Review of “Consistent picture of the horizontal circulation of the Atlantic Ocean over three decades” by Cainzos et al.

The authors use an inverse model applied to hydrographic sections from 45°S to 60°N in the Atlantic and repeated over 3 decades to estimate the circulation and the volume, heat and freshwater transports associated with the main currents. In addition to quantifying these quantities from North to South over three different decades, a major conclusion of the study is that the circulation has not changed significantly over the three decades. The analysis required considerable effort. It is relevant and has clear figures and, in general, adequate reference is made to previous work.

I do, however, have a number of comments that need to be adequately addressed by the authors before the manuscript can be considered for publication.

***** Major Comments *****

Comparison of the results of the inverse model with previously published studies shows that the inverse model gives satisfactory results in most regions except at subpolar latitudes where the circulation is more barotropic. In the North Atlantic subpolar gyre, the transports of the western boundary currents are strongly underestimated by the inverse model. Along 45°S, the Zapiola anticyclone is not resolved. This is a well-known defect of this type of inverse model based on an a priori state with a zero velocity reference level selected between two water masses and which fails in barotropic current regions where the zero velocity reference level is a poor approximation of reality. This defect can only be remedied by the use of direct current measurements. The problem is well described in Alvarez et al (Journal of Marine Research, 2002). In the subpolar gyre, the transport estimates subsequently produced by Lherminier et al. (Journal of Geophysical Research 2007 and Deep Sea Research, 2010) or Holliday et al. (Journal of Geophysical Research 2018) have all combined direct current measurements and hydrographic data. This is also the case for the transport estimate at 45°S proposed by Saunders & Bacon (Journal of Physical Oceanography, 1995). The consequence is that the circulation at the northern and southern boundaries of the inverse model is incorrect, with artefacts related to the poor estimation of the transport of barotropic currents (e.g. a southward transport of DSOW along the western side of the Reykjanes Ridge). The authors need to remedy this problem. There is also the question of the impact of this problem on the estimation of transport at other latitudes.

Thank you for your comments on the inverse model. As you mention, inverse solutions may have some issues resolving the circulation at certain areas where less direct measurements can be used as constraints. For example, the deep-water transport out of the Labrador Sea has a strong barotropic component (Fischer et al., 2004, 2010; Zantopp et al., 2017) that cannot be resolved with inverse models A possible solution to estimate this barotropic component would be using LADCP or SADCP direct velocity measurements (Hernández-Guerra and Talley, 2016; Hernández-Guerra and Joyce, 2000; Casanova-Masjoan et al., 2018; Arumí-Planas et al., 2022). However, these kinds of data are not often available. We have included this discussion in the manuscript, in the ‘Methods’ section regarding the inverse model, as well as for some of the comparisons with literature.

Lines 170-175: “Moreover, inverse models are able to resolve the circulation satisfactorily in most regions, except at subpolar latitudes where the barotropic component of the current is strong and the velocity at the reference level from the inverse model does not take into account this barotropic velocity (Álvarez et al., 2002). Therefore, mass transports in these regions can be underestimated. This issue could be resolved with the use of LADCP/SADCP direct velocity measurements capturing this barotropic component, but these data are not often available (Hernández-Guerra and Talley, 2016; Casanova-Masjoan et al., 2018; Arumí-Planas et al., 2022; Lherminier et al., 2007, 2010; Holliday et al., 2018).”

Lines 556-5457: “These differences may arise due to the strong barotropic component observed for this current that inverse solutions fail to resolve.”

The results of the inverse model are interpreted as being representative of a decade. While this assumption may be acceptable for integrated coast-to-coast quantities for which the mesoscale contributions may have been averaged, I do not believe that this can be the case for the estimation of quantities associated with currents of limited regional extent. The authors are aware of this and acknowledge for instance that their results may be biased by the seasonal cycle, as for their discussion of the Canary Current transport. Additionally, the inverse model is forced by an Ekman transport calculated for the time of the cruise, which seems to contradict a search for a solution representative for a decade. Shouldn't a decade-averaged Ekman transport have been used? The authors need to find a way to present their results without interpreting the variability of individual currents as decadal variability (or spend more time convincing the reader that their solution has been derived in such a way that daily to interannual variability has been filtered from their results). In this context, the significance of the errors on te transports determined by the inverse model should be clarified. Should they be interpreted as an estimate of sub-decadal variability and if so why?

