The comments and suggestions provided by the reviewer are very much appreciated. We have tried to follow them to produce an improved version of the manuscript. A detailed reply (in bold) to each specific query (in light) is given below.

1. The joint use of an in situ data set collected in a few weeks and WOA climatology. The authors argue that the northern section and the eastern section have a strong asynopticity to build a box with hydrographic stations on the continental slope from WOA. Isn't this also a great asynopticity that is difficult to defend? WOA strongly smoothes the structures and it is difficult to believe that the boundary currents are restituted in the same way in WOA as they would have been with hydrographic data made at the same time as the section.

The objective of the procedure followed is to produce a dataset that allows us to consider mass conservation within a closed volume minimizing the impact of asynopticity. As pointed out by the reviewer, WOA provides smooth data which finally helps to produce an average mass transport close to the continental slope. Such a smooth and average nature of this dataset makes them particularly suitable in this context to avoid adding any imbalances to the dataset.

2. Searching for an annual-mean like solution when there is strong seasonal variability in the region.

We agree with the reviewer and we have modified the manuscript accordingly. The numerical model GLORYS is now employed to develop a climatological summer velocity field from 25 years of data. This climatological summer velocity field is used as the a priori reference level velocity in the four transects. In addition, the seasonal variability of velocity in the eight layers is estimated from the summer months covered by those 25 years of GLORYS.

3. I don't understand why the salinity conservation equation has a freshwater forcing term. Salt is conserved without a forcing source.

These equations are added to the inverse model as anomaly equations. The fresh water flux does not affect the salt transport but it does affect the salt anomaly transport. In the definition of salt anomaly transport, T'_S, given by:

$$T'_S = T_S - \overline{S_c} \times T_M$$

T_S is the salt transport, $\overline{S_c}$ is the mean salinity in a layer and T_M is the mass transport which includes the whole mass even the freshwater. So once the subtraction is performed, the freshwater term appears in the salt anomaly equation (Ganachaud, 2003).

***Ganachaud, A. (2003). Error budget of inverse box models: The North Atlantic. *Journal of Atmospheric and Oceanic Technology*, *20*(11), 1641-1655.**

4. It is not correct to use the error on the velocities to calculate the error on the dynamic equations. The velocity error is taken into account in the velocity term of the cost function. It should not be taken into account twice.

We disagree with the reviewer. These procedure has been applied previously with satisfactory results (Machín et al., 2006; Burgoa et al., 2020). In particular, Ganachaud (2003)* made an analysis of these errors concluding that mass transports and their imbalances are largely related to the error of the velocity, since the sea water density and vertical areas to estimate lateral transports are known more accurately. Hence, the main source of uncertainty in the equations comes from the error in the velocity.

*Ganachaud, A. (2003). Error budget of inverse box models: The North Atlantic. *Journal of Atmospheric and Oceanic Technology*, *20*(11), 1641-1655.

5. In general, there should be a table that summarizes the different parameters of the inverse model and presents the different errors used, and the a posteriori errors as well.

We agree with the reviewer and have modified the manuscript accordingly. On the one hand, the values for a priori uncertainties are included in a new table on "*Characteristics and constrains*" of the inverse model. On the other hand, in a new sub-section about the solution of the inverse model, a new figure (right on Fig. 4) shows that the full-depth integrated error is around 1 Sv for the mass transports extracted from the inverse model and the text has been modified to state that the errors in the reference velocities are below 0.025 m s^{-1}.

6. What is missing is an posteriori analysis of the solution to verify that the a priori hypotheses are satisfied. GLORYS could also be compared to the reference level velocities estimated from the inversion. If there is good agreement, this validates both the inversion and the use of GLORYS velocities on section E. If there is not good agreement it will invalidate the method.

