We appreciate the comments and suggestions provided by the reviewer. We have tried to follow them to produce an improved version of the manuscript. A detailed reply is given below.

1) **Can the imbalances be justified in terms of the evolving velocity fields? The authors could compare the numerical and field transports for the times each individual was done (four sections, each carried out in about five days). They could also consider a mean numerical field during the entire period (25 days) and compare it with the combined realizations of the individual sections. Finally, they could use the numerical data to see if, at each time, the transports are balanced.**

   We have proceeded as suggested by the reviewer, trying to evaluate with numerical model outputs if we might have had a problem of synopticity. On the one hand, we have estimated the imbalance for numerical mass transports in a given day selected in the middle of the cruise and found that the imbalance is as low as only 0.2 Sv (Fig. 1). This result indicates that the numerical model is conditioned to balance mass transports in every time step.

   On the other hand, we have produced a synthetic cruise, where we extract the model profiles in the same dates and locations of the cruise, hence introducing a time dependency in the dataset. In this second case, the imbalance is ten times larger, 2 Sv. This result indicates that the time dependency has added some noise in the velocity fields, losing some synopticity (Fig. 2). This imbalance is rather similar to that obtained from the inverse model, suggesting that indeed we might have had a problem of synopticity.

   The next version of the manuscript will contain a section to discuss with more detail this issue with the synopticity.

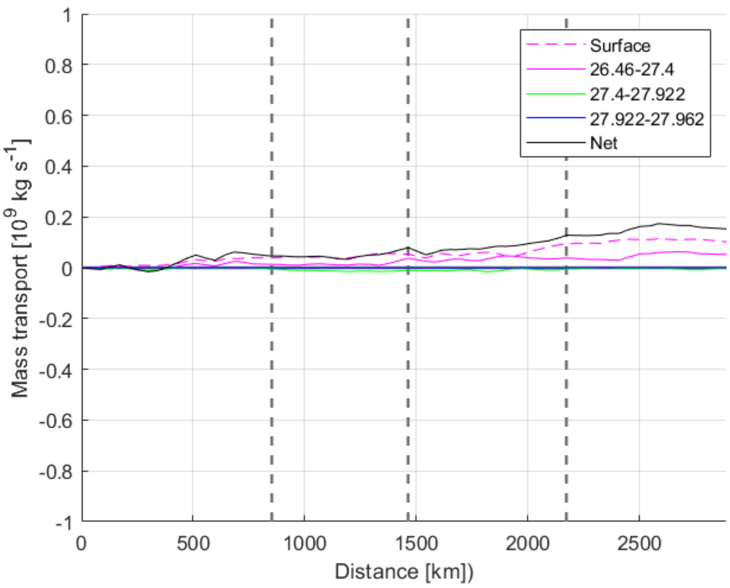

Figure 1: Accumulated transports for SW, CW, IW and DW estimated with GLORYS on July 26.

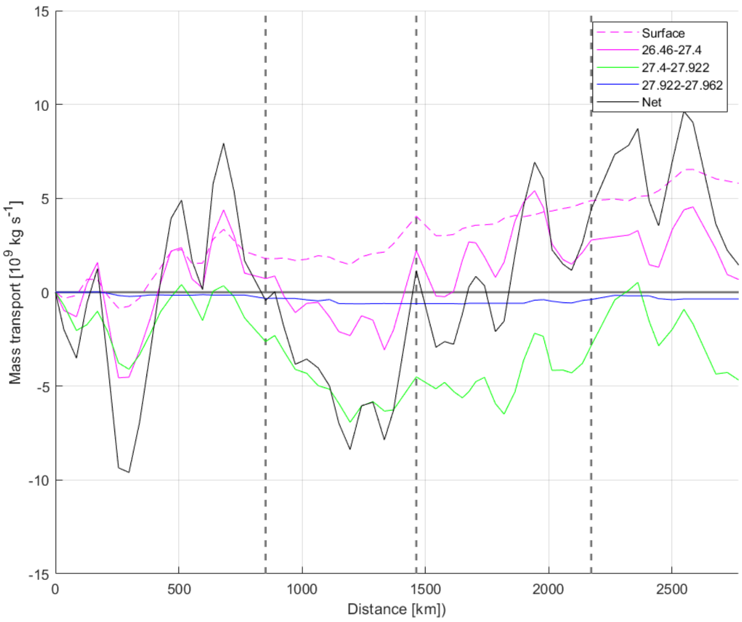

Figure2: Accumulated transports for SW, CW, IW and DW estimated with GLORYS in the specific days in which they were performed.