The reviewer raises some interesting points. Regarding the use of the Ekman transport, there is no consensus on the best choice of averaging time for Ekman transport when exploring the differences in transport between different occupations of a section. The error analysis of Ganachaud (2003a) motivates the use of long-term mean Ekman transport when assuming that the synoptic section is representative of the long-term mean circulation. The Ekman transport choice has been previously studied by one of the coauthors (Hernández-Guerra and Talley, 2016), who found no significant difference in the model results using the Ekman transport of the time of the cruise and the year average due to the fact that the inverse model adjusts the Ekman transport. That is the rationale behind choosing the time of the cruise to calculate the Ekman transport. We sustain that the geostrophic calculation of each section is balanced by the Ekman transport for the surface layers for the time of the cruise.

The second point relates to the presentation of our results without considering variability as decadal. Our inverse models use multiple sections carried out at different times of the year and different years, so that it could be understood as an average result for the years used in each section, filtering out the variability. However, this is a controversial aspect, so we have made changes in the manuscript to interpret our results in terms of their individual variability and not as a comparison with a decadal average, included as part of the methods section.

Lines 157-169: "The Gauss-Markov estimator is applied to solve this highly underdetermined system of equations (Wunsch, 1996) with a minimum error variance solution from the initial estimates of the unknowns - the velocities at the reference level (**b**) and the corrections to the Ekman transport (ΔT_{Ek}). To solve it, we first need a priori estimates and uncertainties that give an initial approximation to the actual value. Despite obtaining similar results, this study provides smaller uncertainties than other global inverse solutions (Ganachaud, 2003) and decadal studies (Fu et al., 2020). This was achieved by using physical constraints with a simpler model with only the velocities at the reference level and the Ekman adjustments as unknowns. However, we have to be aware that there are some limitations associated with inverse modeling of hydrographic data, as inverse models with single-section snapshots are presumably subject to aliasing (Frajka-Williams et al., 2019; Wunsch and Heimbach, 2006). Therefore, each inverse solution could be interpreted as representative of a relatively short time interval (Fu et al., 2020) or could give information of monthly variations of the AMOC (Bryden et al., 2005). Ganachaud (2003a) refer to their estimates as time-average transoceanic transports with realistic uncertainties, although they acknowledge the temporal sampling problem inherent to the discrete sampling of hydrographic data. Therefore, the validity of hydrographic data to reconstruct climatological estimates is an open debate."

And, finally, we have expanded on the meaning of the uncertainties obtained from the inverse solution with the Gauss Markov estimator. These uncertainties arise as an intrinsic solution from the system of equations applied to all the sections, obtaining estimations for the unknowns that satisfy this system and, therefore, cannot be interpreted as only an estimate of sub-decadal variability. These uncertainties respond, similarly to the actual solutions of the system, to values with variability that have been averaged out.

A major point of discussion in the manuscript is the transport of the different currents. The way in which the geographical boundaries (horizontal and vertical) of the different currents are defined is not made explicit, although this is a crucial point for the interpretation of the results. The reader needs to know whether from one latitude to another the geographical boundaries of the currents have been chosen in a relevant way to allow a meaningful comparison. Should recirculations be included in the transport estimate of a current (this would seem to be the best option to allow a comparison of transport across latitudes without a bias being introduced by the existence or not of recirculations)? In some places, the choice of the geographical limits of the currents goes against the definitions usually chosen in the literature, as is the case for the North Atlantic Current (NAC) for which the definition chosen in this manuscript only includes a small part of the NAC compared to the choice made by Holliday et al. (2018) for example, making it difficult to compare it with 48°N where the whole NAC is taken into account. The authors should explain the criteria used to define the geographic boundaries of the different currents and how they ensure the meridional consistency of the different transport estimates.