We have followed the comment provided by the reviewer and instead of comparing the velocities at the reference level where the differences between the velocities estimated with the inversion (of the order of 10^{-3}) and those by the numerical model GLORYS are negligible, we have performed the comparison at the sea surface, where velocities are higher and also their potential impact on the integrated transports in the water column. In addition, we have also used the geostrophy derived from altimetry as a third independent element for validation. The comparison is performed in terms of the accumulated transports in the first layer. As shown in Figure 1 in this document, the accumulated transports estimated with the inversion including GLORYS' velocities as the reference velocities have the same behaviour as the accumulated transports using both the geostrophy derived from altimetry and from GLORYS, being the final difference among the three methods in the order of 1 Sv. That result supports using this methodology in the present work.

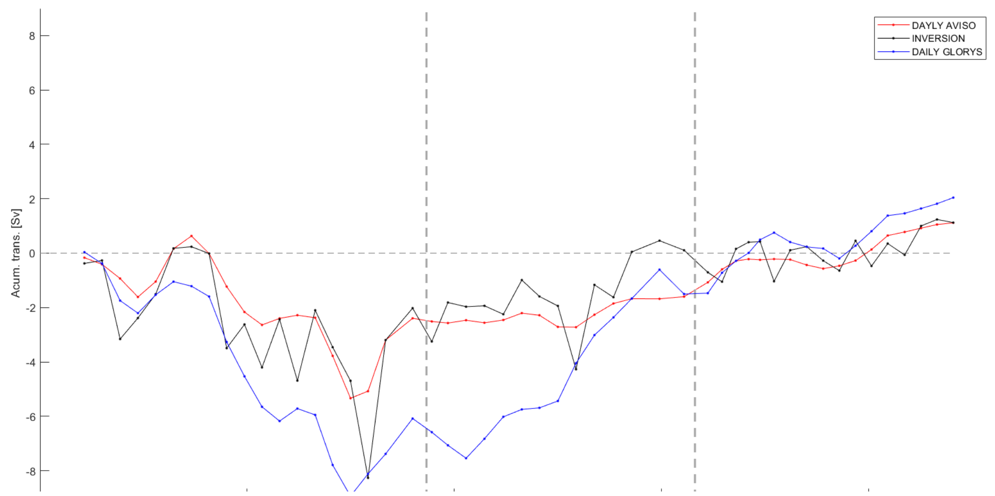

Figure 1: Accumulated transports in the first layer estimated along transects N, W and S (without WOA stations) with altimetry's derived geostrophy (red line), inversion (with GLORYS' as reference velocities, black line) and GLORYS' field (blue line).

7. The data do not resolve the sub-mesoscale which, as indicated in the introduction, plays an important role in property transfers in this region.

We agree with the reviewer and we discuss it now in the final section. With this methodology it is not possible to distinguish the percentage of the transport which is due to the meso and sub-meso-scale (probably part of the error is due to these processes). Samplings with lower resolutions should be carried out to study in detail the role of the meso- and submesoscale in the transfer of properties as the one developed by Hosegood et al. (2017) in the study area.

***Hosegood, P. J., Nightingale, P. D., Rees, A. P., Widdicombe, C. E., Woodward, E. M. S., Clark, D. R., & Torres, R. J. (2017). Nutrient pumping by submesoscale circulations in the mauritanian upwelling system. Progress in Oceanography, 159, 223-236.**

In several places in the text you repeat information already given in the figure captions (see for example page 7, lines 22 to 27). This makes the text unnecessarily heavy.

We have reviewed these redundancies to make the manuscript lighter.

Add the locations of the CVFZ in figures A2, A4, A5, A7.

We include a new figure (Fig. 2 in this document) with the vertical location of the front on a map and also two vertical sections produced by an OMP analysis. We have not added it to the rest of figures to not overload the images.

