Reviewer 2

In this study, Piracha et al. use satellite observations of sea surface quantities over the period 2011–2020 to analyze the variability of "density flux" in the Atlantic. Density flux is a concept akin to water mass transformation and represents the change in seawater density due to processes such as air-sea buoyancy fluxes, mixing, and mixed layer entrainment. The authors produce maps of annual and seasonal mean density flux, including a decomposition into temperature and salinity contributions, as well as an attribution to annual and sub-annual periodicity. Finally, the study looks at changes in the density flux in the subpolar North Atlantic over recent years, and identifies anomalous conditions in 2019 that were not seen in previous years.

Overall, I believe that this study presents valuable results concerning density fluxes in the Atlantic over the recent past. The analysis itself is not especially novel or groundbreaking, but it seems that the methodology is robust, the calculation of the density flux using these datasets is novel in itself, and the results regarding recent changes in 2017–2020 are interesting. It is unfortunate that the analysis stops in 2020, just when a potential "regime shift" is detected (Ln. 14) — it would be ideal to extend the analysis to 2024/5. In some places, the conclusions are based on speculation and not necessarily supported by the results or by cited literature.

I believe that this manuscript could be suitable for publication in Ocean Science after the comments below have been addressed.

Having read the comments of reviewer #1 after having written my comments, I can say that I fully agree with their assessment, I differ only in my recommendation to the editor (revision instead of rejection).

We sincerely thank the reviewer for their constructive and detailed summary of our work. We appreciate the acknowledgement of the novelty in calculating density flux from the chosen datasets and the recognition that the results for 2017–2020 are particularly interesting.

We fully agree with the reviewer's observation that extending the analysis period beyond 2020 is important, especially in light of the potential "regime shift" noted in 2019.

However, a temporal extension to the study is beyond the scope of this paper. Instead there is another project dedicated to extending density flux observation over the subpolar North Atlantic to 2025 (www.arctic-flow.net).

We also acknowledge the reviewer's point regarding speculative conclusions. All such statements will be either substantiated with appropriate literature references or reworded to ensure they remain within the bounds of our results.

We thank the reviewer again for their positive assessment and agree that addressing the comments listed below will strengthen the manuscript considerably.

<u>Specific comments</u>

Ln. 1 The abstract should begin with a sentence introducing the "density flux" concept and a motivation of why we should care about it.

We agree with the reviewer's suggestion. The following sentence will be included at the beginning of the abstract: "Density flux at the ocean surface represents the rate at which surface density changes due to air-sea, ice-sea, and land-sea exchanges of heat and freshwater, and serves as a precursor to water mass transformation and ventilation processes."

Ln. 14 "Regime shift": given the importance of this result, it would be good to extend the analysis further in time. I assume that the time period 2011–2020 has been chosen due to the availability of the chosen datasets, but surely there are other sea surface salinity datasets that could be used to see if a regime shift has actually taken place over recent years.

We agree that extending the temporal coverage would help clarify whether the observed December 2020 anomalies indeed mark a sustained regime shift or a singular event. Our core analysis is constrained to 2011–2020 due to the temporal limits of the global sea surface salinity product. A temporal extension to the study is beyond the scope of this paper. Instead there is another project dedicated to extending density flux observation over the subpolar North Atlantic to 2025 (www.arctic-flow.net) which the authors are also part of.

Ln. 45 "Atlantic Thermohaline Circulation": this term may be a bit outdated, I would suggest using "AMOC" (which is already used further up in the Introduction).

We will replace "Atlantic Thermohaline Circulation" with "Atlantic Meridional Overturning Circulation (AMOC)" to maintain consistent terminology throughout the manuscript.