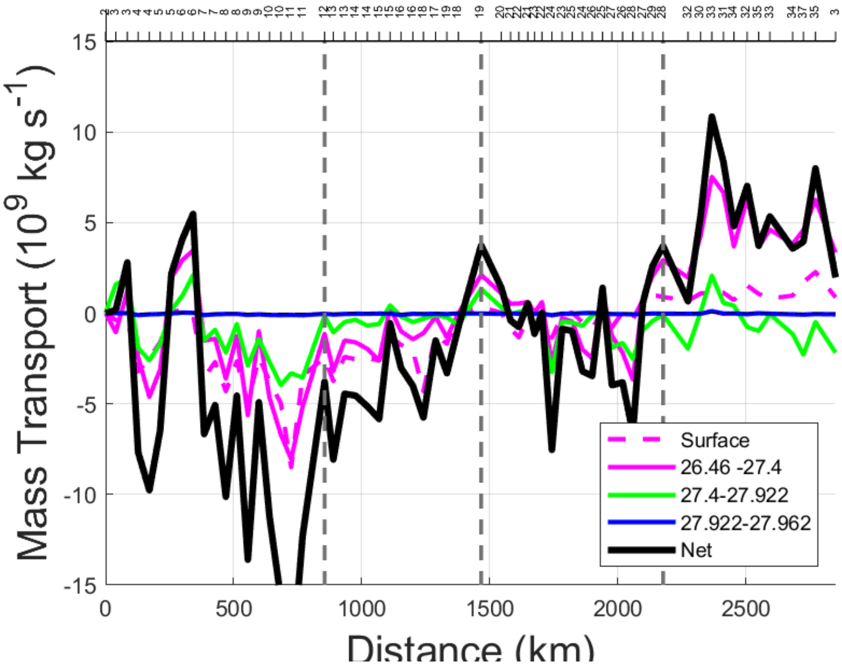

Figure 3: Accumulated transports for SW, CW, IW and DW estimated with in situ data.

A different alternative would be to use the numerical data together with water-tracking software, such as available in https://oceanparcels.org/. That would allow the authors to differentiate between Eulerian and Lagrangian streamlines, as constructed with the numerical data.

We consider that with the first approach we have addressed the main concern raised by the reviewer.

2) The field data should be viewed as an opportunity to validate and identify the limitations of the numerical data. Possibly, numerical data does well near the surface but this may not be so at the subsurface where much less data is assimilated.

The accumulated transport produced with GLORYS does not resemble any of the transports estimated with in situ data, neither at the near surface nor below (please, see figures in the previous answer). However, GLORYS recreates quite accurately the position of the front with the isohaline S_p=36 (S_A=36.15 g/kg) at 150-155 m which is detected between stations 22-23 and 32-33 in transects S and E (Figure 4).

The next version of the manuscript will present these limitations of the numerical model in this domain.

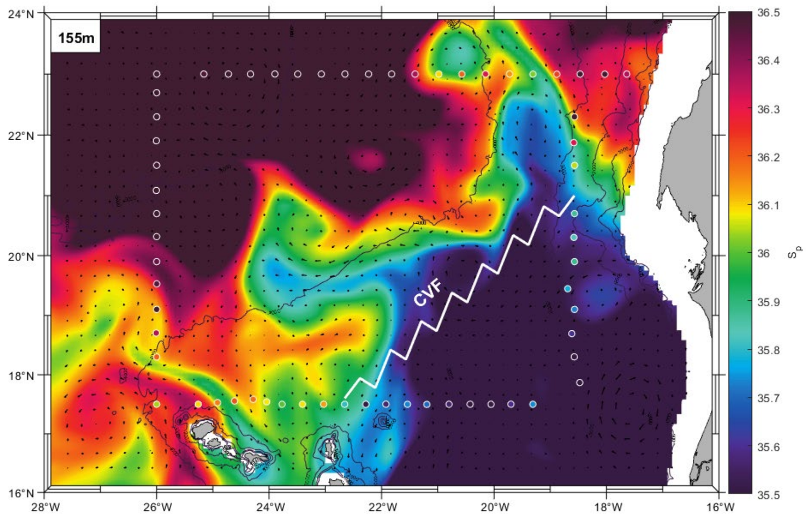

Figure 4: Salinity and geostrophy fields of GLORYS with in situ values superimposed at 155 m. The position of the CVF is also indicated.

