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Interactive comment on "Definitive evidence of the Mediterranean Outflow heterogeneity. Part 2: all along the Strait of Gibraltar" *by* Claude Millot

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Dear Dr. Claude Millot,

Now that I have received two reviews of the Part-2 paper of the series that you submitted for publication to Ocean Science (reference OS-2017-53). You also provided answers to the referees' comments. The period of Open Discussion is now closed (since 2 February).

Although you indicated in your answer to my recommendations relative to the Part-1 paper that you have decided to re-handle your trilogy and to resubmit all your work to Ocean Science before mid-April 2018, it is necessary to finish the review process of this Part-2 paper. I therefore address my recommendations for the revision of this

paper.

Note that because of your decision to re-submit you whole work in a different form (4 papers), I do not ask you to respond point by point to my comments or recommendations, but to seriously consider them when re-structuring and re-writing your work.

I carefully read the referee's comments to your paper and the answers you provided. I also read the paper myself. I consider that the paper presents relevant evidence of the heterogeneity of the MW outflow in the Strait of Gibraltar, but also that it suffers from major presentation flaws of similar nature as the Part-1 paper that lead me to ask for a major revision.

Note that I shall provide you with my recommendations for Part-3 paper in a few days, and this will be the occasion to comment on the ensemble of the trilogy.

Both referees express a need for clarification, simplification and synthesis. I had myself a great difficulty to read throughout the paper and to identify its main scientific outcome. The description of the T,S profiles (Fig. 2 and Fig. 3) is very fastidious, especially because it is not streamlined and no effort is made to synthetize the analysis regarding the objectives of the paper. So many details are given in such long sentences, that it is difficult to relate them to the main conclusion of the paper that the MO is organized in as a set of different components juxtaposed side by side. In addition, the paper very often mentions results presented only in the Part-3 paper so we feel that we are reading the conclusion of this Part-3 and lose the purpose of the analysis of Part-2. This should be avoided in the revised version. The paper also presents extended considerations of already published results (especially in the discussion) that are not directly relevant for the focus of the paper. Note also that the frequent use of words like tremendous, dramatic, astounding, etc., often seems out of proportion with regard to the changes or results they qualify. The effect, as I feel it and as mentioned also by referee #2, makes more harm than good to the results you want to convey. You may consider this remark when revising the papers.

Referee #1 main concerns are on the text clarifications. I recommend that your take these remarks seriously in revising the paper.

Referee #2 is far more critic regarding the paper. I retain three main comments that you must consider while revising the paper. Referee #2 notices a polemical character in the paper and insists that this does not have its place in a major journal. The emphasis given to the homogeneity vs heterogeneity controversy is also qualified as excessive. I read several papers mentioned in this “controversy” to make my own opinion. I tend to share the opinion of referee #2. This controversy deserves only a minor importance and seems to me as being semantic rather than scientific, and to rely mainly on what is meant by heterogeneity. As I recommended for the revision of the Part-1 paper, you should be more moderate and less focused on the “controversy”. I also agree with reviewer #2 that the assertion given in the paper that the heterogeneity of the MO and the mixing with the AW is sufficient to explain the splitting into several veins and to neglect the role possibly played by the bottom topography. The analysis presented here does not support such a statement, which to be acceptable should also provide dynamical evidence of the no-role played by the topography. I can mention here a study that shows that the mixing induced by local change in the topography has a major impact on the mixing of the MO (e.g. Nash et al., 2012, G.R.L., Vol 39). Also the topographic steering of the MO should certainly have consequence on the acceleration of the MO veins and therefore enhance the Coriolis effect. You correctly mention that the MW and the AW continuously flows with large velocities (e.g. lines 198-211) and that certainly influences the mixing. However, the discussion of mixing presented in the paper generally discuss the vertical mixing as an isolated process, the influence of the advection remaining qualitative when mentioned. The remark made by Reviewer #2 that the discussion should consider the difference in the diffusive and advective time-scales is quite pertinent. If as indicated by the referee the vertical mixing time scale is 20 time longer than the advective one, this could have an impact on your analysis. Baringer & Price (1997 already distinguished the local mixing processes and the mixing related to entrainment (Bulk mixing process). The discussion of mixing and entrainment should

attempt to be process oriented and quantitative. The study, which is essentially based on a visual interpretation of T, S diagrams, is hardly situated in a synoptic circulation context (no circulation map is shown that could link the various observation sites). The revised paper should attempt to enhance the links of the analysis with the circulation.

Allow me a personal comment. After reading several papers presenting studies of the MO in order to increase my scientific knowledge of the ocean dynamics of the Strait, I think that the word “definitive” used in the title is not pertinent. The reason is that the definition of heterogeneity/homogeneity is relative and will likely remain so (it depends on the magnifying glass used to look at it). For example, the study of Naranjo et al., (DSR 2015) pretend that west of the sill, speaking of a unique MW seems appropriate. You probably disagree with such a statement, but it is consistent with the data analysis performed in this study (which uses a cluster analysis as magnifying glass). A different approach, for example the one you propose, can yield a different vision. Nevertheless, the one proposed in Naranjo et al. is consistent with the analysis that was performed. This emphasizes the relativity of the notion of homogeneity of the MO. Note that the comment of the second referee of the Part-3 paper about the destruction of the “myth of a homogeneous MO” goes in the direction of my remark.

Here is a comment that concerns your answers to the referees. The tone of your answers surprised me, as it appears often personal and even conflictual, not in proportion to the critics or remarks made. I think that this is largely why referees did not answered to your comments, which at the end is damaging to the open discussion process. I expect a more moderate attitude in the following of this review process.

Just a last short comment to tease you on your strict attitude about the use of Names and Acronyms. I do not find very consistent your use of Northern Ocean for the North Atlantic when you are commonly using Surface Atlantic Waters (SAW) and North Atlantic Central Waters (NACW). Shouldn’t you use instead NOSW for Northern Ocean Surface Waters and NOCW for Northern Ocean Central Waters? I do not see any reason for not giving the North Atlantic its common name. Use the names and acronyms that you think appropriate but please let others the same possibility.

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