Ln. 57 Since the datasets have yet to be introduced, it can be confusing to read "All datasets provide…" at the beginning of the paragraph. You could include a general first sentence along the lines of "We use observational datasets of XYZ. These datasets provide…"

We will add an introductory sentence before this paragraph to contextualize the datasets. The revised text reads:

“We use observational datasets of sea surface temperature (SST), sea surface salinity (SSS), and mixed layer depth (MLD) to calculate sea surface density (SSD) using the TEOS-10 equation of state. All datasets provide global coverage over ice-free regions and are derived from satellite observations, with the exception of MLD.”

This addition introduces the variables used, clarifies that sea surface density is computed from TEOS-10, and addresses the sequencing concern.

Section 2 (Datasets) — Instead of the division into “satellite datasets” and an “in-situ/satellite blended dataset”, it might be more useful to simply divide this section into one subsection (or bullet point) per dataset, i.e., one paragraph each for the SSS, SST, SSC, and MLD datasets.

We agree that presenting the datasets individually will improve clarity and readability. In the revised manuscript, Section 2 has been restructured into separate subsections for each dataset: SSS, SST, SSC, and MLD. Each subsection will now describe the source, coverage, and processing method for each of the respective datasets.

Ln. 75 What is the spatial (and temporal) native resolution of the SSC product?

The native spatial and temporal resolution of the dataset in question is 0.125° × 0.125° and 1 Jan 1993 to 19 Nov 2024, respectively. This will be added in the revised manuscript.

Ln. 81 MLD definition: do you use a density threshold or a temperature threshold to compute MLD? The way I understand it, you compute the density change that would result from a 0.2K cooling of the water at 10m depth and use that as a density threshold. Due to the non-linear equation of state of seawater, that would imply that the density threshold itself depends on temperature and salinity, correct? For example, a quick calculation shows that at a reference salinity of $S = 35$ g kg^{-1}, the density threshold at $T = 25$ ◦C is 0.06 kg m^{-3} and at $T = 2$ ◦C it is 0.01 kg m^{-3}. Does this lead to substantially different MLDs compared to a fixed density threshold (e.g. 0.03 kg m^{-3}), and does this influence your results?

The Armor3D MLD dataset uses a temperature threshold, defined as the depth at which the density increase is equivalent to a 0.2 °C decrease from a 10 m reference depth. As noted, the corresponding density threshold depends on both temperature and salinity due to the non-linear equation of state of seawater. The dataset documentation refers to “local conditions”, meaning that they use the local temperature/salinity pair.

To answer the reviewers' latter questions, the choice of MLD definitions will give rise to differences in the surface density flux. Different definitions will define the mld *slightly differently causing slight differences in surface density flux. These changes influence the onset of convective and restractification events. Furthermore, with the thermal expansion coefficient decreasing with colder waters, it was shown that compared to a fixed density criterion, a density equivalent temperature criterion underestimates the deep convectively driven MLDs which occur in winter (https://doi.org/10.1029/2004JC002378).*

For our results, this will affect the strength/phase of the annual cycle. We also foresee that these differences will be exacerbated in terms of anomalous surface density flux events. However, an assessment and quantification of these differences is beyond the scope of the manuscript. Nonetheless, we will attempt to address this comment in the discussion section of the revised manuscript.

Ln. 85 The paragraph on the 3D T/S dataset used for the calculation of MLD is too detailed; assuming the dataset has been shown to be trustworthy, you can simply cite its reference. More important is the calculation of the MLD itself, as mentioned in the previous comment.

We have shortened this section to focus on the MLD calculation and cite the product references, removing the blending-procedure details. The revised text now reads:

The MLD dataset used in this study is obtained from the Multi-Observation Global Ocean ARMOR3D L4 product (Greiner et al., 2021), which blends satellite-derived surface observations with in situ measurements (Guinehut et al., 2004). MLD is estimated using a density-threshold method corresponding to the temperature change induced by a 0.2 °C cooling from a reference depth of 10 m, with density calculated using local temperature and salinity."

