

Interactive comment on “Profiling float observation of thermohaline staircases in the western Mediterranean Sea and impact on nutrient fluxes” by Vincent Taillandier et al.

Anonymous Referee #1

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General comments:

This manuscript presents new measurements of thermohaline staircases from cruises and BGC-ARGO floats in two regions of the western Mediterranean Sea, the Tyrrhenian Sea and the Algerian Basin.

The observation of thermohaline staircases in these regions is not new, and it has already been shown that they develop over epicentral regions confined inside large scale circulation features and are sustained by saltier LIW inflows.

The novelty of this work is in the use, together with data acquired during oceanographic cruises, of data from BGC-ARGO floats, which have the potential to sample

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an extended areal (following their drift) over a long period of time, which in the specific work is 4.5 years. Although these types of floats include biogeochemical observations, only CTD profiles measured by them are used in the work.

The objective of studying the impact of thermohaline staircases on nutrient fluxes relies only on the nutrient data obtained during oceanographic cruises, one in particular. This raises some doubts about the robustness of the relative calculations, also considering that the part of methods is not very exhaustive on this point.

However, the paper also contains interesting analysis and results, and although it is lacking in some parts, I think it can be considered for publication in this journal after the following issues have been addressed.

Specific comments:

1) Given the preamble in lines 80-85, one expects to find in this work a rich database of nutrients, including from the BGC-ARGO floats. But the analysis of nutrient stocks is only based on 4 stations in the Tyrrhenian Sea and 3 stations in the Algerian basin. I suggest you rewrite this part, giving less emphasis to the biogeochemical observations that are lacking in other studies, and avoiding mentioning that profiling floats can include biogeochemical parameters, because that's not your case.

2) Check the text from line 120 to 125 because there are inconsistencies when compared with Table 1:

line 122: May 2017 should be December 2017 from Table 1, but it is not a date of the cruise PEACETIME;

line 124: float 6901491 was deployed in May 2013 (Figure 5) but the first profile is on 16 June 2013 (Table 1). Why? Is it correct?

3) Terminology (line 141 and following, Figure 2). In this study the vertical region between a mixed layer and the adjacent one in the staircase profile is called “step”. Although this definition is found in the literature (for example Bryden et al. 2014), the

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term most frequently used to indicate this region is "interface", while the term "steps" generally refers to the overall feature in the profile: "The well defined steps . . . consist of nearly uniform layers separated by thin stratified interfaces" (Radko, J. Fluid Mech., 497, 365-380, 2003). This terminology is also found in Radko, 2005; Zodiatis and Gasparini, 1996; Merryfield, 2000; Falco et al., 2016; Durante et al., 2019; to name a few among many. I therefore suggest using the most common definition, ie the term interface for indicating the vertical region between a mixed layer and the adjacent one in the staircase.

4) Calculation of vertical fluxes (Section 2.3). This is a very important part but some important information is missing or it is not clear enough.

Lines 197 and 201: Why vertical diffusivity "would be" ? What guided the choice of the two methods for calculating it in the two zones? What are the basic assumptions?

Line 195: The statement "the vertical diffusivity of salts (including dissolved inorganic nutrients)" needs a reference. Furthermore, K_{Sf} in equation (5) is for Salinity. The salts composing the Salinity contribute in different proportions, and each of them has its molecular diffusivity. Shouldn't you derive the K_{NO3} and K_{PO4} values from K_{Sf} to calculate their fluxes? Or does K_{Sf} also apply to these individual components, and if so why? Please explain or add references.

As for equation (6),

- why did you choose 0.2 for mixing efficiency?

- it provides an upper bound for K_{rho} (Osborn, 1980). Therefore, also the flux that you calculate with equation (7) will be an upper bound, and you should replace " $=$ " with " \leq " in both equation (6) and (7). I expect this introduces some uncertainty in the consistency of the values shown in Figure 15, with consequences for the conclusions that are drawn from these numbers (section 3.3). Please provide some discussion on the consistency of your calculation.

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As for equation (7), Oman and Mahadevan (2015) proposed a model for NO_3 , based on specific assumptions, that you export to PO_4 and to the Mediterranean Sea. Please support it.

5) Line 219: "... Falco et al., 2016", please add Durante et al, 2019.

6) Lines 223 and 279: "which is lower than 1.7, the threshold for the development of thermohaline staircases" . . . I suggest to specify "the upper threshold", or to replace with "which is in the range for the development of thermohaline staircases".

7) The paragraph from line 305 to line 308 is quite confusing. Can you perhaps highlight in the Figure 10 the profiles or their parts that you think are "jumbled" by using a different color? Do you really mean that the profiles are "jumbled" or maybe their staircase shape is not well defined in some part of the profile? Also the sentence "the temperature and salinity profiles with depth-decreasing values are locally inverted in the depth range of the transition zone" is not clear. Please rewrite it more clearly. To make it easier to compare upper and lower right panels of Figure 10, can you identify the 5 steps also in the upper panel?

8) Lines 315-320, Figure 11: As previous comment.

9) Lines 411-413: You have not mentioned Durante et al. (2019) who documented enhanced salt finger processes near the bottom after 2010, which are attributed to the ingression of a new denser water mass due to the Western Mediterranean Transition. They show an upward lift of several hundred meters of the steps starting from 2010, and the presence of smaller steps below the deepest thick step, whose number also varies with time, starting from the profile recorded in May 2010 until the end of their series (2016).

10) Line 442: "Sparnocchia et al. (1999) confirmed such extension in the Sardinian Channel". This citation is wrong.

11) Lines 506-508: I agree and I think you should emphasize this new result more in

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the Abstract and Conclusion.

Technical corrections:

Line 303: “the float 6901513 drifted westwards” - it is eastwards.

Line 770 (Caption of Table 2): “. . .(Figure 4)” – it is Figure 3.

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