Reviewer 1

This study looks at the seasonal variability of density fluxes in the Atlantic basin between 2011 and 2020. It computes the mean, seasonal cycle amplitude and phase. It also looks at density flux in the subpolar North Atlantic.

Looking at the seasonal variability of surface fluxes of density is a valuable exercise, but I did not find this paper did that in a coherent way. This is evidenced by the proliferation of detailed comments below. I am ambivalent about whether to allow the authors to resubmit, or urging the editor to reject it. However, due to the large number of issues shown below, I would recommend rejection.

The paper is poorly focused. Figs. 1-4 are about the mean and seasonal cycle of density flux over the entire NA basin. However, much of the text focuses on the subpolar NA, and on interannual variability.

We thank the reviewer for taking the time to help improve the overall quality and scientific impact of the manuscript. Even though the reviewer recommends rejection, a constructive critique of the manuscript which will help to refine the overall focus of the manuscript is provided. Below are our responses to the comments from the reviewer. While the overall focus of the manuscript was on the Atlantic as a whole, the reviewer is correct in stating that a disproportionate amount of the paper regards the role of density fluxes in the subpolar gyre. We will answer this comment by changing the manuscript title to:

"Seasonal and Interannual Variability of Atlantic Surface Density Fluxes".

Along with this we will focus more on the patterns of surface density flux over the whole Atlantic. Especially the last section on anomalies, we will analyse anomalous surface density fluxes over the tropical, sub-tropical and subpolar North and South Atlantic. Thereby, removing a significant amount of bias towards the subpolar North Atlantic.

7. This number "70-80%" does not appear anywhere in the paper besides here. Ditto the next line (80-85%). The comparison of annual and semi-annual cycles mentioned here is brushed off as irrelevant in the text of the paper (line 206).

We will state those percentages in the relevant sections, and in the revised manuscript we will include a section on the semi-annual cycle.

16. "Surface density flux is a fundamental driver of oceanic buoyancy exchange" Density flux and buoyancy exchange are synonymous, so this is a tautology.

This sentence will be rewritten as: "Surface density flux is a fundamental driver of ocean circulation"

68. The BEC website referenced here says that the dataset is available "2011-2019". The ftp site for downloading the data was not operational when I tried to access it.

The original site has undergone several failures, now it has been moved to a new location.
Host: sftp://eodata-bec.icm.csic.es
Username: ftpuser
Password: .x8UP(ar.YZ2R)
Port: 22758

85-86. This seems a stretch, to use SSS/SST data to infer profiles down to 1500 m. I am not sure why the authors need to do this, or what the validity of such profiles would be. It depends on what these synthetic profiles are being used for - which is not evident on reading through the entire paper.

We have simplified this part of the text by adding a reference to the product and describing only the part of the computation of the MLD performed in this study:

"The MLD dataset used in this study is obtained from the Multi-Observation Global Ocean ARMOR3D L4 product. MLD is estimated using a density threshold method, which corresponds to the temperature change induced by a 0.2°C cooling from a reference depth of 10 m (Greiner et al., 2021). ARMOR3D product is freely available at (https://data.marine.copernicus.eu/product/MULTIOBS_GLO_PHY_TSUV_3D_MYNRT_015_012/description).

123. Why is the word "subpolar" here? This paper is supposed to be about the entire Atlantic basin.

The anomaly section referred to here focuses on the subpolar North Atlantic. We did this because the density flux anomalies in the subpolar North Atlantic were found to be larger than the other oceanic regions in the Atlantic. However, in the current text this was not highlighted enough.

We plan to address this in the following way:
The section of the anomalies will be done by computing density flux anomalies in the; subpolar, subtropical and tropical Atlantic (separately for the North and South Atlantic). We will show the time series of those anomalies, and highlight the region with strongest anomalies. We will then do a similar exercise to the current paper and select times of pronounced anomalies and show maps where those maximum anomalies occur.

Also we will modify the title to become:

"Seasonal and Interannual Variability of Atlantic Surface Density Fluxes"

Figure 1. The caption should state that a positive number means density gain by the ocean. Also, it appears that the ocean experiences a net gain of density- there are much more red colors on this map than blue. Shouldn't there be a balance between loss and gain? Or maybe the gain in the Atlantic basin would likely be balanced by loss elsewhere. Or if the predominance of density gain is expected, then the authors need to explain why.

