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Ocean Science Discussions

Interactive comment on "Seasonal variability in mass, nutrients and DOC lateral transports off Northwest African Upwelling System" *by* Nadia Burgoa et al.

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We appreciate very much the comments made by the reviewer, that have helped us to produce a clearer version of our manuscript. We have followed them and have introduced several modifications in the paper according to his/her comments. In the next lines we give a detailed repply about how we have handled every comment.

Hereafter, the author's repplies are presented in capital letters.

Main comments:

1. One of the main problems I see is that the data used in this study are not sufficient

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to address the term of seasonal variability. The authors essentially used two snapshots of hydrographic sections in two different seasons to quantify the differences in transports instead of variability. In my understanding, transport variability can only be discussed when there are continuous timeseries of observations (or model results) or very frequently resampled hydrography, which is not the case for this study. Therefore, I would suggest that the authors may consider (1) to focus on seasonal difference instead of variability; (2) to use more repeats of the hydrographic sections to increase the samples size (if applicable); and (3) to include and compare with timeseries or/and seasonal cycle of mass, nutrient, and DOC transports from the assimilation models (i.e., GLORYS/GLORYS-BIO) to put the inverse results in a more synthesized context. Please also see the detailed comments below.

WE AGREE WITH THE REFEREE'S COMMENT ABOUT THE SEASONAL NATURE OF THIS MANUSCRIPT. THE TITLE WILL BE CHANGED TO "'MASS, NUTRIENTS AND DOC LATERAL TRANSPORTS OFF NORTHWEST AFRICA DURING FALL 2002 AND SPRING 2003"'. THESE CRUISES WERE PERFORMED SOME 16 YEARS AGO, AND THOSE WERE THE ONLY REALIZATIONS AVAILABLE TO BE ANALYSED AS PART OF COCA PROJECT.

THE MAIN STRENGTH IN THIS ANALYSIS IS RELATED TO ITS OBSERVATIONAL NATURE. WE HAVE CHECKED OUT THE HISTORICAL DATABASE AND IT IS REALLY SCARCE IN THIS DOMAIN, WITH LESS THAT 100 STATIONS DURING EACH SEASON AFTER 2001. HENCE, WE CONSIDER THAT RESULTS BASED ON OBSERVATIONS MADE IN THIS PARTICULAR DOMAIN ARE MORE ROBUST THAN RESULTS OBTAINED FROM ASSIMILATION MODELS AS THE HISTORICAL DATABASE THEY ARE BASED ON MIGHT BE UNDERSAMPLED IN THIS DOMAIN.

2. The authors have performed and listed a large number of analyses including the inverse calculation, and the property transport calculation. However, the "bigger picture" is not very clear and should be improved. The authors mentioned the importance of the EBUS region in association with the southward eastern boundary current of the

subtropical/tropical gyre and the CVFZ, which has been studied by many scholars. The authors, however, may emphasize how their study differs from the previous ones, what the new findings are, and why they matter.

THIS STUDY DIFFERS FROM OTHERS WHICH ANALYZE THE SAME AREA BECAUSE SO FAR THE CIRCULATION OF THE EBUS ZONE HAS BEEN STUDIED MAINLY SINCE THE UPWELLING PROCESS ITSELF, BUT STUDIES WHICH RELATE THE MESOSCALAR ACTIVITY AND THE POSITION AND ORIENTATION OF THE CVFZ WITH THE CIRCULATION PATTERNS, OR WITH THE LATERAL ADVECTIVE TRANSPORTS OF BIOGEOCHEMICAL VARIABLES, ARE NOT VERY ABUNDANT IN THE ZONE. WE WILL MODIFY THE CONCLUSIONS IN THE MANUSCRIPT TO HIGHLIGHT THE MAIN OUTCOMES.

3. The discussion appears not well connected with the conclusion. After reading the discussion, I miss how consistent the results in this work are with previous studies. For instance, in lines 414-416, it writes "... This region is featured by a late summer northward progression of AAIW in fall, and by a weak southward flow of MW in spring...". Whereas, not until lines 464-466 readers would hardly realize the fact that in intermediate layers transport is northward in fall and southward in spring. However, after reading the entire discussion and finally arriving at lines 464-466, readers might have forgotten what was written in the discussion. Therefore, I suggest that the authors merge the discussion and conclusion in one closely related section.

