BG-2019-504 Responses to referee comments

Deadline for revision: 19 march 2020

We gratefully thank the reviewers for their thorough reading of the manuscript and their constructive comments. Responses hereinafter and changes in the revised manuscript are indicated in blue.

Anonymous Referee #2

General comments

In the paper "Profiling float observation of thermohaline staircases in the western Mediterranean Sea and impact on nutrient fluxes" cruise and float temperature and salinity profiles are used to characterize thermohaline staircases in the western Mediterranean. The spatial and temporal coverage of the cruise data is limited, but it is nicely complemented with the float profiles, which show the large spatial extension and temporal persistence of the staircases. The authors also use nutrient profiles collected during the PEACETIME cruise to assess the role of turbulent and salt-finger diffusion for the nutrient enrichment of Levantine Intermediate Waters along their path across the Western Mediterranean basin. My overall evaluation of the manuscript is positive, and I think it should be suitable for publication after some revision.

We have addressed all the comments proposed by the reviewer, details are given in the responses of each comment.

Specific comments

Manuscript structure. The goals of the study are quite broad (including a characterization of the structures, its temporal and spatial persistence and their role in the nutrient budgets), and the authors use and mix data from different sources, which makes the manuscript a bit dense sometimes. The novelty of the results should be stressed more clearly from the beginning. For example, I feel the abstract is quite long and contains some general statements, but the description of the main results, their novelty and implications is quite vague (the same applies to the conclusions). I would also suggest to shorten some parts of the manuscript, where many details are given, for example in section 3.2 you could go to more straight the point. That may help to make the manuscript more easy reading. Also, I like that you included phosphorus in the nutrient part, but I don't know how useful it is for the point you want to make, and it increases the manuscript length.

Following the Reviewer's suggestion, several parts of the manuscript have been shortened, in particular the section 3.2, and the phosphate profiles and fluxes have been removed. The novelty of the results has been clarified in the abstract and the conclusion of the revised manuscript.

Nutrient flux calculations and uncertainties. One of the main novelties of the present study is to provide estimates of diffusive nutrient fluxes to assess their role for the fertilization of LIW. However, I think the description of the calculation and results are a bit too concise and lack of a serious assessment of the uncertainties (see also next point). For example, for the calculation of the nutrient fluxes it is critical to properly estimate the vertical nutrient gradient

(or diapycnal for Equation 7, in the nutricline). Yet, not much information is available about this. Where the gradients calculated from a mean nutrient profile in each basin? How variable are nutrient profiles within a basin (Inter-basin variability seems quite high in Figure 13)? In which depth range was the calculation done and how? The vertical resolution seems quite coarse in the transition layer (Figure 13), how does this affect the results. Overall, uncertainty estimates should be included in Figure 15.

A particular attention has been drawn to the description of the nitrate dataset (new section 2.2) and the calculation of the nitrate fluxes (now detailed in table 6, with the identification of depth/density/nitrate intervals from the Figures 13, 14). The flux calculations have been reconsidered with increasing uncertainties over three distinct cases (last paragraph of the section 3.3). The Figure 15 now specifies a range of values for nitrate fluxes.

Uncertainties of diffusivity parameterizations. The authors should better justify the choice of the diffusivity parameterizations and assess the uncertainties, both for turbulence and double diffusion. Some of the existing parameterizations for salt-fingers diffusion (eg. Kelley, 1990), do not always compare well with direct estimates molecular diffusion across the interfaces (eg. Umlauf et al., 2018). How does the Radko and Smith (2012) formulation compare with the more classical Kelley (1990) parameterization in your case, for example? Regarding turbulent mixing through the nutricline, you used ε values from the literature. How this affect your flux estimates? What is the magnitude of the uncertainty associated with this assumption? You could consider using some Thorpe-scale based parameterization (eg. Park et al., 2014) applied to the cruise CTD data, to obtain some in-situ estimates of ε. Overturning motions appear evident above the salinity maximum in Figure 7, for example. ε estimates using this information should be possible. Due to the coarser resolution of the floats, this approach is probably not suitable in this case.