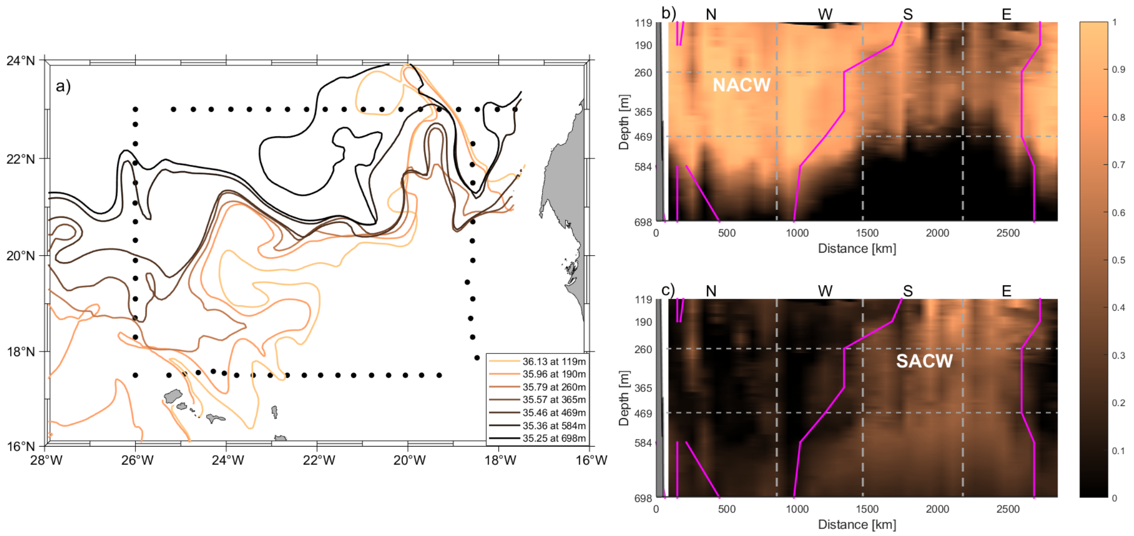

Figure 2: a) Location of the front at the isohalines 36.07, 35.88, 35.67, 35.43, 35.31, 35.2 and 35.08, corresponding to average depths of 119, 190, 260, 365, 469, 584 and 698 m equivalent to 26.46, 26.63, 26.85, 26.98, 27.162, 27.28 and 27.40 kgm^{-3}. Vertical sections of the three layers of CW with the percentages of NACW (b) and SACW (c) and the front location indicated by pink lines. The 4 transects are separated by three vertical gray dashed lines located at stations number 12, 19 and 28. Three layers are also separated by two horizontal gray dashed lines.

Figure A8 and associated discussion. I can see an anticyclonic eddy between station 3 and 6 on figure A8 (3 y 5) but I can't see a cyclonic eddy between station 4 and 6 on the same figure (3 y 5). Please clarify. Add a few station numbers on Figure A8, it will help to follow the discussion.

Some stations numbers have been included in Figure A8 (it is updated below in Fig. 3 of this document). In the northern transect, the first small anticyclonic eddy is centred at station 4. Next to it, there is a larger cyclonic eddy between stations 4 and 7 with its centre between

stations 5 and 6. This description is made simultaneously observing this figure and the vertical section of velocities.