Thank you for your clarification. As per your and Reviewer 1's comments, we have included in the manuscript the criteria we have used to identify the currents described. Basically, our definition for the longitudinal ranges for each current is based on the eastward accumulated horizontal transport, generally depicted in the middle panels of figures 5 to 9. For each of the layers defined between neutral density (γ^n) surfaces, we observe their behaviour on the area where the literature has often situated each of the currents studied here. We then define the extension within certain pairs of stations that present a similar slope in the eastward horizontal accumulated mass transport and delimit their range in the vertical by neutral density interfaces with the same flow direction. Therefore, we present a more experimental approach attending to the mass transports obtained between each pair of stations for each of the 11 neutral density layers.

Lines 190-192: "The longitudinal extension of each current has been defined based on the eastward accumulated horizontal mass transport, defining the chosen station pairs with a consistent slope in the accumulated horizontal mass transport. The vertical extension is ascribed with the net mass transport integrated over the chosen station pairs with the same flow direction."

The summary figures presented at the end of the manuscript include only part of the meridional transports through the different hydrological sections. The diagram presented in Figure 7 do not conserve volume. For example, summing all the volume transports through the OSNAP-E section, the residual volume transport is -12.8 Sv, which is greater than any of the individual contributions. Major circulation elements are missing! This should be corrected so that an approximate volume balance be achieved in the circulation sketch of Figure 7. This also poses a problem when interpreting the freshwater and heat fluxes of the individual currents, which must be interpreted at zero volume transport.

The analysis of the reviewer is right. The results presented here do not show the volume conservation across each of the sections, but it is nor the aim of the study. We focus only on the main features of the circulation and main currents. The transport needed to conserve the total mass transport lies in the ocean interior. We have acknowledged this issue in the manuscript:

Lines 192-195: "In this study, we have focused on the main currents, restricted to certain longitudinal and vertical regions. Therefore, no mass transport balance can be obtained only from these main features of the circulation. The mass balance within a transoceanic section could be obtained after accounting for the transport over the ocean interior."

The manuscript presents weighted temperature and salinity transports for each of the currents. No conclusions are drawn from these values. It would seem that if the manuscript were to include an analysis in terms of water mass it would be more appropriate for it to be used to determine the geographical limits of the currents.

Thank you for the comment. We agree with the reviewer - unfortunately we were not able to obtain any conclusions from these values. It could be a very interesting and useful addition, but to do that, we would have to change the focus and methodology of our study. Your suggestions will prove useful in future studies, where we will take into account this comment and profit from these data.

**** more minor comments ****

Lines 16 to 18 : Referring to Figure 2, I find a net upper ocean transport through 45°S of 2 Sv for the upper ocean. Noting that 61 Sv enters the South Atlantic Ocean in the upper layer, without noting this close to zero net (coast-to-coast) transport is misleading as it neglects outflowing waters. This illustrate the problem of solely considering the transports of individual currents without placing them in a wider context. The same remark can be made for the 55 Sv exported southwards through 45°S. What is the significance of this number given that the net coast-to-coast transport is much lower (Figures 5 and 6)?

We were trying to illustrate the influx or outflux of water through individual currents in the Atlantic, describing the differences in the horizontal transport across a section. As you mention, this value is not representative of the net transport across a section that can balance out a strong boundary current with the ocean interior. However, in our previous paper in GRL (Caínzos et al., 2022), we have analyzed the results for the Atlantic Ocean in these terms, and, therefore, our focus for this study was describing the main currents contributing to the transport across a section.

Line 102 : what about heat transport ?

Thank you, that was a mistake on our part, we meant 'for either mass, heat or freshwater'. We have changed that in the manuscript now.

Line 109 : I'm not sure what you mean by "geographical structure".

We thank you for pointing out this issue. We wanted to manifest the difference between the text and figures. In the figures, currents are depicted from north to south. Contrarily, in the text, the currents are described following their path. To avoid misunderstandings, we have replaced the wording in the manuscript.