We will add that description on the meaning of the colors to the caption. Now the captions reads as:

"2011-2020 averages of DJF (first row), MAM (second row), JJA (third row), SON (bottom row) net (left column), thermal (middle column) and haline (right column) components of the density flux. The units are $kgm^{-2}s^{-1}$. Positive/Negative values indicate surface density gain/loss."

Overall, the seasonal maps (Figure 2) show that density gain in winter and autumn are stronger than the buoyancy gain in summer and spring. The reason for this is the intense cooling that occurs over the subtropical gyres, ultimately leading to a very deep mixed layer and the formation of water masses. The key point is that density flux tends to be compensated at different seasons and at different hemispheres, but when you take the global average over the 2011-2020 period we see a net gain of density, mainly due to the thermal component, meaning a net transfer of heat from the oceans to the atmosphere. This is something that we have started to investigate.

We will discuss this point in detail (see below) in the discussion section of the revised manuscript:

"Surface density flux serves as a diagnostic of water mass transformation rather than a direct measure of transport. It quantifies the rate at which air-sea buoyancy exchange—via heat and freshwater fluxes—modifies the density of surface waters. When integrated over density classes and regions, it provides a robust framework for assessing transformation rates and has been instrumental in diagnosing global water mass formation and ventilation (Walin, 1982; Speer & Tziperman, 1992; Groeskamp et al., 2019).

However, transformation inferred from density flux does not imply immediate subduction. The ocean integrates forcing across diverse temporal and spatial scales, and densified surface waters may remain at the surface until dynamic processes—such as Ekman pumping or eddy-driven subduction—remove them from the mixed layer (Marshall et al., 1993; Nishikawa et al., 2010). Consequently, instantaneous density flux fields may not reflect actual mass transport across isopycnals, and their integrals often exhibit persistent imbalances.

Regional variability in air-sea fluxes further complicates the density flux budget. In the subpolar North Atlantic, intense wintertime cooling drives deep water formation and influences AMOC variability (Petit et al., 2021; Marsh et al., 2024), while tropical regions experience net warming and precipitation, reducing surface density. These contrasting regimes produce imbalances that mirror the spatial structure of climate forcing and the ocean’s role in redistributing energy and freshwater (Howe & Czaja, 2009).

Ocean dynamics modulate the diagnostic utility of density flux. Wind-driven advection, mesoscale eddies, and submesoscale processes can redistribute surface density gradients and buoyancy anomalies, altering local transformation rates without changing net input (Small et al., 2022; Zhang et al., 2025). This decoupling between forcing and transformation sites complicates the interpretation of density flux maps and underscores the importance of lateral mixing processes in shaping stratification and transformation (Biló et al., 2022). Taken together, density flux should be interpreted as a tendency field—one that signals the potential for transformation but does not alone determine the fate of surface waters.”

Walin, G.: On the relation between sea-surface heat flow and thermal circulation in the ocean, Tellus, **34**, 187–195, https://doi.org/10.1111/j.2153-3490.1982.tb01806.x, 1982.

Speer, K., and Tziperman, E.: Rates of water mass formation in the North Atlantic Ocean, Journal of Physical Oceanography, **22**, 93–104, https://doi.org/10.1175/1520-0485(1992)022<0093:ROWMFI>2.0.CO;2, 1992.

Howe, N. R., and Czaja, A.: Diagnosing the time-mean overturning circulation in the ocean, Journal of Physical Oceanography, **39**, 865–879, https://doi.org/10.1175/2008JPO4025.1, 2009.

Groeskamp, S., Zika, J. D., McDougall, T. J., Sloyan, B. M., and Bindoff, N. L.: Mixing inferred from hydrography: Impact on vertical and horizontal fluxes of energy and momentum, Journal of Geophysical Research: Oceans, **124**, 2468–2486, https://doi.org/10.1029/2018JC014250, 2019.

