

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

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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 $8.8\text{--}37.9 \cdot 10^{12}$ g DOC yr⁻¹.

We will rephrase the abstract to better clarify that the main result of the paper is that lateral advection of the AW plays a crucial role in regulating DOC concentration and dis-

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tribution in the Tyrrhenian Sea and that horizontal transport of DOC into the Tyrrhenian Sea is of the same order of magnitude, or even larger, than the in-situ DOC production. Our study addresses this process for the first time in the Mediterranean Sea. The small size of the basin allows for short transfer times which, in turn, favors the preservation of the DOC stock produced elsewhere. However, this transfer may be important also in other regions of the oceans. In this scenario surface advection may set up, via horizontal transfer, a sort of compensation among regions with different trophic regimes, smoothing trophic gradients. We believe that understanding these transport processes is a crucial and preliminary step to understand and quantify all the other processes (biological, chemical, geological) that influence DOC distribution on a variety of timescales.

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.

We definitely disagree with the fact that the result is highly speculative. Coupling circulation patterns and transport, derived by a fully validated, basin-scale hydrodynamic model, and repeated DOC data on a 6-station section, we believe that we have provide convincing evidence that AW advection plays a crucial role in shaping DOC stocks and distribution in the basin. Of course, there are uncertainties in the total amounts, which we have discussed in the text but the fact the highest DOC transport is associated to the AW is clearly showed by the data. In addition, this section of the Mediterranean Sea is a very dynamic, but tractable, study site for understanding biogeochemical process that could, in principle, operate over broader scales of time and space. We do not understand either why the paper is not well structured. We agree that some paragraphs might be moved in other sections and that other ones can be improved in clarity, but it is difficult to understand why the paper, after better clarifying the parts that have raised perplexities in the referees, could not be published.

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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.

We obviously agree with the referee that a larger data set would reinforce our conclusions, but we disagree that a single transect is not the proper sampling scheme when the issues at stake are water and tracers transport. Indeed, Geosecs, WOCE and, more recently, Geotraces have all organized their sampling along 'single' transects. The referee should also acknowledge that our DOC data-set has a good spatial and temporal resolution, compared to what is available in the literature. A DOC data-set with a 6-station section repeated for 8 times in different seasons and years is unique and precious, nothing similar is available for the Med Sea nor for the Oceans. There are fixed stations, that cannot give a 3D idea of DOC distribution. In addition the position of the section is crucial, since it located in an area of the Tyrrhenian Sea where a strong stream of AW of remote origin is present every year, for half of the year. We therefore strongly believe that our data clearly shows the link between DOC surface distribution and AW advection and that this work could be the first one to address this overlooked mechanisms opening to further investigation in the field.

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.

Figure 1 does not present a scheme of the circulation; the white arrows are only added to further highlight the different paths of the AW in winter and summer. We should have better explained the content of this figure in the text. The figure represents a scientific result, since it displays the winter and summer maps of the Absolute Dynamic Topography (ADT; indicated by colors) that we have obtained by averaging over 21 years of AVISO altimeter data (1993-2013), with the corresponding average geostrophic circulations (also derived from the satellite data) superposed. The maps clearly show a

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robust AW stream circling cyclonically all around the Italian coasts in winter (actually from the end of the autumn to mid spring), which is absent in summer. Looking at individual years, one always gets the same picture. This seasonal difference in the surface circulation of the TYS is known since the seminal works by the Russians in the eighties (e.g., Krivosheya, 1983), and has been further analyzed in more recent investigations (Rinaldi et al., 2010; Iacono et al., 2013; Napolitano et al., 2014)

Nevertheless, the circulation in the Sicily 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.

It is not clear to us why the referee is so concerned about the complexity of the circulation in the Sicily Channel, and which postulate he/she is referring to. The complexity and variability of the circulation in the Sicily Channel does not alter the well-known fact that the AW does not enter the TYS in summer (see previous point). We note that, in any case, the main point of the paper is about what happens in winter and beginning of spring, months in which a robust stream of AW (of the strength of about 1 Sv, according to both observations and numerical results) enters the TYS.

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?

