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 and introduction, improved the framing of the key findings and increased the signposting to the main points raised in the discussion. Some of these changes also incorporate suggestions by Reviewer 1. We largely agree with the minor comments and will implement 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 will shift the focus of the abstract towards the key points and shorten in the revised manuscript.

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.

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.

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.

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

Text amended.

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

Restructured paragraph for clarity.

L78, add 'in the eastern basin'

Text modified.

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.

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.

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.

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, Section 2.4.2. 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 made it clearer that x is along-contour. 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.

Defined Q and v as suggested. 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

Changed to subscript "ref".

L295, counter-clockwise -> cyclonic

Text amended.

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.

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."

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

Agreed, changed to "Porcupine Bank".

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.

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.

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.

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 throughout the paper.

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

Thank-you for the comment.