Lines 107-109: "These currents are discussed in this study following the direction of their flow, from their origin to their destination, to be able to compare how they might change on their way. However, the currents in the figures are depicted from north to south."

Line 118 : 9 should be)

Thanks, we have changed it.

Lines 130-132 : I recommend computing the Ekman transport for each pair of stations so that each Ekman transport value is ascribed to the local density (and not a section averaged density what can create bias when densities are outcropping along the section)

We thank the reviewer for their recommendation. In our inverse model, we have also included outcropping layers for the Ekman computation, as stated in the manuscript:

Lines 132-133: "If outcropping is found at the surface, we measure the percentage of each layer at the surface and then associate these different weights to the Ekman transport for each outcropping layer."

Your comment about considering Ekman transport for reach station pair is an interesting suggestion. However, the number of unknowns in the system of equations would increase significantly, from one for each section to around 100-150 (according to the number of pair of stations) per section. Therefore, the system would be even more underdetermined, and would result in a solution after applying the Gauss-Markov method with higher uncertainties for the solutions.

Line 145 : In your manuscript on the anthropogenic carbon, you mention conservation of biogeochemical properties and here only volume and salinity. This is confusing. Are you using a different inverse solution. If yes this is not reasonable and I strongly recommend that you present here an inverse model solution that is consistent with previous publications.
In this case, we have followed the solution of the inverse model published in GRL (Caínzos et al., 2022b), which is the one being cited in the manuscript. In this inverse model, only mass and salt were conserved. In a subsequent article published in GBC (Caínzos et al., 2022a), conservation of biogeochemical properties was included into the inverse model, as they were studying the distribution of anthropogenic carbon in the Atlantic. These results show a circulation pattern that is not different from the one obtained considering just mass and salt conservation.

Lines 149-151 : This is a repeat.
Thank you for pointing this out – maybe the text in the manuscript was not written appropriately. This is not a repeat, since in these lines we are specifying the conservation applied to each single layer of neutral density and, previously, to the net transport integrated over all the vertical layers. We have tried to rewrite to mark this difference.
Lines 146-151: “Mass is conserved for the whole box for all station pairs of both sections and considers the Ekman corrections for each section. Moreover, to define the continuity of mass transport for each single layer, conservation of each layer was imposed between both sections, with Ekman correction in the outcropping layers. Regional constraints, based on direct current measurements and topographic features, are applied to each section, despite having different station pairs and neutral density layers affected. In addition to mass conservation, we have also constrained the salinity content of each single section.”

Equations (4) : Some notations are not defined. What is the value of the reference salinity ?
The reviewer is right, and our definition of Freshwater flux was incomplete. We have modified equation 4 to use the same notation as equations 2 and 3 and defined the notations used. The reference salinity is the area-weighted section average.

Lines 187-189: “$FW = -\frac{T_i^M S' - \int \rho \, S' \, (v_r + b) \, dA}{S_0}$, (4)
where T_i^M is the interbasin mass transport, i.e., across the Bering Strait (-0.8 Sv), S’ is the salinity anomaly and S_0 is the area-weighted section average.”

Lines 174-175 and 185 : These numbers are also compatible with those of Maamaatualahutapu et al. (Journal of Marine Research, 1998).
Thank you for this reference. We have included it in the manuscript.
Lines 218-219: “Other estimations from inverse solutions using hydrographic data have found slightly larger results (45 ± 7 Sv by Maamaatuaiahutapu et al., 1998; 42.7 ± 6.5 Sv by McDonagh and King, 2005).”

Lines 200-201 : The recirculation immediately east of the Brazil Current is of the same amplitude as the Brazil Current itself. How should this structure be interpreted? Could this be an eddy? Which quantity is more suitable for comparison at another latitude, the transport of the southern branch or the sum of the transport of the southern and northern branches? This point deserves discussion and the choice made needs to be justified.
The reviewer is right – in some cases the Brazil Current is of the same magnitude as its recirculation. However, we interpret the Brazil Current as the permanent feature of the western boundary current that presents a continuation over a large latitudinal range. Therefore, we

compare the values of the actual Brazil Current and not the net transport associated with the possible recirculation. Moreover, as the reviewer suggests, we are not able to identify if this recirculation is part of an eddy.