Section 3.2 (Anomalies) This subsection feels overly complicated since the calculation of deseasonalized anomalies is fairly standard. It could be reduced to a single sentence, e.g. "We calculate the monthly time series of deseasonalized anomalies by subtracting the 10-year mean seasonal cycle from the full time series". The latitude-longitude definition for the subpolar gyre can be mentioned in the text and figure caption.

The section of the anomalies referred to in the text will be modified by analysing anomalous surface density fluxes over the tropical, sub-tropical and subpolar North and South Atlantic.

The subsection has thus been simplified to a single sentence:

"We calculate the monthly time series of deseasonalized density flux anomalies by subtracting the 10-year mean seasonal cycle (2011–2020) from the full time series, for the tropical (-15–15N), subtropical (15-40N/S), and subpolar (40-70 N/S) Atlantic."

Ln. 143 “driving dense water formation”: Fig. 1 shows the whole Atlantic basin, not only the high latitude where actual dense water formation occurs. Of course, positive density fluxes occur across all latitudes as shown in Fig. 1, but this is not commonly called “dense water formation”.

We agree and have revised the text to avoid implying that all positive density fluxes correspond to dense water formation. The sentence now reads:

“The 2011–2020 mean and seasonal averages of the density flux are shown in Figures 1 and 2.”

Ln. 170–172 This sentence needs one or several references.

We have added appropriate references to support the statement on the role of equatorial currents and countercurrents in freshwater redistribution. The sentence now reads:

“Equatorial currents and countercurrents further distribute freshwater laterally, due to the interplay between precipitation, evaporation, and atmospheric heat transport via Intertropical Convergence Zone (ITCZ) dynamics (e.g., Yu, 2011; Schott et al., 2004).”

Yu, L. *(2011). A global relationship between the ocean water cycle and near‐surface salinity. Journal of Geophysical Research: Oceans,* ***116****(C10). https://doi.org/10.1029/2010JC006937*

Schott, F. A., McCreary, J. P., & Johnson, G. C. *(2004). Shallow overturning circulations of the tropical‐subtropical oceans. Earth’s Climate: The Ocean–Atmosphere Interaction, Geophysical Monograph Series,* ***147****, 261–304. https://doi.org/10.1029/147GM15*

Ln. 201 This should be made consistent.
“winter peak”: on Ln. 181, the timing is instead characterized as “boreal autumn”.

We thank the reviewer for pointing out the inconsistency. We have revised the text so that both lines use consistent terminology, specifying “boreal autumn” in both instances to accurately reflect the seasonal timing.

Ln 201 will be revised as follows:

“Unlike the thermal-driven seasonal cycle, which follows a smooth boreal autumn peak, the haline phase structure is more erratic, reflecting the irregular timing of freshwater input and redistribution.”

Ln. 214 Figure 4 should already be referenced in the first paragraph of this subsection.

We have now added a reference to Figure 4 in the first paragraph of this subsection, ensuring that the figure is introduced at the point where the discussion of the explained variance of the annual and semi-annual harmonics begins. Specifically, the line in question is changed as follows:

"The explained variance of the annual and semi-annual harmonics offers a quantitative measure of how much of the total density flux variability is captured by dominant seasonal modes (figure 4)."

Ln. 263 What does the importance of low-frequency variability imply for the results of your study, which are based on a short (10-year) observational record?

The reference to low-frequency variability was intended to explain why the combined explained variance of the annual and semi-annual cycles does not reach 100% in our analysis. We hypothesize that variability on periods longer than one year, which is not captured by these harmonics, contributes to the remaining unexplained variance. Studies such as Bamber et al. (2024; https://doi.org/10.1038/s41561-024-01592-1) indicate that Arctic freshwater content and related fluxes evolve in multi-year periods of roughly 5 years, suggesting that the freshwater-driven component of density flux in the subpolar gyre is sensitive to low-frequency variability. Thus, our short (10-year) observational record may not fully capture these low-frequency contributions, but it remains representative of the interannual and seasonal variability.