Following the reviewer's suggestion, two other formulations have been considered to compute the salt diffusivities across the transition zone (detailed in section 2.4). They provide a range of uncertainties for this parameter, that have been considered in the evaluation of nitrate fluxes (Table 6). Note that the formulation of Kelley (1990) is only applicable to diffusive convection (instable temperature profiles). In the same way, the Thorpe-scale based parameterization suggested by the reviewer has been followed to build a look-up table for dissipation rates (presented in table 2). The values of the look-up table have been discussed with historical measurements (section 2.4) and used to provide a range of values for nitrate fluxes (table 6 and Figure 15).

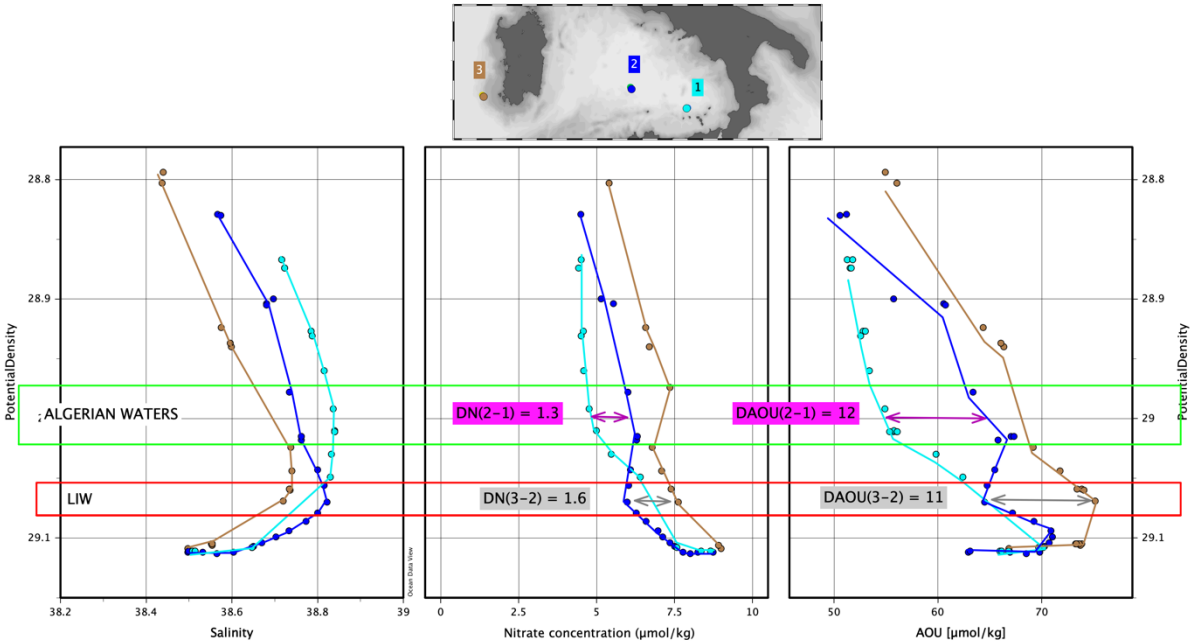
Representativeness of the nutrient fluxes. In my view, the strength of the study is the use of float data to significantly extend the spatial and temporal coverage of the observations of thermohaline staircases. On the other hand, the weak point is that this extensive coverage does not apply to the nutrient fluxes. Why you did not use biogeochemical data from the floats? Didn't they include a nitrate sensor? I wonder whether, even if this information is not available, you could still think of using some local potential density – nitrate relationships, or other similar approach, to generalize your results to the float profiles, and better quantify the uncertainties.

Following the reviewer's suggestion, SUNA data collected by the float 6901769 deployed in the Tyrrhenian Sea (the only one equipped among the 5 considered floats) has been added. This dataset is described in section 2.2. It provides a mapping of the particular nitrate layout of the local nitrate maximum above LIW inferred by nutrient-rich waters coming from the

Algerian Basin (new paragraph in section 3.4). It also allows to quantify the uncertainty of the nitrate fluxes above LIW, over a broader range of situations, seasons and areas (new paragraph in section 3.4 and Figure 15).

The role of the biological carbon pump for LIW fertilization. This aspect is briefly discussed in lines 574–582, but I think is relevant. I feel this discussion is a bit insufficient and the mechanism is not well explained, in my view. You suggest that organic matter exported from the photic zone reaches the LIW layer and it is remineralized there, contributing to an important fraction of the observed nutrient enrichment, is that right? It is nice that you link the nitrate fluxes into the photic zone with other production estimates, but I think you should strength the connection with your observation of the nutrient enrichment of LIW through organic matter remineralization. If this is a dominant mechanism you should observe an increase in apparent oxygen utilization between the Tyrrhenian sea and the Algerian basin in the LIW layer. Do you observe this? Is this comparable to the nutrient increase, in terms of Redfield stoichiometry?

