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Interactive comment on "Lateral transports of mass, inorganic nutrients and dissolved oxygen in the Cape Verde Frontal Zone in summer 2017" *by* Nadia Burgoa et al.

Anonymous Referee #2

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This paper presents the analysis of a hydrological dataset in the Cape Verde Basin. The analysis differentiates contributions from the North from those from the South in the context of the Cape Verde Frontal Zone. This dataset and its analysis is worthy of publication, but the current limitations of the analysis mean that the conclusions are not robust enough. I have identified the major problems below. I sometimes had difficulty understanding what the authors had done or meant, so it is possible that some of my remarks are related only to a misinterpretation of the text.

The use of an inverse method to estimate volume, oxygen and nutrient transports from in situ data based on geostrophic balance and conservation equations (volume, trac-

ers) is now a well-established approach in oceanography. Several formalisms exist, but they all amount to solving a least-squares problem whose solution will depend on errors in the observations and conservation equations used. Statistical tools exist to verify that the solution obtained is consistent and does not violate the assumptions made a priori. The application of the inverse method in this paper suffers from various limitations:

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.

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

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

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.

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.

6. What is missing is an a 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.

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.

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.

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

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

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.

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.

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.

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.

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.

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.

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