WE CONSIDERED THAT TWO SECTIONS MIGHT BE CLEARER IN PRESENTING THE RESULTS. WE TAKE THE COMMENT MADE BY THE REVIEWER AND WILL MODIFY THE DISCUSSION AND CONCLUSIONS SECTIONS TO MAKE THEM MORE UNDERSTANDABLE.

Comments in detail:

1. At the end of the introduction, the authors should point out how this work is different from the previous studies, and why this work is important.

WE AGREE WITH THE REVIEWER AND HAVE MODIFIED THE TEXT AS: "'The ocean dynamics in the region between 20° and 28° N off Northwest Africa during two different seasons is addressed in this manuscript. This domain south of the Canary Islands has historically received less attention as compared to other domains off Northwest Africa, and a proof of that are the few observations available in the historical databases. An inverse box model is applied to hydrographic observations to estimate mass transports. This method provides a velocity field consistent with both mass and properties conservation within a closed volume and with the thermal wind equation (Wunsch, 1996). Several authors have already described the circulation patterns of the NASG by applying an inverse model (Ganachaud and Wunsch, 2002a; Ganachaud, 2003b, a; Hernández-Guerra et al., 2005; Machín et al., 2006; Pérez-Hernández et al., 2013; Hernández-Guerra et al., 2017).

To sum up, the main goal of this manuscript is to present a high quality hydrographic database and to estimate mass, nutrient and organic matter transports during fall and spring seasons south of the Canary Islands in the context of a highly variable environment as the CVFZ. The remaining of this manuscript is 80 organized as follows: the dataset is presented in section 2; the seasonal distribution of the water masses and their properties is displayed in section 3; the technical details of the inverse box model are covered in section 4; the resulting velocity field and the corresponding mass, nutrient and organic matter transports are presented in section 5. Section 6 is devoted to the discussion to end up with some conclusions at section 7."'

2. Only two repeats of the hydrographic section in fall 2002 and spring 2003 are used in this study. If applicable and convenient, the authors may consider to include more repeats in other years or seasons. This could potentially make this study more representative.

WE AGREE WITH THE REVIEWER BUT MORE REPETITIONS WITH THE SAME QUALITY AND DATA DISTRIBUTION ARE NOT AVAILABLE IN THE REGION.

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3. Many figures contain subplots. The authors may number the subplots and directly cite the subplots in the text.

WE HAVE MADE AN EFFORT TO BE CLEAR IN THIS ISSUE AND SUBPLOTS CONTAIN THE WORDS "'FALL"' OR "'SPRING"' TO BE PRECISE IN WHAT WE ARE CITING IN THE TEXT. WE HAVE DETECTED THIS TO BE MISSING IN A FEW CASES (Figures 2, 15 and 16) AND HAVE NUMBERED THOSE SUBPLOTS.

4. Line 196. It is not clear what water mass "this last water mass" refers to.

IT REFERS TO SACW. THE TEXT HAS BEEN MODIFIED AND NOW READS AS "'SACW presents maximum..."'

5. The uncertainty of the reference velocity is estimated from the GLORYS velocity, which serves as the a priori error of the unknowns. However, it is not clear whether the reference velocity is taken as 0 everywhere or also estimated from GLORYS. The authors should give information about the reference velocity clearly.

WE HAVE NOW REALIZED AFTER THE REVIEWER'S COMMENT THAT WE DID NOT PRESENT THE REFERENCE LEVEL AS A MOTIONLESS LEVEL. WE HAVE MODIFIED THE TEXT TO CLARIFY THIS ISSUE.

WE HAVE BEEN MODIFIED THE TEXT AS: "'Initially, the reference level is considered as a motionless level where the geostrophic velocity is taken as null before applying the inversion."'

6. Line 232. The deepest common depth is used as the reference depth of each CTD pair. But in case CTD stations are above continental slopes, it should be clarified how the bottom triangle is treated.