Marshall, J., Nurser, A. J. G., and Williams, R.: Inferring the subduction rate and period over the North Atlantic, Journal of Physical Oceanography, **23**, 1315–1329, https://doi.org/10.1175/1520-0485(1993)023<1315:ITSRAP>2.0.CO;2, 1993.

*Nishikawa, T., Kaneko, H., and Yasuda, I.: Formation and subduction of North Pacific subtropical mode water in an eddy-resolving OGCM, Journal of Physical Oceanography, **40**, 376–389, https://doi.org/10.1175/2010JPO4261.1, 2010.*

*Petit, T., Maze, G., Mercier, H., and Thierry, V.: Subtropical mode water formation and its role in the ventilation of the North Atlantic subtropical gyre, Ocean Science, **17**, 1353–1370, https://doi.org/10.5194/os-17-1353-2021, 2021.*

*Marsh, R., Zika, J. D., and Groeskamp, S.: Persistent density flux in subtropical gyres: A seasonal transformation perspective, Geophysical Research Letters, **51**, e2024GL110356, https://doi.org/10.1029/2024GL110356, 2024.*

*Small, R. J., Bryan, F. O., and Tomas, R. A.: Mesoscale eddy impacts on subtropical mode water formation and subduction, Journal of Geophysical Research: Oceans, **127**, e2022JC018972, https://doi.org/10.1029/2022JC018972, 2022.*

*Zhang, Y., Maze, G., and Mercier, H.: Submesoscale dynamics and mode water variability in the North Atlantic, Ocean Science, **21**, 1047–1065, https://doi.org/10.5194/os-21-1047-2025, 2025.*

*Biló, T. C., Souza, J. M., and de Boyer Montégut, C.: Submesoscale processes and their role in mode water formation in the South Atlantic, Frontiers in Marine Science, **9**, 832992, https://doi.org/10.3389/fmars.2022.832992, 2022.*

162. The haline contribution at high latitude may be difficult to compute due to the uncertainty of satellite SSS in cold water. The noisy haline component at 50-60S verifies this.

This is a known problem of satellite based salinity retrievals. In the new version of the manuscript we will also discuss how the heightened uncertainties of satellite salinity retrievals in cold water influences the results.

Along those lines the new discussion section will include the following text:
“A known limitation of satellite-derived sea surface salinity (SSS) retrievals is the reduced accuracy in cold, high-latitude waters, (Reul et al., 2020). These retrieval issues result in increased noise and bias in SSS observations poleward of ~50°, particularly in the Southern Ocean and subpolar North Atlantic. The haline component of the surface density flux, which is directly proportional to surface salinity gradients and freshwater fluxes, is therefore especially vulnerable to uncertainty in these regions. The elevated noise levels in the haline transformation between 50°S and 60°S observed in our analysis are consistent with this known limitation, and caution should be exercised in interpreting the amplitude and spatial distribution of salinity-driven transformation in these bands.Future refinements of this

framework could incorporate salinity uncertainty estimates to better quantify the reliability of haline contributions under cold-water conditions.

168-169. It's not clear from Fig. 1 that the Amazon and Congo Rivers contribute much to the overall NA buoyancy budget. The red and blue areas are interspersed, and strongly confined to the coast.

True. In the multi annual average the Amazon and Congo rivers contribution keep confined to the coast. The text has been modified as follows: "Tropical and equatorial regions exhibit mean negative density flux (Figure 1a), reflecting strong precipitation and riverine freshwater input (Figure 1c)."

181. The maximum heat and freshwater flux typically occurs in late boreal winter, Feb.-Mar. See Wang & Carton (2002, Fig. 5). This is especially true in the subpolar NA.

The paper in question refers to an analysis into heat fluxes across the North Pacific and Atlantic Basin. However, the authors of that paper specifically say that "the uncertainties in estimates of surface heat flux based on bulk formulas are considerable". We will address those concerns by highlighting the fact that with the approach used in our paper we may see a different picture on the timings and magnitude of surface density flux as compared to literature. Owing to the fact that literature makes use of bulk formulas in the estimation of fluxes.

