Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-418-RC2, 2018
© Author(s) 2018. This work is distributed under
the Creative Commons Attribution 4.0 License.

Interactive comment on "Surface transport of DOC acts as a trophic link among Mediterranean sub-basins" *by* Chiara Santinelli et al.

Anonymous Referee #2

Received and published: 29 November 2018

The paper by Santinelli et al. presents results of Dissolved Organic Carbon (DOC) measurements in the Tyrrhenian area of the western Mediterranean Sea (MS), together with data from models and satellite observations. The overall goal is to explore the impact of the surface circulation on carbon dynamics in the western MS. According to the abstract, the main result is the quantification of the annual DOC input by the advection of Atlantic water (AW) in the Tyrrhenian area (TYS) of 8.8-37.9 10ˆ12 g DOC yr-1. Although it could be true that the advection of AW may play a crucial role in shaping DOC distribution in the TYS, I found the main result highly speculative and the paper although short, not well structured. I therefore will not recommend his publication in BG. I am not convinced that a strategy with a single transect as the one proposed here (Fig. 1) will allow to answer the question of the DOC entrance in the TYS with AW. The

very simple scheme presented in figure 1 considers no flow in the TYS during the summer periods. In paragraph 3, Line 19, it is indicated:" . . .a global anticyclonic circulation settles in the area, preventing the AW from entering the basin (Fig. 1b)". Paragraph 3.3 line 30, it is indicated: "In summer, the AW does not flow into the TYS. . .". In the 2 cases, there were no references to this statement. Nevertheless, the circulation in the Silicy Channel is highly complex and not very well described as documented by several papers (references below) and because it will affect the main result, it appears as a necessary minimum condition (not respected yet) to discuss about this initial postulate and how it may affect the main result. As written before, the paper is not well constructed and can be greatly improved regarding only the form. The beginning of the abstract is not correct for a scientific paper in BG. Many aspects presented in the result section of the paper are not mentioned in the abstract. Are they necessary to answer the central question raised? The paper is not easy to follow. Results and discussion are presented together. It seems necessary to present the results in a separate section and to take more attention on the presentation. The results section begins by a description of the hydrological context which is not a result from the paper! The result section contains some sentences that should appear in the method section. As an example, Line 26: "Doc averages, calculated by vertical integration in the upper 50 m. . .". It is not at the good place and not well explained. Using vertical integration, it is inventories and therefore it should be expressed in mol m-2, and not average concentrations expressed in μM. This is confusing. The discussion should be in a separate section and should focus on some aspects, as for example on the consequence of the simplification used for the seasonal circulation in the Silicy Channel indicating that there was no input of AW in the TYS during the summer periods (Figure 1). Regarding the presentation, DOC measurements are presented using a color bar on Fig. 2 and with symbols and unexplained ranges on Fig. 3d (70-73 μM; 63-64 μM; 56-60 μM; 47-53 μM): why? It is necessary to be consistent in order to help the readers, and, as an example, adding isohalines on a DOC section could may be more helpful to present salinity gradients in the present case. Page 5 Line 18: "As previously reported, DOC distribution is markedly different in November 2006 and 2011. Average DOC concentrations (0−50 m) are ≈ 6 μM lower in 2006 than in 2011, and this difference results in ≈ 0.31 mol Âům−2 reduction in the 0−50 m accumulated stocks (Table 1)". I don't understand. It is lower in 2006 than in 2011, and therefore, it is increasing!? In Table 1, the first column presents DOC concentration from Jan 2009, the third column from Apr 2007... I finally understand that the month was more important than the year for the authors... but it is clearly not straightforward as many other points in the ms. Regarding the large interannual variability, are you sure you will be able to show seasonal variations?... and add a DOC annual cycle section (3.5). January 2009 is clearly different (Fig. 2) but are the others DOC sections presented different? At the end of the ms, the authors propose DOC budget at the scale of the TYS with taking into consideration all the data collected during the 8 cruises on the same section (Fig. 1). It is although highly speculative and need at least to be discussed. DOC measurements are scarce in the MS and it will be of interest to publish these data. Nevertheless, I will encourage the author to find a better way to do it.

References: Rio, M.-H., Pascual, A., Poulain, P.-M., Menna, M., Barceló, B., and Tintoré, J.: Computation of a new mean dynamic topography for the Mediterranean Sea from model outputs, altimeter measurements and oceanographic in situ data, Ocean Sci., 10, 731-744, https://doi.org/10.5194/os-10-731-2014, 2014. Jouini, M., K. Béranger, T. Arsouze, J. Beuvier, S. Thiria, M. Crépon, and I. Taupier-Letage (2016), The Sicily Channel surface circulation revisited using a neural clustering analysis of a high-resolution simulation, J. Geophys. Res. Oceans, 121, doi:10.1002/2015JC011472. Gerin R., Poulain P.-M., Taupier-Letage I., Millot C., Ben Ismail S. and C. Sammari, 2009. Surface circulation in the Eastern Mediterranean using Lagrangian drifters (2005-2007). Ocean Science, 5, 559–574.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-418, 2018.