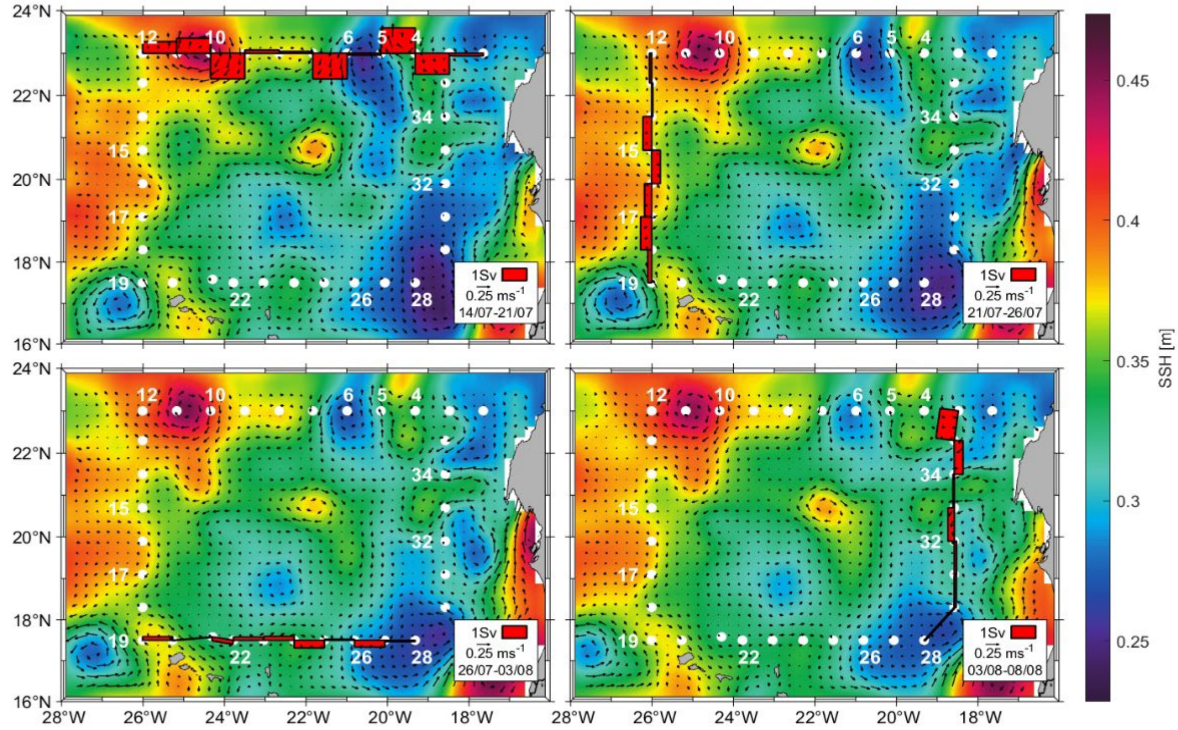

Figure 3: Averages of SSH and derived geostrophic velocity from SEALEVEL_GLO_PHY_L4_REP_OBSERVATIONS_008_047 for the sampling time period of each transect: from July 14 to 21 (upper left), from July 21 to 26 (upper right), from July 26 to August 3 (lower left) and from August 3 to 8 (lower right) of 2017. The red bars represent the mass transports in the shallowest layer. White dots are CTD stations of FLUXES-I cruise.

The term filament (page 9, line 12) is misleading as filament dynamics in the presence of a mixed layer is not adequately described by the classical thermal wind balance and thus cannot be resolved by altimetry.

We agree with the reviewer and have changed it for the term "intrusion".

Figures A9 and A10 are difficult to interpret, especially when the results of the different sections have to be connected to each other. I suggest to use Figure A14 and similar figures for biogeochemical transports to better convey the message in the discussion of these transports.

We consider that it is convenient to include these two figures (A9 and A10) because they provide more evidence in relation to mass transport variability in each layer.

Figures A9, A10 and A14: it is not clear if section N and S include the transports from the slope and shelf regions where WOA hydrography was used.

No, they are not included. Images' and tables' captions have already been modified specifying this point suggested by the reviewer.

Figure A14 shows very well that mass balance is far from being satisfied. This become a very serious problem when interpreting the biogeochemical tracer transport imbalances in terms of accumulation/consumption of the tracer while the primary reason for this imbalance is that the mass is not conserved. It is also a problem for the mass balance leading to sentences like the one in page 12 line 7: a significant input of -1 +/- 1.3 Sv. I would not say it is significant**.** The direction is not robust.

This last sentence has been removed from the manuscript because the calculations and analysis have been redone. On the other hand, a new analysis is made considering the front in Central Water (CW) layers. The imbalances in the transports of biogeochemical variables in subtropical and tropical zones of CW are now analysed taking into account the imbalances in mass transports and the correlations between salinity and nutrients and/or salinity and O_2. We hope this new approach is more satisfying for the reader.

Concerning the mass balance, I find it hard to believe that the inclusion of section E in the inversion would have created a problem of synopticity such that the mass would have been less well conserved than with the current solution.

The first versions of the model included the section E. Next figures exhibit the results when section E is included in the inversion (left on Fig. 4) and the results obtained without including it and including the transports considering WOA stations (right on Fig. 4).