3) I would also suggest separating the region in two different domains, split by the frontal system. This would require first to set some criterion to define the position of the front, a criterion that may change with depth. Then the authors could check if properties are balanced for the tropical and subtropical domains.

We really appreciate this comment. The front has historically been defined at only one depth, 150 m, where the isohaline must be 36 to identify the front. Here we have followed the suggestion provided by the reviewer to produce a method to estimate the front distribution with depth. Hence, we have taken two climatological profiles, one fully within NACW and the second one within SACW (Fig. 5a). Those climatological profiles provide a relationship between salinity, temperature, and depth. Based on those profiles, we have checked that the average salinity between those two profiles at 150 m is 36; in other words, at 150 m the front is located in a salinity where NACW and SACW contribute with 50%. We have followed the same reasoning at standard depths from 100 to 600 m depth, obtaining the salinity that would define the front location for every depth (Figure 5b). Finally, we have performed a linear, quadratic, and cubic fit between depth and salinity, so for a given depth we can estimate the salinity that would define the front location.

Based on the quadratic fit, we have been able to depict the front location with depth, a result that have never been produced before (Fig. 6). This distribution of the front is useful to estimate the property balances in the tropical and subtropical sides of the front. Therefore, the front is considered in the three layers of CW separating the subtropical and tropical zone in which it will try to analyze the balance of properties, in new figure and table.

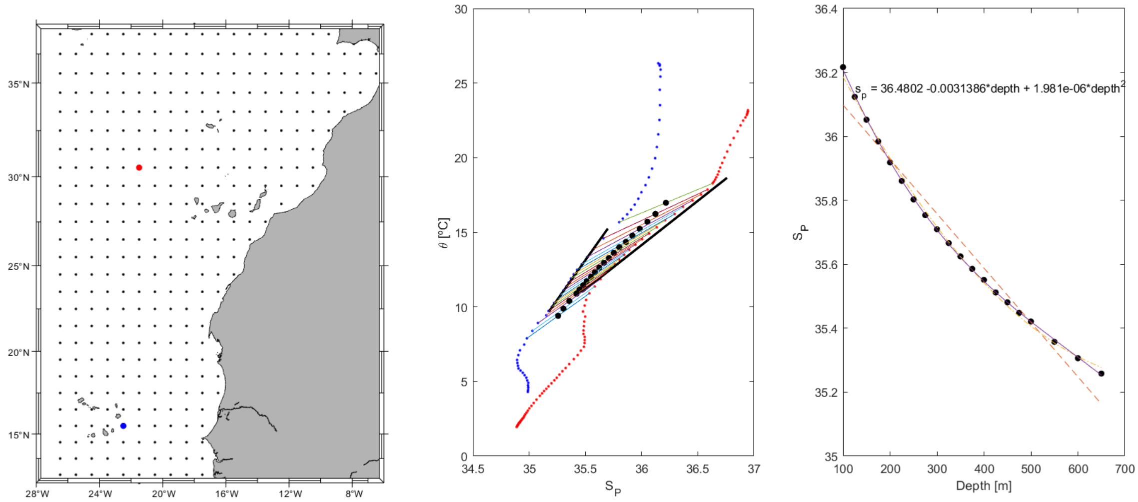

Figure 5: a) Map with the two selected WOA stations in NACW (red) and SACW (blue) domains. b) TS diagram with the average salinity for each depth (in the range 100-650m) in black dots between the profiles of northern and southern WOA stations. c) Linear, quadratic and cubic fits for depth versus salinity.

