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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.

Anonymous Referee #1

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This work focuses on the seasonal differences in mass and nutrient transports in the upwelling region off northwestern Africa. A inverse model is applied to two sets of closed hydrographic boxes measured in fall 2002 and spring 2003, respectively. The solutions of the inverse model in the two seasons are compared to quantify the differences in horizontal circulation and the resultant mass, nutrient, and DOC transports between fall and spring. The authors have made a lot of efforts to conduct a number of analyses and presented interesting results showing the seasonal differences, which may provide reference for other observational studies and be useful for validation of model outputs.

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Discussion paper

However, I do not think this manuscript could be published in the current form despite the efforts made by the authors. I would recommend a series of modifications that may to some extent strengthen the conclusion of this work.

Main comments

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

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.

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...".

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Discussion paper

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.

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.

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.

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

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

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.

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.

7. Line 253. Typo error.

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 re-

gion 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.

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

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

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.

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

14. Line 417. Typo error.

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