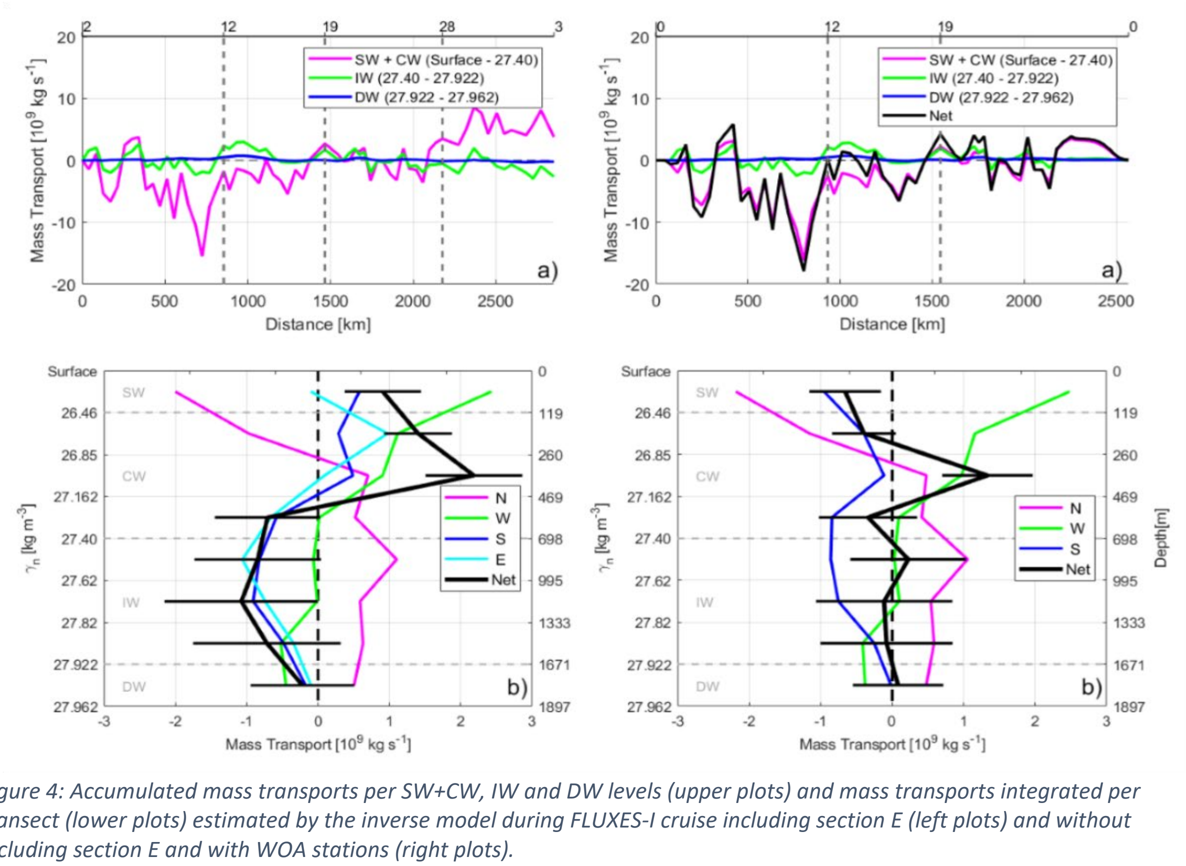

Figure 4: Accumulated mass transports per SW+CW, IW and DW levels (upper plots) and mass transports integrated per transect (lower plots) estimated by the inverse model during FLUXES-I cruise including section E (left plots) and without including section E and with WOA stations (right plots).

If we compare both results, a lower imbalance is obtained when WOA stations near the coast are considered and section E is removed from the estimate of mass transports.

Figures A11 and A12. I'm not quite sure what these figures and the discussion that goes with them add to the discussion of figures A2 to A7 (most of the discussion is spent arguing that this data set agrees with previous ones). At a minimum, the figures and the two discussions should be grouped together.

The physical-chemical properties that characterize the different water masses are related in these two figures (A11 and A12). Following the reviewer's suggestion, these figures have been included in the "*Hydrography and water masses"* section of results where they are described together with the rest of the physicochemical variables of the water masses.