We will revise the text in the following way:

"In contrast to the annual and semi-annual cycles, which do not fully capture the total variability, processes on other timescales must contribute. In particular, the haline component of the surface density flux in the subpolar gyre is largely controlled by freshwater fluxes, which have been observed to vary on multi-year periods of roughly five years (Bamber et al., 2024; https://doi.org/10.1038/s41561-024-01592-1). This highlights the importance of low-frequency processes for understanding the temporal evolution of haline-driven density fluxes."

Ln. 284 The last paragraph reads a bit too much like an advertisement for the Arctic-Flow project. Instead, you should state more generally how future work should build on your findings in this paper.

We thank the reviewer for this suggestion. We have revised the paragraph to focus on future research directions building on the findings of this study, without emphasizing specific ongoing projects.

We will revise the text as follows:

"Future work should build on the methods and findings presented here to generate high-resolution, satellite-constrained estimates of surface density fluxes in regions critical for deep water formation, such as the Nordic Seas. By combining dynamically consistent sea surface temperature, salinity, and velocity fields, such studies can compute kinematic density fluxes and validate them against in situ observations. This will provide a robust framework to assess and monitor seasonal and interannual variations in water mass transformation and improve understanding of the processes influencing the AMOC's strength and long-term stability."

Ln. 294 "significant": significant under which statistical test? From Fig. A1 it seems that uncertainties in the density flux are generally smaller than 10% of the seasonal amplitude, is this the heuristic of significance used? Furthermore, the significance testing detailed in this appendix section should be referenced or briefly summarized in the main text.

The term "significant" refers to a practical, heuristic measure based on the signal-to-noise ratio: uncertainties in SST and SSS were propagated to surface density fluxes and compared to the magnitude of the observed variability. In most regions, the flux variability exceeds the estimated uncertainty by an order of magnitude, indicating that the signal is well resolved. We now clarify this in the main text and reference Appendix 1, which describes the error estimation in detail.

The diagnosed variability in surface density flux is considered significant relative to the uncertainties in the input satellite products. Uncertainties in SST and SSS were propagated to the density flux and normalized by the annual harmonic amplitude, providing a practical signal-to-noise assessment (Appendix 1, Fig. A1). In most regions, the variability exceeds the estimated uncertainty by approximately an order of magnitude.

Anywhere: It should be mentioned at least once in the manuscript that the increased influence of salinity on density at high latitudes is due to the non-linear equation of state of seawater (specifically the dependence of α on T). This is a key fact and should be used to interpret some of the findings.

Indeed, the dominance of salinity in setting density at high latitudes can be understood in terms of the thermodynamic coefficients of seawater. The thermal expansion coefficient (α) decreases rapidly with temperature, approaching very small values near the freezing point, while the haline contraction coefficient (β) remains relatively stable (IOC, SCOR and IAPSO, 2010; McDougall et al., 2021; Talley et al., 2011). As a result, in cold high-latitude waters, heat fluxes have a weaker effect on surface density variability than in warmer regions, whereas freshwater fluxes and salinity anomalies exert a stronger control. This thermodynamic behavior provides a natural interpretation of our findings: in the subtropics, where α is large, the seasonal cycle of density flux is primarily controlled by surface heat fluxes, whereas in subpolar regions, where α is small, freshwater fluxes dominate the density flux variability.

We will add this text to the discussion section of the revised manuscript:

"The influence of salinity on density is enhanced at high latitudes because of the non-linear equation of state of seawater. Specifically, the thermal expansion coefficient (α) decreases sharply at low temperatures, while the haline contraction coefficient (β) remains relatively constant, so that density becomes more sensitive to salinity variations than to temperature (IOC, SCOR, IAPSO, 2010; Gill, 1982; McDougall, 1987). This effect is particularly important in cold, high-latitude regions where temperature variability is small and water is close to its maximum density, leading salinity changes to dominate density fluxes (Talley et al., 2011; Bindoff & McDougall, 1994).