Along those lines, we propose an update to the the last paragraph of the introduction in the following way:

"This study aims to improve the quantification of buoyancy changes in the upper ocean by replacing estimates obtained through the use of uncertain bulk formulas with a surface kinematic approach. Traditional methods rely on bulk formulas and parameterisations to estimate heat and freshwater fluxes—both of which introduce significant uncertainty (Wang & Carton, 2002; Brunke et al., 2011). Our method infers surface density changes directly from observed velocity and tracer fields, allowing us to capture the combined effects of air-sea exchange, mixed layer dynamics, and lateral mixing. By removing coarse parameterizations, we reveal a seasonal cycle in density flux that is more physically accurate and dynamically consistent.

However, two key limitations underlie this kinematic approach: the first concerns the accuracy of the satellite observations themselves, and the second relates to the main assumption behind the kinematic methodology. The accuracy of satellite retrievals can be constrained by sensor precision and retrieval algorithms, which *propagate into the derived flux estimates. Meanwhile, the kinematic framework assumes that the ocean mixed layer is perfectly homogeneous—an assumption that can introduce significant uncertainties in representing water mass dynamics (Iudicone et al., 2008).*

Despite these limitations, the advantage of satellites lies in their broad spatio-temporal coverage and ability to provide synoptic observations of the ocean surface. This allows for routine, high-resolution estimates of surface density flux across large regions, potentially revealing variability and dynamics that would remain hidden by sparse in-situ sampling or the error-prone parameterisations used in numerical models.

This paper specifically aims to characterize the seasonal cycle of Atlantic surface density fluxes in terms of its annual and semi-annual components, and to identify extreme density flux events over the past decade. Thus providing new insight into the timing, intensity, and variability of surface density fluxes throughout the Atlantic."

196-198. This is a vague generalized statement. Perhaps the authors can elaborate, or provide the reader with a reference that goes into more detail. Sea ice melt/freezing would certainly have a strong annual component, along with continental runoff. I don't know about precipitation, but I would guess there is a seasonal component of variability to that too.

In the results section, the statement will be replaced with:
"The weak haline seasonality results from the combined effects of temporally and spatially variable freshwater sources—such as the seasonal timing of sea ice melt, episodic precipitation events, and the irregular advection of low-salinity Arctic waters—that do not align consistently with a strong, predictable annual cycle. These processes introduce freshwater anomalies on multiple timescales, reducing the coherence of the haline signal. This complexity and its implications are examined further in the discussion section."

And the new discussion section linked will read:
"The seasonal signal of the haline component of surface density flux in the subpolar North Atlantic is notably weaker than its thermal counterpart. This subdued haline seasonality reflects the complex and often desynchronized nature of the freshwater processes operating in the region. Unlike surface heat fluxes, which are tightly coupled to the seasonal cycle of solar radiation and exhibit a clear annual periodicity, the freshwater fluxes that drive haline changes—namely precipitation, sea ice melt, river discharge, and advection of Arctic-origin freshwater—are subject to significant variability on both seasonal and interannual timescales.

For instance, the timing and spatial extent of sea ice melt, particularly along the Labrador and Greenland shelves, can vary by weeks to months from year to year, influenced by both atmospheric variability and oceanic heat transport (Timmermans *et al., 2011; Haine et al., 2015). Similarly, the advection of low-salinity waters from the Arctic through the Fram Strait and via the East Greenland Current introduces episodic freshwater anomalies into the subpolar gyre that do not necessarily align with the local seasonal cycle (Bamber et al., 2012; Holliday et al., 2020). These freshwater pulses may be modulated by wind-driven changes in gyre circulation or interannual variability in Arctic runoff and sea ice export.*

As a result, the net freshwater input lacks the strong, coherent seasonal forcing observed in surface heating, and this is directly manifested in the lower amplitude of haline-driven density flux throughout much of the subpolar region."

202-205. I guess Fig. 3f is being referenced here. It's hard to tell from the figure exactly what the month of maximum density flux in the SPNA is, or even where the authors are referring to.

Here, we were trying to interpret the strong annual cycle of salinity East of Greenland. Based on the current comment and previous comments by the reviewer, we will re-focus the paper based on a new title:
"Seasonal and Interannual Variability of Atlantic Surface Density Fluxes"
Meaning that all specific references to the subpolar North Atlantic will be removed in favor of a more general description of surface density flux patterns over each region of the Atlantic.

