I like this a lot. Very convincing.

Thank-you for the comment.

Section 3.4. I need more context here. This overturning is different from say the OSNAP estimate as it’s overturning around a closed contour around the subpolar gyre.

The phrase “overturning divergence” has been used by other studies to signpost this distinction between overturning within a closed contour and an open section such as OSNAP. While it doesn’t strictly make sense to discuss the divergence of a non-vector quantity, it is useful phraseology. Rather than use this phrase throughout the paper, we have added a note to clarify the meaning of overturning in the context of this study [Line 536].

Could you add the OSNAP mean to Fig. 9 for context? The overturning in this calculation occurs at a lighter density seems to be the key difference (OSNAP 27.5-27.7, here 27.3).

OSNAP mean is 14.9 Sv @ 27.66 kg/m3. As this is mentioned in the text (more prominently in response to Reviewer 1’s comments) and is a different measure of overturning it doesn’t seem necessary to adjust the axes away from the observations to make this comparison.

As this is a very OSNAP inspired paper—could you break the streamfunctions into an analogue of OSNAP east and OSNAP west?

A streamfunction for subsets of the boundary would not have the constraint of volume conservation so would be dominated by accumulation or loss of water driven by net volume flux rather than true overturning. We included 9b and c as they illustrated the contributions of the boundary vs. 47N transect, but we do not think that subsetting the OSNAP regions would be very informative.

Similarly, I would suggest adding OSNAP estimates of heat + fw flux to Fig. 10. You get half the heat flux and ¼ of the fwater flux of OSNAP.

This is an interesting suggestion, but we feel that this addition would detract from the figure. Our results are not strictly comparable to OSNAP because OSNAP Heat and FW fluxes are for the entire region north of the OSNAP line (i.e. all of the Arctic Ocean). Our results are effectively a divergence of heat and FW within the boundary of our domain. If we were to make a direct comparison to OSNAP it would be a residual telling us what happens to the north of OSNAP / our northern boundary. However, as our northern boundary is not the OSNAP line it would not be a meaningful comparison.

L585. I don't agree that's what you're doing! Specifically you've calculate the flux across the 1000m isobath + 47 N. I think you need to say that you've built in a definition of interior and exterior at least.

Agreed, added "between the interior and exterior of the SPG" to clarify [Line 644].

For the discussion, a visual summary would be very useful. It's hard to keep all the numbers in mind.

This was the motivation behind Fig. 12 in the discussion. We have improved signposting to Fig. 12 [Line 126].

I like Fig. 12 and the discussion that goes with it.

Thank-you for the comment.