

Authors replies to the interactive comments of anonymous referee #2 (21 July 2019) on “Seasonal variability of intermediate water masses in the Gulf of Cadiz: implications of the Antarctic and Subarctic seesaw model” by David Roque et al.

---

**RC:** denotes referee’s comments

**AR:** denotes authors’ reply

---

**RC:** In past studies, the Eastern North Atlantic Central Water (ENACW) was separated in a subtropical and a subpolar fraction (i.e. Voelker et al., 2015). Yet, the authors only define ENACW without specifying its origin. The authors instead define Subarctic Intermediate water (SAIW) which I am not sure what the difference is between the previously reported ENACW of subpolar origin. Perhaps the authors could clarify this for the reader. I think this would help to relate their study better to previously published papers on the modern hydrography of the region.

**AR:** Reviewer is right as to the convenience of clarify this subject. There is no general agreement in the denomination of these water masses along the existing literature. We have added some text in order to clarify these denominations, also considering new references to Voelker et al., 2015 and other works.

New added references:

Rios, A.F. Perez,F.F., Fraga,F., Water masses in the upper and middle North- Atlantic Ocean East of the Azores. Deep Sea Research. Part A Oceanogr. Res.Pap.39 (3-4A), 645–658, 1992.

Voelker A.H.L., Colman A., Olack G., Waniek J. J., Hodell D., Oxygen and hydrogen isotope signatures of Northeast Atlantic water masses, Deep Sea Research Part II: Topical Studies in Oceanography, 116: 89-106, 2015.

**RC:** Secondly, the authors argue that AAIW “pushes” MOW up the shelf during autumn. I was wondering how these findings relate to the seasonal changes in MOW density. During autumn, MOW reaches its annual density minimum (Millot et al. 2006). Though my question is, does the MOW flow higher up on the shelf during autumn due to the AAIW pushing it up or does the less dense MOW simply settle higher up on the shelf by itself, and thus allows AAIW to extend vertically within the water column?

**AR:** Reviewer has provided a very interesting point which deserves to be included in the interpretation of our results about the relation between AAIW and MOW. Unfortunately, our observations do not allow to assess what of the two mechanisms may be behind this behavior. However, taking into account the information published in Millot et al. (2006), the possibility for a less dense MOW be settled closer to the slope leaving room for the AAIW penetrates easily into the Gulf of Cadiz may not be discarded. These ideas are added in the modified version of the manuscript.

New added reference:

Millot C., Candela J., Fuda J.L, Tber Y., Large warming and salinification of the Mediterranean outflow due to changes in its composition, Deep Sea Research Part I: Oceanographic Research Papers, 53 (4), 656-666, 2006.

**RC:** AAIW intrusion into the Gulf of Cadiz and its interactions with MOW. Millot et al. (2006) argued that MOW become more saltier and warmer after the 1990s. The change in MOW characteristics between 1960-1980 vs the 1990-2000s is often used as an analog for glacial-interglacial changes of MOW conditions. Hence, I am wondering if the authors see any change in AAIW presence and simultaneously MOW settling depth between 1950-1980 and 1990-2000s? I feel that including this kind of temporal information could substantially enhance the current discussion under section 4.2. where the authors try to hint at this relationship and its possible interest for paleoceanographers but do not provide any real new insights.

**AR:** Certainly, the suggested subject is very interesting and consequently we have tried to deal with it. Unfortunately, the spatial coverage of the observations is not adequate for performing such analysis. Note in the attached figure, where indicated are the locations of the available casts, the lack of data in the spring and summer seasons for the period 1990-2018.

**RC:** p.2 Lines 2 to 4: Please rephrase this sentence. It is very long and contains too many information. Please break it up into at least two sentences. Although is meridional heat transport not the same as MOC? Does meridional freshwater transport not imply THC?

**AR:** Reviewer is right, we have taken into account his comments and have redrafted this lines accordingly.

**RC:** p.2 Line 4: What do you mean by “their”? THC or MOC or any other transport you listed previously? Specify.

**AR:** Reviewer is right, the phrase has been redrafted and now it leaves clear that we refer to the intermediate water masses.

**RC:** p.2. Line 12-13: What do you mean by “cold” and “warm” periods? Interglacial and glacial? Stadial and interstadials? Specify.

**AR:** In the new version this subject is clarified and specified.

**RC:** p.2 Line 25 to 26: Is this not more or less the same information as provided in Lines 15-17. Please check if this sentence and the above-mentioned sentences one cannot be spliced together.

**AR:** We have removed the duplicity of the information as reviewer suggests.

**RC:** p.4. Lines 5 to 9: Did you compare the wind data between 1979-2018 only to hydrographic data for the same time frame or did you use the entire data set from 1900 to 2013 for that? If you used the entire time series of the hydrographic data set would that not be problematic given the effects that global warming had on wind field changes in the last decades?

**AR:** Reviewer is right in making this question. However, in our analysis the wind data we used match with the WOA data in the section located at 36 ° N and not with the observations. We have added some lines to leave more clear this subject in the section 2.1, where we describe the data set.

**RC:** p.5 Line 11ff: Generally, I am missing some statements here regarding the data preprocessing steps, and the software (e.g. MATLAB?) used for the analysis. To the best of my

knowledge PCA requires the data to be gaussian distributed prior to analysis. How did you pre-process your data?

**AR:** The analysis has been performed using the MATLAB function PCA. It is now indicated in the manuscript. In the PCA observations are not imposed to follow a Gaussian distribution. What reviewer must mean is that the observations must be referred to their averaged values before the PCA application. In the we have redrafted this section in order to allow for more clarity in the description of the technique and the way it will be applied to the observations new version on the manuscript.

**RC:** p.6: Line 18 to 24: This is does not like a result. Does this paragraph really require a section header and number? I find this bit somewhat confusing.

**AR:** We think that a previous description of the spatial distribution of the variables along the different seasons is convenient and choose to emplace it as a first subsection within the results section, we think that this option is better than dedicate an additional section dealing with the descriptive analysis.

**RC:** p.7 Line 20: What do you mean by “: : seems to have a certain intra-annual variability,..”. Please elaborate on this if you consider it important; if not rephrase it.

**AR:** We have redrafted the sentence accordingly the reviewer suggestion.

**RC:** p.10 Line 31: I think this is a typo by resolution of “1o” of the wind: : do you mean 10?

**AR:** Effectively, there is a typo mistake, actually it should read as 1° (one degree) when talk about the Machin and Pelegri (2009) analysis. As we described in the methodology section, the wind data providing by the ERA-interim reanalysis have a much better spatial resolution (0.125 x 0.125 degrees)

The rest of minor corrections have been corrected following the reviewer suggestions.