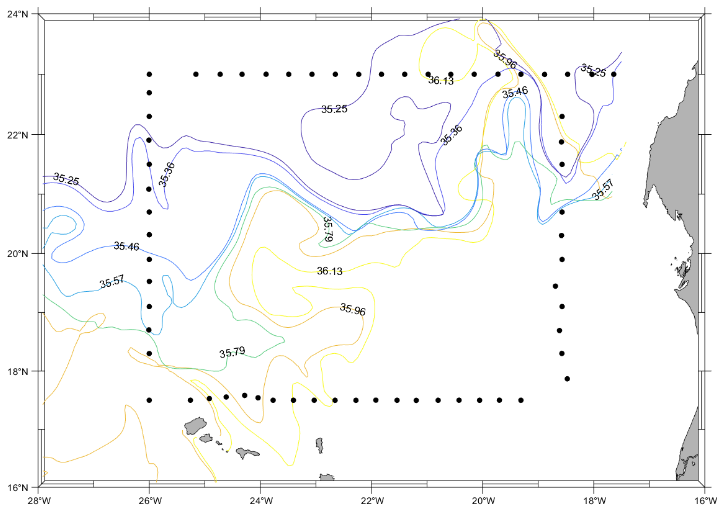

Figure 6: Location of the front at the isohalines 36.07, 35.88, 35.67, 35.43, 35.31, 35.2 and 35.08, corresponding to average depths of 119, 190, 260, 365, 469, 584 and 698 m equivalent to 26.46, 26.63, 26.85, 26.98, 27.162, 27.28 and 27.40 kg/m^3.

4) **Have the authors explored whether the imbalances in oxygen and inorgnanic nutrients are consistent? For example, if a deficit of oxygen is accompanied by a surplus of inorganic nutrients then the likely implication is remineralization. In my opinion, splitting the box in two regions would facilitate a joint interpretation of the imbalances in inorganic nutrients and oxygen.**

   This domain is particularly sensible to the suspended particle content, being a significant element in the carbon cycle (Bory et al., 2001). That is why it is not clear whether the discussion about remineralization, production and respiration could be made like in any other area of the open ocean. In addition, it would be convenient to have collected the particulate and dissolved parts of both organic and inorganic nutrients (at least C or N), which are not available in our case. The next version of the manuscript will include a discussion about the relationship between remineralization, respiration, and production processes, in terms of the contents of nutrients and oxygen.

   *Bory, A., Jeandel, C., Leblond, N., Vangriesheim, A., Khripounoff, A., Beaufort, L., ... & Buat-Ménard, P. (2001). Downward particle fluxes within different productivity regimes off the Mauritanian upwelling zone (EUMELI program). Deep Sea Research Part I: Oceanographic Research Papers, 48(10), 2251-2282.

5) **Finally, I would recommend the authors to have the manuscript revised by a native English speaker. In particular, the manuscript is sometimes redundant and verbous. I also suggest to include fewer values throughout the text (tables already serve this purpose) and simply mention the most distinctive and peculiar features.**

   We will make the text lighter including only relevant results such as maximum and minimum values.

The repply to comments:

1. **I understand the convention of negative/positive values for water mass and water properties entering/leaving the domain but I would avoid saying that e.g. "-3.2 Sv entered through the north", simply say that "3.2 Sv entered through the north"**

   We agree with the reviewer and they have been changed.

2. **page 4, line 1: I believe data is not gathered every 1 dbar, this is a result of the program that interpolates the gathered data**

   The text is changed as: "…more than 2000 m depth and processed with a vertical resolution of 1 dbar".

3. **page 4: have the authors explored if there is Argo data available for the region at the time of their cruise?**

We have now checked for Argo data in our domain and have found 7 profilers that provided 20 profiles (Figure 7). They have not been considered for the analyses in the current version of the manuscript.