207. "comparatively small" Can the authors give some numbers to back this up.

For the revised manuscript we intend to include a figure showing the ratio between the annual and semi-annual harmonic, and in doing so we can give a more quantitative explanation and reasoning.

216. Remove "(80%)".

To be removed in the revised manuscript.

216-218. This is unclear. It needs re-wording.

This will be reworded as follows in the revised manuscript.

"Regions of highest explained variance—reaching up to 80%—are found in the western boundary currents and the subtropical gyre, where thermal processes dominate"

219. Figure 5a is referenced here, but I see no reference to Fig. 4. Maybe this refers to 4a?

The reviewer is correct. This is a typo and will be revised in the revised manuscript.

Figs. 4a and b. It looks like the variance explained for the total density flux is less than that explained by the thermal part in much of the SPNA (and the rest of the Atlantic basin too). I thought that the total variance explained would be the sum of the thermal and haline parts, so the total could not be less than either component. That is what the figure caption implies, and would be the most sensible way of displaying this information.

This should be better worded. The methodology, instead of being the sum of the explained variances of the respective thermal and haline components is: the percentage of each density flux component explained by the respective annual or semi-annual cycle (for fig 4b and c). Then for fig 4a, we take the density flux, which is the sum of the respective thermal and haline density flux, and compute how much variability in the density flux is explained by its respective annual or semi-annual cycle.

224-225. I do not see how this indicates what the authors say it does.

Here, we were trying to say that the seasonal cycle of net density flux is mostly driven by its respective thermal contribution over most of the Atlantic (esp. the subtropical gyres), and that this suggests heat fluxes are the dominant forcing driving density fluxes over those regions. In the new version of the manuscript this will be worded better.

225-229. None of this is evident from looking at Fig. 4. Perhaps it would be valuable to display a map of the ratio of explained haline variance to the explained thermal variance. It seems that in the SPNA, density flux has a weak seasonal cycle, but that does not imply that haline processes are important.

This conclusion was drawn due to the fact that haline contribution to density flux shows a weak sensitivity to the annual cycle whilst its respective thermal contribution shows the opposite (over the subpolar gyre). However, the net density flux-similar to its respective haline contribution-shows a weak annual cycle over this region. A figure showing this would improve the readability of the section and help in explaining this finding. We also agree with the reviewer in that the finding does not necessarily imply the importance of the haline contributions. This will be added as a discussion point and will be addressed in the discussion section of the revised manuscript.

231. This plot, while interesting, does not belong in a paper about seasonal variability (see the paper's title).

This point will be addressed by a change of the title of the paper to one that is more fitting with the themes addressed.

The title will be changed to:
<u>"Seasonal and Interannual Variability of Atlantic Surface Density Fluxes"</u>

Fig. 5. The area covered by this plot needs to be specified somewhere, with exact coordinates and a box (or region) on a map. Also, in the caption: I think the authors mean "lightening", not "lightning". Only 3 of the 4 events shown are lightning events. The first dashed line does not correspond with one.

Following the reviewers comments about the focus of the manuscript, we decided to overhaul this section on anomalies in surface density flux. The new section will show the time series of anomalies in the tropics, subtropics and subpolar Atlantic. On the time series with the strongest anomalies, we will mark the three strongest positive and negative anomalies (i.e. extreme events). And these events will be mapped to see their spatial structures.

239-244. What are these events? I guess a positive (negative) event is an indication of the SPNA salinifying/cooling (freshening/warming). They seem to occur mainly in the late fall/early winter. There must be some clues. Perhaps they are associated with advective events, or ice melt/freezing. Line 249 refers vaguely to freshwater forcing, but does not give the reader a sense that this has been carefully thought through or investigated.

In the revised manuscript, this section on extreme surface density flux events will be revamped in favor of an assessment of anomalies throughout the Atlantic basin (which is more in line with the paper's focus). However, we will make a greater effort to expand on the underlying drivers behind these anomalous events.