Lines 216-217 : the observed variability, determined from snapshots, cannot be attributed with certainty to decadal variability.

Following also your second main point, we have included the caveats of the inverse method in section 2.2 and discussed their use as decadal averages of the circulation. To acknowledge the issues regarding this assumption, we have modified the manuscript, as shown in the major comment above.

Line 218 : This is a pattern consistent … a reference is needed to support the statement.

We have rephrased to remove this sentence.

Lines 254-257: "The BrC at this latitude presents similar structures for the first two decades (**Figure 5 i, j** and **Table 2**), with a decreasing southward transport among decades and a sharp decrease for the last one (-26.7 ± 1.6, -22.2 ± 1.5 and -9.5 ± 0.7 Sv for the 1990-99, 2000-09 and 2010-19 decades, respectively; **Figure 2**). Similar tendencies appear for both heat transport (-1.63 ± 0.09, -1.31 ± 0.08 and -0.64 ± 0.04 PW; **Figure 3**) and freshwater flux…"

Line 236 : "appear to decrease among the first decades" … I think that you do not have the data set to ascribe the variability of regional currents to the decadal time scale. I stop at this point to comment in detail on the subject, but the whole manuscript needs to be revised to take this point into account.

As mentioned above, we have changed the manuscript accordingly.

Line 244 : You should explain why your western BeC boundary (2.9E) is different from that chosen by Saunders & King (1995) (10.5E) and why this boundary is a better choice.

As mentioned previously, our choice for the BeC boundary reflects the horizontally accumulated transport for the section. Our boundary for the BeC is different from the estimates of other studies, like 10.5°E from Saunders and King (1995), ~11°W by Hernández-Guerra et al. (2019) or 3°E by Reid (Reid, 1989). We have followed these and other studies that have situated the BeC in this region, and optimized our results based on the mass transports obtained from the inverse models.

Lines 272-274 : Could you indicate how, in your circulation scheme, the SEC at 30°S connects to the BeC at 45°S?

In our study, we consider the Benguela Current as the equatorward flow parallel to the African coast, conforming the eastern boundary current, analogous to the Canary Current, the Peru-Chile Current or the California Current. The SEC is the current flowing northwest from the southernmost point of the African continent, following the path of warm/cold waters from the Indian/Pacific Ocean into the South Atlantic (Bower et al., 2019; Rousselet et al., 2023). However, other authors have considered a wider BeC that includes part of the SEC. That is not the case in this study, where we have separated both branches.

Line 313 : How do Piecuch's results (2020) compare on a ten-year average with your results?

The average between 1982 and 2018 by Piecuch (2020) agrees with the results for 2000-09 and 2010-19 decades. Using the data provided by Piecuch (2020) for each year, we have included the estimations for the averages for each decade in the manuscript.

Lines 350-353: "… which yields an average transport of 31.8 ± 0.1 Sv (Piecuch, 2020), with decadal values of 32.5 ± 1.2 Sv for 1990-99, 32.4 ± 1.0 Sv for 2000-09 and 31.9 ± 0.6 Sv for 2010-19 for the 95% percentile. These results agree with the FC transports estimated from the inverse models although the value for the cruise carried out during 1990-99 is slightly larger."

Line 349 : it is more than twice as large and not 'slightly' higher.

We have replaced ‘slightly’ to ‘twice as large’ in the manuscript.

Lines 364-355: Above you mention that your transports have a better agreement with the fall estimates found in the literature and now it seems that it is with the spring ones. Could you clarify this point?
We have now removed the statement regarding the spring estimate from the text.