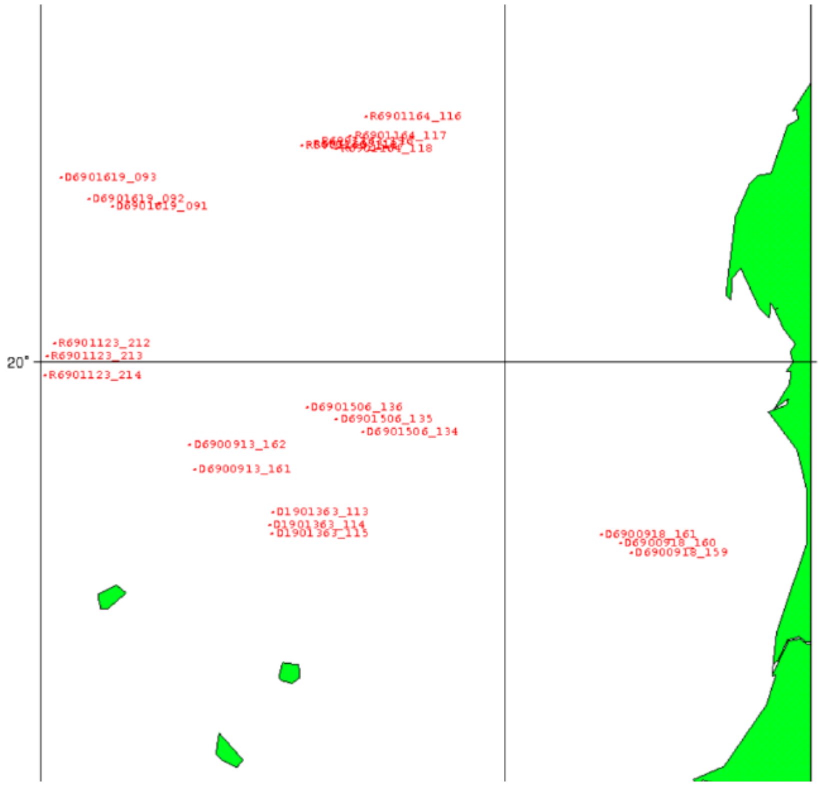

Figure 7: Argo profilers in the domain during FLUXES-I cruise.

4. **page 5: could you use the temperature XBT data, aided by T-S relations, to obtain an improved resolution of the salinity fields? Would it be possible to do something similar with the inorganic nutrients and dissolved oxygen fields?**

   DIVA was the interpolation method used along this manuscript because it provides estimates about the errors made in all the interpolations. DIVA has the advantage that it estimates an error field for each interpolated variable considering the distance between the interpolated and observation positions and the variability of the interpolated field. In order to check DIVA's performance, a station was randomly removed in each transect and the interpolated profile was compared with the real one. This was done for temperature and salinity with relative errors* below 10% in all the water column. For example, the maximum differences observed mainly in the shallowest depths between the original and interpolated profile in temperature were less than 0.5ºC in all the transects (Figure 8). Therefore, we considered this interpolation method valid for the rest of variables.