The Section 4.4 has been re-organized and rewritten to clarify the proposed mechanisms of LIW fertilization in the Tyrrhenian Sea. The suggestion of the reviewer to explore further the mechanism of remineralization using apparent oxygen utilization has not been inserted to the revised manuscript because the addition of new parameters, the presentation of new notions would extend and heavy the manuscript, compared to the added value of the result. This point will certainly be developed using in course deployments of BGC-Argo floats embarking nitrate and oxygen sensors.



The Redfield ratio AOU / NO3 is equal to +10 in theory. It is measured close to 10 in Algerian waters between the entrance of the Ionian inflow and the central station, as well as in LIW between the central station and the Tyrrhenian outflow.

Technical comments

Lines 44–46. I am not sure whether this sentence is grammatically correct

The sentence has been rephrased in the revised version.

Lines 217–220. Indicate the duration of the station here?

The six days duration is now indicated.

Line 231. Maybe “The AMPLITUDE of the temperature-salinity steps..”

Done

Lines 284 onward. This part was confusing for me because I had the feeling that the language was guiding me to interpret the observed variations in the layers as temporal variations but the final interpretation seems to be that they are rather a result of spatial inhomogeneities, is that right? Could you stress this a bit at the beginning?

This part has been shortened considering the reviewer’s comment and the sentence “The two episodes are further analyzed in their geographical context“ has been added before the description of the two episodes.

Line 303. Did you mean eastwards?

Yes, it is corrected in the revised version.

Lines 305–306. What is the physical meaning and implications of the fact that the temperature and salinity profiles are inverted within the layers?

This observation reveals lateral intrusions of heat and salt flowing inside the structure along isopycnals. A sentence has been added to the revised manuscript.

Line 324. The meaning of this sentence looks not very clear to me: “... confirmed the connection between layers of fluctuating properties, characteristic of spatial variations rather than temporal changes”.

This sentence has been rephrased accordingly, stressing the continuous characterizations of the layers rather than the changes of layer properties.

Line 334. I would say “closer” instead of “close”

Done

Lines 354–356. For clarity, you may list the stations in geographical order.

The list follows the LIW pathway in the revised manuscript.

Lines 384–385. How do this diffusivities compared with diffusivities through the nitracline?

The diffusivities across the nitracline are now estimated using Thorpe-scale parameterization. They are reported in the table 2. Note that the comparison between Table 6 and Table 2 gives the same order of magnitude (not mentioned in the revised manuscript).

Line 404. “bound” → “bounded”, maybe?

This change has been done.

Line 415. “extends” → “extended”?

This change has been done.

Line 425. Could you show how you get these numbers?

The critical heights as predicted from bulk density ratios are shown in the Figure 3 of Radko (2005). This is clarified in the revised manuscript.

Line438. “According to Zodiatis and Gasparini (1996) that studied”, maybe change to “According to Zodiatis and Gasparini (1996), WHO studied”.

This change has been done.

Line 449. “nearby” what?

This sentence has been clarified.

Line 482. “reduction of sensing aperture”. Sorry, I am not sure of understanding this. Do you refer to the vertical extent of the layers?

Right, this part was not clear: the usual ranges of temperature and salinity are truncated inside the Sardinia eddies as they are considered on fixed depth ranges. Because these structures have a deeper LIW core, profiles are observed shifted downwards and “cut” at the 1000m profiling depth. This point has been clarified in the revised manuscript.

Figure 4 and 7. Could you number the layers according to the code in Tables 2 and 3? I think that would help the reader.

Layers numbers have been added in Figure 3 and 7, and the cast numbers have been changed by capital letters to avoid confusion.

Figure 9. The red lines delimiting the “events” are thin and difficult to see, could you improve this?

The red lines are thicker in the new Figure 9.

Figure 10 and 11. I believe some dates are not correctly reported in the caption.

The dates in the captions have been corrected.

Figure 13. Red and purple dots are not easy to distringuish once printed. Could you maybe use a different color?

The purple color has been changes for brown color in the new Figure 13.