THE SAMPLING IS MADE DOWN TO 2000 M AND, AS IT CAN BE CHECKED OUT IN VERTICAL SECTIONS ON FIGURE 2, THERE IS ONLY ONE STATION OVER THE CONTINENTAL SLOPE DURING THE SPRING CRUISE (NUMBER 66). HENCE, WE ARE NOT GIVING ANY SPECIAL TREATMENT TO THE BOTTOM TRIANGLE

FOUND BETWEEN STATIONS 63 AND 66 AS THE UNCERTAINTY FROM THAT SINGLE BOTTOM TRIANGLE IS LIKELY WITHIN THE UNCERTAINITY OF THE WHOLE ANALYSIS.

7. Line 253. Typo error.

THE TEXT IS CORRECTED AS: "'The velocity variance from the annual mean velocity for each layer is estimated with GLORYS and transformed into (...)"'.

8. The inverse model is constructed without considering vertical (dianeutral) transfer of mass. As the authors stated in the introduction, the EBUS is a constant upwelling region due to constant northeasterly winds. What is the influence of the vertical mass flux on the lateral transports? Many of the inverse studies (e.g., Ganachaud 2003; Lumpkin and Speer 2003; Hernandez-Guerra et al., 2005, 2014; Fu et al., 2018) include dianeutral fluxes in different forms in their inverse models, although the dianeutral fluxes are usually not significantly different from 0. It may be convenient for the authors to provide some comments on the sensitivity of inverse solutions to vertical fluxes in the studied region.

WE AGREE WITH THE REVIEWER AND HAVE ADDED THE NEXT TEXT AT THE END OF SECTION 4: "'According to the previous documents north of the Canary Islands, dianeutral velocities are of the order of 10^8 m s^{-1}, while dianeutral diffusion coefficients are of the order of 10^6 m^2 s^{-1} (**?**). The model results are much less affected by these values than by the reference velocities: a mean dianeutral velocity of 10^8 m s^{-1} would contribute with only 0.01 Sv, a value much less than the lateral transports obtained from the inverse model. On the other hand, the inverse model provides information only from the box boundaries and cannot be used to infer any detailed spatial distribution of dianeutral fluxes in the coastal transition zone."'.

9. Line 274. It should be indicated which period is used to calculate the mean SLA.

THE MEAN SLA FOR EACH CRUISE IS ESTIMATED WITH SLA FIELDS PRO-

VIDED DURING THE TIME PERIOD THAT EACH CRUISE WAS PERFORMED. THE TEXT IS MODIFIED AS: "'These results are validated by comparison with the surface geostrophic velocity and the sea level anomaly, SLA, derived from altimetry during the time period that each cruise was performed. To do this, the average fields of SLA and geostrophic velocity at the sea surface are calculated during each cruise and shown as a representation of the synoptic situation during both surveys"'.

10. Line 277. It would be better to indicate the exact position of the "remarkable" eddy.

ACTUALLY, AT LINE 277 WE ARE REFERING TO OVERALL MESOSCALE ACTIVITY IN THE SECTIONS. LATER ON, AT LINE 279, WE REFER TO THE PARTICULAR CASE OF A SINGLE EDDY.

11. Line 327. I assume the "points where the concentrations of DOC are taken" refers to the horizontal position of the stations where DOC are measured? Please indicate that clearly.

YES, THE REVIEWER IS RIGHT. THE VELOCITY FIELD IS INTERPOLATED HORIZONTALLY TO THE COORDENATES (LAT,LON) OF THE STATIONS WHERE DOC ARE MEASURED, KEEPING THE DEPTHS, IN WHICH THE VELOCITIES HAVE BEEN CALCULATED. THE TEXT NOW READS AS: "' the velocities are horizontally interpolated to the locations where the concentrations of DOC are taken"'.

12. Throughout the study, the GLORYS/GLORYS-BIO outputs are used. The authors compared the surface layer transport of the inverse solution with AVISO, but they did not show direct comparison of the inverse solution with GLORYS. It is interesting to see to what extent the inverse results agree with the assimilation model, or the other way around. A comparison between the two would serve as a two-way verification. From the assimilation results, timeseries of transports may be calculated and a seasonal cycle may be extracted. These would provide the readers useful information about long-term fluctuation and how representative the inverse estimates are in terms of seasonal, interannual, and long-term variability.