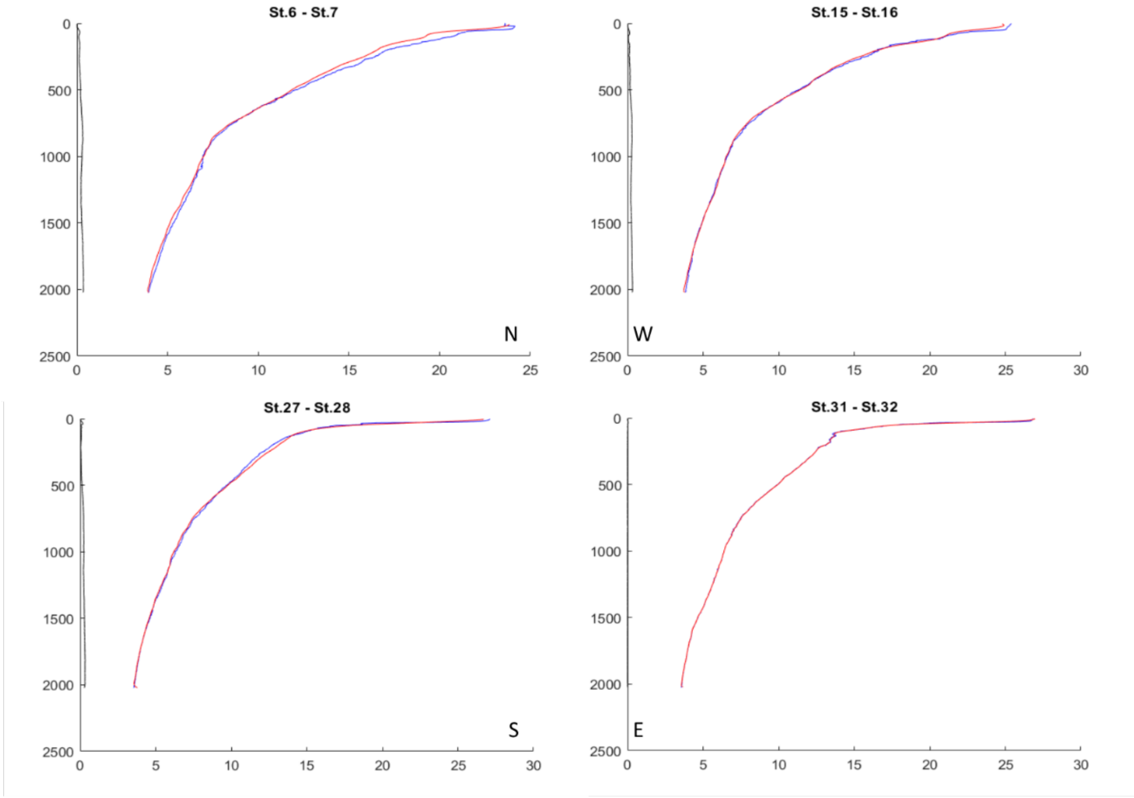

Figure 8: Real (blue) and interpolated (red) profiles of temperature in 4 random stations at the northern, western, southern, and eastern transects with the relative error profiles indicated in black.

*Relative_error = abs(interpolated value - real value)/ real value *100;

5. **page 5: do not use lower-case t for the temperature, rather use capital T**.

   It is changed throughout this paragraph on page 5.

6. **- page 10, line 23: I can see a MW signal only in one station; possibly a meddy in the northern section?**

   We have changed the text like this: "At IW levels a second latitudinal transition is observed between AAIW and MW from south to north (Zenk et al., 1991). In these layers, AAIW is clearly the dominant water mass with an exception at the northern area where a very diluted MW reduces the presence of AAIW."

7. **- Figure A1: indicate the bathymetric contours.**

   It has been done (Figure 9).

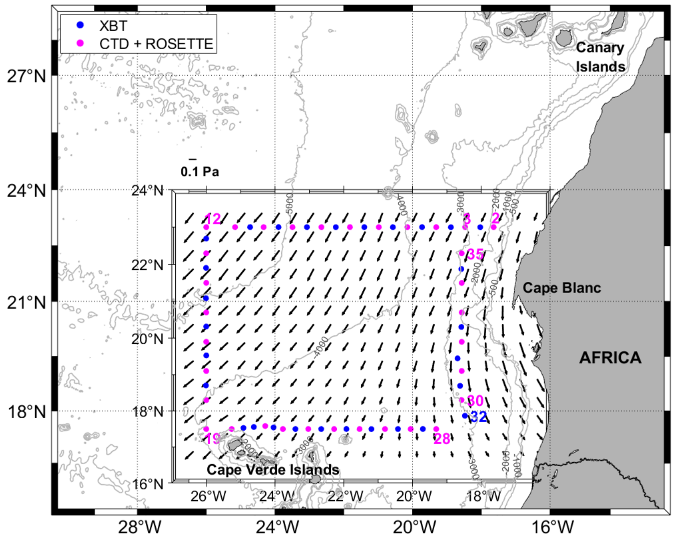

Figure 9: Fluxes-I cruise stations with the bathymetric contour values.

8. **Figures A9 and A10: include a curve with the net values.**

   It has been done.

9. **Figures A11 and A12: the figure caption is unclear: you have scattered plots of two different variables among NO3, PO4 and dissolved oxygen. The color bar is simultaneous for depth and neutral density, which cannot be right; it may be an approximate color bar but you need to indicate so.**

   We agree with the reviewer. Actually, the color bar presents the neutral density and the equivalent average depth. It has been changed in the figures.

10. **- Figure A14 and tables A1, A2: I suggest you show the transports as separated by the frontal system.**

    We will incorporate a new figure and table for CW layers where the front is detected as recommended by the reviewer. We will also add a section to the paper to describe the procedure followed to estimate the front location with depth.