Line 375 : The North Atlantic Current is a current that extends from the surface to the bottom. It should be made clear that you are only studying the upper part of it (and justify this).
This comment is related to the previous one referring to the definitions of each of the currents. In this case, we have also followed the vertical structure of the net transport per layer, integrating the layers with northward transport. We have included this detail in the manuscript.
Lines 423-427: “The main mass transport is subsuperficial with a mass transport of 21.7 ± 2.0 and 17.3 ± 0.8 Sv in the layer between 26.45 (~200 m) and 27.23 kg m^{-3} (~400 m) in 1990-99 and 2010-19, respectively. Meinen and Watts (2000) using current meter moorings and IES attributed a much higher value of 146 ± 13 Sv for the NAC at 42°N for the whole water column, located close to our hydrographic section at 47°N carried out during 1990-99.”

Line 385 : In Meinen and Watts (2000), the North Atlantic Current extends down to the bottom, this might explain the difference with your estimate, which is limited to the upper layer.
Thank you. We have included this in the manuscript.
Lines 426: “… attributed a much higher value of 146 ± 13 Sv for the NAC at 42°N for the whole water column…”

Line 394 : Please justify why you are using limits for the NAC that are so different from those used in OSNAP (Holliday et al. 2018)?
We refer the reviewer to the comment about Line 375.

Line 402 : Houpert et al (2018) only considered the Rockall Trough branch of the NAC.
Thank you. We have included this detail in the manuscript.
Lines 442-444: “Moreover, recent glider sections from July 2014 to August 2016 along 58°N from 21°W to 15°W (Houpert et al., 2018) over the Rockall Trough branch have provided a year mean absolute geostrophic mass transport of 5.1 ± 1.0 Sv, with 6.7 ± 0.9 Sv for summer and 2.8 ± 1.7 Sv for winter, comparable to our estimations at 58°N.”

Lines 408-413 : You may be interested in the article by Petit et al (Journal of Geophysical Research, 2019) which provides an updated and slightly different view of the Irminger Current and East Reykjanes Ridge Current.
We appreciate the referral to this article. Considering their findings, we have introduced in the manuscript this new approach to the flows of the IC and ERRC.
Lines 454-455: “Petit et al. (2019) have described the along-stream evolutions of the structure and properties of the ERRC and IC. They found interconnections between both flows governed by the complex bathymetry of ridges and basins.”

Line 416 : The low values for the ERRC transport can be explained by the inability of your inverse model to reproduce the circulation in a barotropic current such as the ERRC.
Thank you for pointing this out. We will include that discussion in the Summary and conclusions section.
Lines 727-729: “The low values found for the ERRC and EGC transports as well as the overestimations of the ISOW can be explained by the inability of the inverse model to reproduce currents with a strong barotropic component.”

Lines 434-436 : See above statement that also applies to the EGC transport estimation.

Thank you very much. The statement above applies here too.

Line 471 : Garcia-Ibanez et al. are discussing contributions from source water masses. This cannot be directly compared to your definition of ISOW which includes all mixing components.
We have included this detail in the manuscript.
Lines 495-496: "García-Ibáñez et al. (2015) built an inverse model for the SPNA discussing the contributions from source water masses, estimating…"

Line 464-475 : ISOW transport is clearly overestimated. This could be an artefact due to the underestimation of southward transport in the EGC and ERRC. The model compensates by increasing the southward transport in regions where the geostrophic current shear is larger.
We have included this in the discussion of ISOW.
Lines 514-515: "Our results can be overestimated due to the low values of southward transport for both EGC and ERRC."

Line 478-479 : Holliday et al. (2018) find 3Sv northward for the deep current transport in the eastern part of the Irminger Sea. Your results contradict both previous transport estimates derived with the same data and the water mass properties that indicate LSW and ISOW at this location.
We thank the reviewer for pointing out this issue. Holliday et al. (2018) used two hydrographic sections adjusted with LADCP and SADCP data, finding 0.2 ± 1.9 Sv for 2014 and 3.0 ± 1.3 Sv for 2016, with large differences between both estimates. Our results for the deep current out of the Irminger Sea present large uncertainties, so that our estimates are not different from zero.