THE FIGURE 14 HAS MODIFIED AND IT IS ATTACHED HERE. THE REVIEWER COULD COMPARE THE INVERSE SOLUTION WITH GLORYS FOR EACH CRUISE.

13. If it convenient, the authors may consider to reduce the number of figures. For example, Figures 1 and 14 may be merged.

WE CONSIDER THAT EVERY FIGURE INCLUDED IN THIS MANUSCRIPT IS RELEVANT. THE FIRST ONE IS IMPORTANT TO EMPHASIZE THAT THE COLLECTED DATA IS NOT HOMOGENEOUS DURING EACH CRUISE. ON THE OTHER HAND, THE FIGURE 14 IS IMPORTANT TO EMPHASIZE HOW THE POSITION AND ORIENTATION OF THE CVFZ AFFECT TO THE TRANSPORTS, BEING ONE OF THE MAIN ISSUES OF THIS STUDY.

14. Line 417. Typo error.

WE HAVE BEEN MODIFIED THE TEXT AS: "'In general, the estimated transport of the three IN shows similar pattern, very marked by the mass transport variability during both seasons"'.

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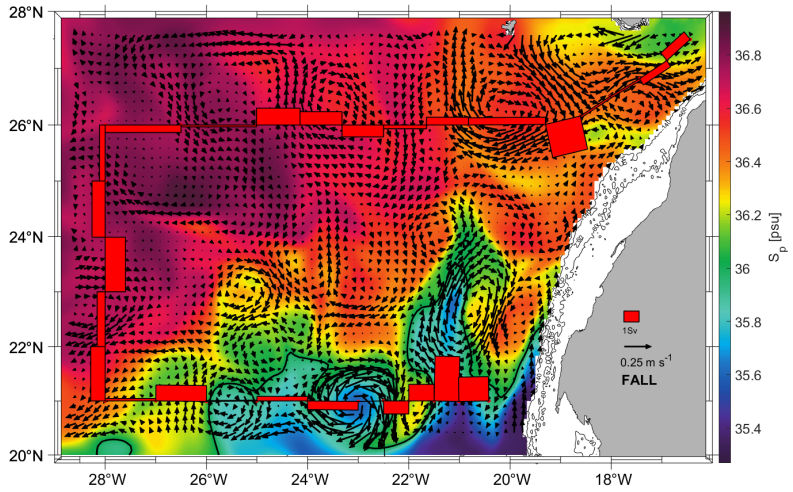

Fig. 1. FALL_GLORYS_VERSUS_MASS_TRANSPORTS

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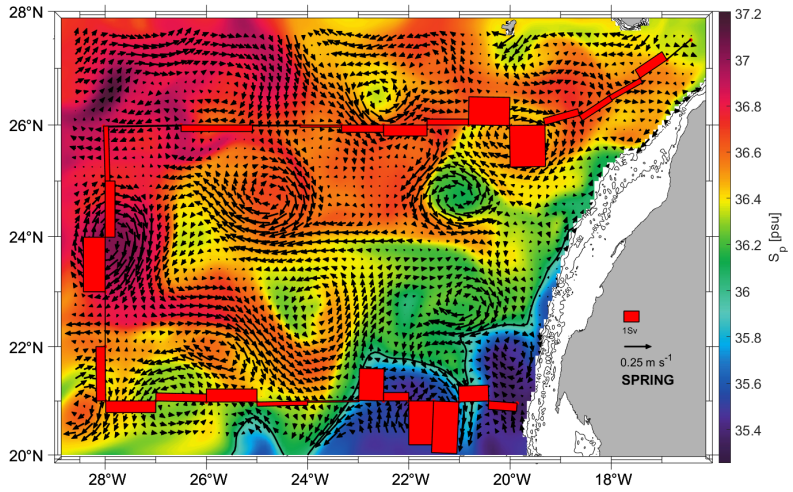

Fig. 2. SPRING_GLORYS_VERSUS_MASS_TRANSPORTS

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