We are quite surprised by the statement “Characterizing carbon cycling and redistribution in the ocean is an important issue for Mankind, because it may affect key ecosystem services, e.g., support to climate system and food provision.” is not correct for a scientific paper in BG. Carbon cycle is likely the most discussed theme in Earth System Science in the last decades and the main reason is that its dynamics impact, directly or indirectly, several aspects of human life. The statement was made to frame our study

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in a highly debated issue. Along with the comments of Ref#1 we will rephrase and improve the abstract.

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.

We do not understand what the reviewer means with “to take more attention on the presentation of the results”, we have the feeling that he/she read the paper very quickly, since all the main results are carefully described and presented. We do not understand either why the paper is not well constructed. We decided to keep together results and discussion, because the results are briefly presented in section 3.1, whereas in 3.2, 3.3 and 3.4 we try to combine in-situ data with model results in order to facilitate the discussion of the results. In our opinion, it is therefore helpful for the reader to have data and discussion combined in the same sections. However, if the editor agrees with this comment, it is not a problem to separate results and discussion.

The results section begins by a description of the hydrological context which is not a result from the paper!

As reported in the text, that section was added to recall some basic elements of the surface circulation in the WMED. However, in agreement with this comment we can move this section to the introduction or add a section dedicated to explain in details the main features of the TYS circulation, before the results.

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.

We agree with this comment, the sentence is not clear. The average concentration was

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calculated dividing the inventories, calculated by vertical integration in the 0-50 m, by the depth (50 m) of the layer. We can easily move this sentence in the material and method section explaining better the calculation.

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 Sicily Channel indicating that there was no input of AW in the TYS during the summer periods (Figure 1).

We do not understand why the referee keeps insisting on the role of the Sicily Channel. The fact that the AW cannot penetrate into the TYS in summer is due to the local dynamics of the TYS, since in this season a broad anticyclonic circulation forms in the southern part of the basin (see, e.g. Krivosheya, 1983; Artale et al., 1994; Marullo et al., 1994; Pierini and Simioli, 1998; Korres et al., 2000; Iacono et al., 2013; Napolitano et al., 2014; among others). In any case, what is more important is what happens in winter and spring, when the AW inflow takes place, so we do not see why we should further focus on the summer dynamics and on what happens in the Sicily Channel.

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.

We totally disagree with this comment. We think that data presentation is consistent, we are just trying to help the readers giving a different way to look at the data. In fig 3d we try to give an integrated view of salinity distribution, current velocity and DOC distribution, in order to show how DOC distribution is shaped by AW flow. Even if we agree that the figure needs a little attention to be understood, it is not so complicated and after a lot of work this was the best way we found to give a complete and integrated view of DOC, salinity and velocity along this section. We are happy to try other

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way to represent these data if the reviewer gives us a constructive suggestion. In the DOC vertical distribution we added isohalines because they are directly related to the circulation.

Page 5 Line 18: “As previously reported, DOC distribution is markedly different in November 2006 and 2011. Average DOC concentrations (0–50 m) are $\approx 6 \mu\text{M}$ lower in 2006 than in 2011, and this difference results in $\approx 0.31 \text{ mol Al}^3\text{C}_u \text{ l}^{-1}\text{m}^{-2}$ 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!?

We do not understand this comment. We say that DOC concentrations (0–50 m) are $\approx 6 \mu\text{M}$ and the stocks are 0.31 mol m^{-2} lower in 2006 than in 2011, we did not say that the DOC concentration 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.

We gave more importance to the months because the Tyrrhenian sea displays a bi-modal physical dynamics that depends on seasons. Therefore is the seasonal signal that must be captured at the best, within the limits of our data-set. The interannual variability, which we also could relate to the interannual variation in the physical dynamics (see below), was not the scope of this study.

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?

Indeed, the difference between DOC distribution and concentration in November 2006 and 2011 is clearly due to the anomalous pattern of circulation observed in 2006. In our opinion the annual cycle of DOC is clearly visible in figure 2 and Table 1, as clarified

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in the text

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.

We can rework the section discussing the limitation of our estimate. Although speculative we believe that these calculations provide substantial evidence of the relevance of AW input in DOC dynamics in the TYS.

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.

We thank the referee for the acknowledgement and we believe that our analysis is robust and gives convincing evidence that AW advection plays a crucial role in shaping DOC stocks and distribution in the basin. However, we will be happy to follow any constructive suggestion that could further improve the quality of this study.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2018-418/bg-2018-418-AC2-supplement.pdf>

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

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