

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 #1

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 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.

On the revised manuscript,

1. we have added a geochemical dataset collected by BGC-Argo float (nitrate concentrations using SUNA sensor). Data are presented in the dedicated section 2.2, results are presented in the section 3.3, Figures 14-15, and discussed in section 4.4.
2. we have improved the robustness of the estimation of the nitrate fluxes considering different parameterizations of the salt diffusivities, and extending the estimations to the nitrate dataset of the float. The formulations and associated assumptions are presented in section 2.4, the results presented in section 3.3, and Tables 2-6.
3. the original result on the evolution of TS properties inside layers is stressed in conclusion and abstract.

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.

The revised manuscript now includes the analysis of an extended dataset of nitrate concentrations, provided both by the cruise and by one BGC-Argo float. A new section 2.2 has been added to describe this dataset.

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;

This point has been clarified in the revised manuscript.

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?

Thanks to the reviewer's comment, we realized that there was a mistake in the cruise of deployment: the float 6901491 has been deployed the 16 June 2013 during the cruise VENUS2 instead of the cruise MEDSEA (that performed one station the 27 May 2013 in the vicinity of the location without float deployment). This mistake has been corrected in the text, in Figure 5a and caption of Figure 5. PI and crew members of the VENUS2 cruise have also been acknowledged.

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 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.

The reviewer's terminology has been followed: "step" changed for "interface", in the text, in Figure 2, and Tables 3, 4 and captions.

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?

This section 2.4 has been rewritten following the suggestions of the reviewer: the alternative formulations have been clarified and detailed, the underlying assumptions posed and checked.

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 suggested by the reviewer #2 and in consistency with float dataset, only nitrates are considered in the study (phosphates have been removed). A reference (Hamilton et al. 1989) has been added concerning the equal diffusivity for nitrate and salinity.

As for equation (6),

- why did you choose 0.2 for mixing efficiency?

We choose this value because the studied regime is transitional between the molecular diffusion regime and the energetic regime, under the assumption that the Reynold number is bounded between 8.5 and 400. This point has been clarified section 2.4.

- 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 “ ≤ ” 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.

The assumption on Reynolds fluxes has been checked in Section 2.4. Following also the comment of reviewer #2, the evaluation of uncertainties in the formulation of diffusivities has been considered in section 2.4.

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.

As phosphates have been withdrawn from the revised version, this comment has not been addressed.

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

This reference has been added in the revised manuscript.

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”.

This point has been clarified with “upper threshold”.

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?

The proposed modifications on the Figures 10 and 11 suggested by the reviewer have not been followed. It is difficult to identify the layer number in the series with pressure because layer pressures encounter important fluctuations (see the Figure 3 upper right panel), which motivated to change pressure for relative pressure in the lower panel. In the same way, we preferred to describe the “jumbled” profiles from high resolution CTD casts shown in Figure

7. Changes in the text have been done following the suggestion of the reviewer 1 and in agreement to make the section 3.2 lighter asked by reviewer 2.

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

Same as response to the previous comment 7.

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).

The comment of the reviewer has been added to the revised manuscript.

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

The geographical extension reported by Sparnocchia et al. (1999) has been corrected with the northwest of Sicily (their section VI and profiles Figure 3a).

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

This part is now mentioned in the abstract and conclusion of the revised manuscript.

Technical corrections:

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

Done

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

Done