In the context of our results, this non-linear behavior helps explain why haline contributions emerge as the primary driver of density fluxes in the subpolar North Atlantic. The reduced role of temperature is not merely due to weaker seasonal variability in surface heat fluxes, but also reflects the diminished sensitivity of density to temperature at low absolute temperatures. Consequently, salinity-driven processes, such as freshwater input from precipitation, runoff, and sea ice melt, play a large role in shaping density variability and transformation in these regions."

IOC, SCOR, and IAPSO. *(2010). The International Thermodynamic Equation of Seawater – 2010: Calculation and use of thermodynamic properties.*

Intergovernmental Oceanographic Commission, Manuals and Guides No. 56, UNESCO.

Gill, A. E. *(1982). Atmosphere–Ocean Dynamics. Academic Press, 662 pp.*

McDougall, T. J. *(1987). Neutral surfaces. Journal of Physical Oceanography, 17(11), 1950–1964. https://doi.org/10.1175/1520-0485(1987)017<1950:NS>2.0.CO;2*

Talley, L. D., Pickard, G. L., Emery, W. J., & Swift, J. H. *(2011). Descriptive Physical Oceanography: An Introduction(6th ed.). Academic Press, 560 pp.*

Bindoff, N. L., & McDougall, T. J. *(1994). Diagnosing climate change and ocean ventilation using hydrographic data. Journal of Physical Oceanography, 24(6), 1137–1152. https://doi.org/10.1175/1520-0485(1994)024<1137:DCCAOV>2.0.CO;2*

Throughout the relevant literature:
In the following places, statements in the text should be supported by citing

- **Ln. 144–146**
- **Ln. 152**
- **Ln. 170–172**
- **Ln. 173–174**
- **Ln. 196–197**
- **Ln. 256 (what potential weakening mechanism?)**

The following references will be added to the lines in question:

Ln 144-146: Piracha et al., 2023; Groeskamp et al., 2018

Ln 152: Dai et al., 2025; Jeong et al., 2023

- *Dai, J., Xu, F., Wright, J.S. et al. Subpolar North Atlantic sea surface salinity as an AMOC mean state indicator. npj Clim Atmos Sci 8, 308 (2025). https://doi.org/10.1038/s41612-025-01190-x*
- *Jeong, H., Turner, A. K., Roberts, A. F., Veneziani, M., Price, S. F., Asay-Davis, X. S., Van Roekel, L. P., Lin, W., Caldwell, P. M., Park, H.-S., Wolfe, J. D., and Mametjanov, A.: Southern Ocean polynyas and dense water formation in a high-resolution, coupled Earth system model, The Cryosphere, 17, 2681–2700, https://doi.org/10.5194/tc-17-2681-2023, 2023.*

Ln 170-172: Cole et al., 2013

- *Coles, V. J., M. T. Brooks, J. Hopkins, M. R. Stukel, P. L. Yager, and R. R. Hood (2013), The pathways and properties of the Amazon River Plume in the tropical North Atlantic Ocean, J. Geophys. Res. Oceans, 118, 6894–6913, doi:10.1002/2013JC008981*

Ln. 173–174: The sentence in question will be rephrased as:

- *"This is seen best in the right most column of Figure 2 showing seasonal changes of haline-driven density flux."*

This removes the need for a reference

Ln. 196–197: Bingham et al., 2012

- *Bingham, F. M., Foltz, G. R., and McPhaden, M. J.: Characteristics of the seasonal cycle of surface layer salinity in the global ocean, Ocean Sci., 8, 915–929, https://doi.org/10.5194/os-8-915-2012, 2012.*

Ln. 256: the line in question has been removed as it was unnecessary here.

Technical corrections

All figures Please avoid the use of the "jet" or "rainbow" colormap (Figs. 3, 4, A1). See
this reference for a critique of this colormap:
https://doi.org/10.1109/MCG.2007.323435

The referenced paper has now been read by the authors, and the shortcomings of those colormaps understood. Based on the recommendation of alternative colormaps in the provided reference, we will adjust the figures in the revised manuscript.