Line 481 : DSOW should have transport weighted temperature lower than that of ISOW. Note that the temperature range you are mentioning for DSOW is that of LSW. LSW is missing from your analyses even though it is a major component of the DWBC.
We appreciate the suggestion from the reviewer. However, we find it difficult to differentiate using 11 neutral density layers. At 47°N, we can see the contribution of two different water masses as part of the NADW, but that signal is lost equatorward. In this study we have focused on the DWBC, without separating by water masses.

Line 531 : Your upper limit is too deep. You are missing part of the LSW, which is a major component of the DWBC.
This comment is again related to a previous one referring to the definitions of each of the currents. In this case, we have also followed the vertical structure of the net transport per layer, integrating the layers with southward transport.

Line 540 : I'm confused, do you mean "East of the DWBC" ? Recirculation is only evident for 2000-2009.
We thank the reviewer for pointing out this issue. We have now removed from the text and tables the DWBC recirculation.
Lines 585-587: "West of the DWBC there is a recirculation that carries water northward at deep layers, previously estimated to be around 13 Sv using current metre moorings (Bryden et al., 2005a) and 8 Sv from CTD and LADCP profiles (Biló and Johns, 2020), despite the poorly defined zonal extent of the circulation."

Line 551 : Schott et al. (2005) used both hydrography and ADCP velocity measurements.
We have included that detail in the manuscript.
Lines 593-594: "while previous studies using hydrographic sections and ADCP velocity measurements have obtained…"

Line 622 : It might be useful to mention the blocking of northward flowing AABW by the Walvis Ridge in the eastern basin.

Thank you for your suggestion, we now have included the flow restriction caused by the Walvis Ridge in the manuscript.

Lines 664-667: “The eastern AABW yields non-significant values for the mass transport at any decades (-0.3 ± 1.0, 0.1 ± 1.0 and 0.5 ± 0.9 Sv; **Figure 9 k, l** and **Table 4**), due to the blockage of northward flowing AABW by the Walvis Ridge (**Figure 1**). As a result, the net total transport across bottom layers is dominated by the western subbasin…”

Line 659 : I thought the SEC was mainly fed by the South Atlantic Current.

The reviewer is right. After studying the literature published about the SEC, we have found that different authors have different definitions for the SEC. We have chosen to study the SEC as a complex system of currents occupying a large horizontal extension of the South Atlantic subtropical gyre flowing northwestward (Rousselet et al., 2023; Bower et al., 2019; van Sebille et al., 2012). This comment is answered above when referring to the BeC of Lines 272-274.

Line 673 : Diapycnal upwelling between layers could be added to Figure 7. I note that there is overlap in density between the upper and deep layers. This is for instance the case at 45°S with the upper layer extending from the surface to 27.84, and the deep layer extending from 27.58 to 28.10. Are the transports in the overlapping densities counted twice ? Could you please clarify this point ? My advice would be to avoid overlaps in density between layers.

Thank you for your interesting suggestion. However, diapycnal upwelling was not the original focus of the paper. It has been explored in more detail in a previous paper attending to the balance in mass transport across each section (Caínzos et al., 2022b). In this manuscript, we have profited from these results in only certain occasions, as the diapycnal budget between sections is not as straightforward. In the case of the subpolar north Atlantic, we were able to determine it due to the extension and latitudinal continuity of the NAC, but that is a rare case. Therefore, attending to individual currents, we would not be able to quantify these vertical transformations and only two or three vertical arrows could be added to the figure. Thus, we consider that it would not help the discussion in the manuscript.

Regarding overlapping, we are aware that it may seem like that, but we have tried to be careful with it. As each current is restricted to a certain longitudinal range, we have studied the net transport across each layer with the same flow direction for these longitudinal ranges (as in subplots for the new figures 5 to 9). Therefore, for a certain longitudinal range, it may be possible that the upper boundary current reaches neutral densities deeper than expected, and that are included as part of the DWBC for a different longitudinal range, where we observe that this layer behaves differently.

References for the revision

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