254. I note that this Summary section hardly mentions the seasonal cycle of surface density flux, which is ostensibly the subject of this paper (see title). I would also note that this section barely mentions any previous results. One of the purposes of a Discussion section is to do this. There may or may not be any previous estimates of surface density flux, but there are plenty of studies of the surface thermal and haline budgets in the Atlantic at the seasonal time scale. How do the results presented here compare to those? (Almost none of these studies are mentioned in the Introduction either.)

This omission stems in part from the limited number of studies that directly estimate surface density fluxes in the Atlantic at seasonal timescales. However, the reviewer is right to point out that there exists a substantial body of literature on the seasonal variability of surface thermal and haline budgets, which are directly relevant to our *findings. We will discuss in greater depth some of them in the new version of the manuscript.*

Also we will change the title of the revised paper to:
<u>“Seasonal and Interannual Variability of Atlantic Surface Density Fluxes”</u>

255. I do not think this study does this at all. It seems to show that the seasonal haline component of density flux is mainly small compared to the thermal part, i.e. Figs. 3b,c, except in very limited areas. (What do the authors mean by “modulating"?)

The sentence should instead say that the freshwater dynamics has a dominant role in modulating (i.e. controlling) density flux over the subpolar gyre.

255-257. "observed sensitivity of density flux to freshwater perturbations suggests a potential weakening mechanism" What is this referring to? Where is this demonstrated?

We were referring to the fact that in the subpolar gyre, freshwater fluxes play a dominant role in controlling the net density flux. Because of this sensitivity, even a modest freshwater perturbation—such as increased precipitation or ice melt—can drive the density flux negative, inhibiting deep water formation and potentially weakening the Atlantic Meridional Overturning Circulation (AMOC). This mechanism is implied by the observed relationship between freshwater anomalies and density flux in the study region, but the original phrasing was too vague and could be misinterpreted. We acknowledge this and agree that the sentence should be revised for clarity.

264-268. With respect, I generally find this sort of self-aggrandizement unnecessary and distasteful - and not warranted. This point can be made without using words like "unprecedented" and "significant advancement”.

In the revised manuscript, we will avoid the use of these words. And replace the passage in question with:

“Satellite-derived density flux measurements provide a novel means of investigating ocean circulation, addressing limitations inherent in traditional in-situ hydrographic observations and numerical models. These remote sensing data enable detailed characterization of surface buoyancy fluxes and their driving mechanisms, enhancing understanding of variability in large-scale ocean circulation.”

273. This repeats line 255.

The repetition will be removed in the revised manuscript.

276. This is a reference to a dataset, which is not appropriate in this context. A reference needs to be given to a paper that describes the approach to justifying and creating the dataset.

In the revised manuscript, we will include a reference on the construction methodology of the dataset in question. That reference can be found here:

https://doi.org/10.5194/essd-2025-212.

277. I do not see how it emphasizes this. Numerical models are not used or even mentioned in this paper. Even the in situ product used for estimating MLD is not model-based (lines 83-89). (I searched for the word "model" in the manuscript and found it only in the "Summary and Conclusions" section.)

Numerical models were not employed in this study, so the original statement is misplaced. Our intention was to highlight that satellite-derived density fluxes—obtained through the kinematic methodology—can serve as a valuable reference for interpreting numerical model output. However, we will remove the passage in question for the sake of clarity

287. "unprecedented accuracy" If this is truly the first product of its kind (line 280), these words are unnecessary.

The phrase will be removed in the revised manuscript.

318. The Worsfold & Martin reference is not in the reference list.

This entry appears in the bibliography as:
M. Worsfold, S. Good, A. M. E. F. J. R.-J. and Martin, M.: Global Ocean OSTIA Sea Surface Temperature Reprocessing - SST-GLO-SST-L4-REP-OBSERVATIONS-010-011, https://doi.org/https://doi.org/10.48670/moi-00168, 2020.18.

Although, the reviewer is correct in that it has been incorrectly referenced in the text making it difficult to locate. Also, the entry in the bibliography needs to properly formatted to match the style of the other entries

354-355. A URL or DOI is needed where the data can be accessed.

It will be added in the revised manuscript