All figures In general, it would be useful to include column (and row) headings in the figures,
e.g. column labels saying "net, thermal, haline" in all figures except Fig. 5, as well as row labels
saying "amplitude" and "phase" in Fig. 3, and so on.

All figures. It would be useful to make the panel labels (A/B/C/etc.) bold for better visibility

All figures. in the figure captions. The units should be used as colorbar labels directly in the plots, not only mentioned

Figure 1 Increase the size of the panel labels.

Figure 5 Do not use decimals (e.g. 2015.0) for years on the x-axis. Increase the size of all axis and tick labels.

Figure 5 caption lightning → lightening

Figure 6 Increase the size of the colorbar, maybe make it horizontal.

All the corrections will be applied to the figures in the revised manuscript

Affiliation The word "Oceanography" should likely be capitalized.

The change will be reflected in the revised manuscript.

Units In all figure captions and elsewhere in the text, units should be typeset without italics
(e.g. 1 kg instead of 1kg). In the egusphere template, this can be done using the \unit{}
command.

Noted, and the change will be reflected in the revised manuscript.

Ln. 29 within the ML

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Noted, and the change will be reflected in the revised manuscript.

Ln. 39 over the ML

Noted, and the change will be reflected in the revised manuscript.

Ln. 101 "one-two": replace by "one to two" or similar.

Noted, and the change will be reflected in the revised manuscript.

Ln. 114 (Eq. 4) remove the f(m) at the end of the line.

As the reviewer has noted that the anomaly calculation is routine and widely known, the equation in question will be removed In favor of a single sentence.

Ln. 114 (Eq. 4) "mod" should probably not be in italics (use \mathrm{} or similar)

Noted, and the change will be reflected in the revised manuscript.

Ln. 135 R2 statistics → R2 statistic

Noted, and the change will be reflected in the revised manuscript.

Ln. 139 Instead of calling σ the "standard deviation operator", you could simply replace σ2 by "var" (for variance) in Eq. 6.

Noted, and the change will be reflected in the revised manuscript.

Ln. 144 "respectively" is misplaced, instead say "...shown in Figures 1 and 2, respectively."

This grammar point will be addressed in the revised manuscript.

Ln. 150 the citations should be in parentheses

This will be rectified in the revised manuscript.

Ln. 158–159 "strait" should be capitalized since Fram Strait and Davis Strait are proper nouns

This grammar point (and others like it) will be addressed in the revised manuscript.

Ln. 172 "Figure" is repeated

The repeated "Figure" will be removed in the revised manuscript

Ln. 219–223 The reference should be to Figure 4, not 5.

Correct. This change will be reflected in the revised manuscript

Ln. 240 The unit is not typeset correctly.

The correction in typesetting will be made in the revised manuscript.

Ln. 291 There is a superfluous newline in the last sentence.

Noted, and this will be removed in the revised manuscript.

Ln. 298 The reference should not be in parentheses.

Noted, and the change will be reflected in the revised manuscript.

Ln. 299 the standard deviation is not a measure of difference between

datasets. I assume you are instead referring to root mean squared error (RMSE), or a similar metric? In addition, the values (0.35 and 0.4) should have units (presumably K or K2).

The reviewer is partially correct. Looking through that reference, there were two metrics that were used to assess the difference between OSTIA (the sst dataset in question) and ARGO. They were the mean and std of the differences over the period 2003-2019. Which explains the standard deviation, which is the standard deviation of the differences over that period. The units are Kelvin, which will be added in the revised manuscript. Also, this clarification will be included in the revised manuscript as:

“McLaren et al. 2019 calculated the standard deviation of the differences between OSTIA SST and ARGO as 0.50K and 0.45K for the North and South Atlantic, respectively“

Ln. 300 boy → buoy

The typo will be corrected in the revised manuscript