Review of manuscript bg-2019-504. "Profiling float observation of thermohaline staircases in the western Mediterranean Sea and impact on nutrient fluxes"

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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.

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.

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.

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.

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.

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?

Technical comments

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

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

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

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?

Line 303. Did you mean eastwards?

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

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".

Line 334. I would say "closer" instead of "close"

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

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

Line 404. "bound" → "bounded", maybe?

Line 415. "extends" → "extended"?

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

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

Line 449. "nearby" what?

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

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.

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

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

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

References

Kelley, D. (1990). Fluxes through diffusive staircases: A new formulation. *Journal of Geophysical Research - Oceans*, 95:3365–3371.

Park, Y. H., Lee, J. H., Durand, I., and Hong, C. S. (2014). Validation of Thorpe-scale-derived vertical diffusivities against microstructure measurements in the Kerguelen region. *Biogeosciences*, 11(23):6927–6937.

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Umlauf, L., Holtermann, P. L., Gillner, C. A., Prien, R. D., Merckelbach, L., and Carpenter, J. R. (2018). Diffusive convection under rapidly varying conditions. *Journal of Physical Oceanography*, 48(8):1731–1747.