

Interactive comment on “Definitive evidence of the Mediterranean Outflow heterogeneity. Part 3: at the Strait of Gibraltar exit” by Claude Millot and Mikhail Emelianov

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Dear Dr. Claude Millot, Dear Dr. Mikhail Emelianov

Now that I have received two reviews of the Part-3 paper of the series that you submitted for publication to Ocean Science (reference OS-2017-54). You also provided answers to the referees' comments. The period of Open Discussion is now closed.

I carefully read the referee's comments to your paper and the answers you provided. They 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

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flaws. Their comments lead me to ask for a major revision.

Because you have already decided to re-handle your trilogy and to resubmit all your work to Ocean Science in a set of papers differently structured, I focus my recommendation of just a few points raised by the referees and your answers to those to elaborate my recommendations.

Note that the final decision regarding the procedure to re-submit your revised series of papers is indicated at the end of this letter.

Recommendations for OS-2017-54

Referee #1 appreciated the demonstration of the heterogeneity of the outflow at the Strait exit, the characterization of the spatial and temporal variability of the Mediterranean Outflow and the suggestions made for future sampling strategies in the Strait. The main remark of referee #1 is that the written text require some clarification and that an effort is required to present the results in a more synthetic way, the actual from recalling sometimes an exhaustive scientific report. You answered to referee #1 that " . . . because we have to "contradict a general idea", our aim was to provide as many arguments as possible in order to try convincing the whole community...". My feeling is that in providing so many arguments, the paper is drowning the most essential arguments that would convince the reader. Fewer but most convincing arguments would better reach the goal. It seems to me that your answer to the remark of referee #1 about the text at line 517 does not bring the requested clarification. The confusion probably comes because no circulation maps is shown, and therefore the location of the transect regarding the position of the MO core is not clearly defined. This suggests the need to better link the analysis with the circulation scheme (as was already pointed out in the review of Part-2). Strengthening this link in the revised paper may increase our understanding of the present analysis. The very long explanation (more than $\frac{1}{2}$ page) that you provide to answer the lack of clarity noted by referee #1 at line 936 is by itself an indication of the difficulty that readers may encounter to understand the figures

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concerned by this remark. You should account for this in the revision of yours papers (referee #2 also mention the difficulty to understand some figures).

Referee #2 provided a very serious, exhaustive and in-depth analysis of the paper. Comments provided are very valuable and most should be considered when revising the papers. In agreement with referee #1 and with the referees of Part-1 and Part-2, referee #2 acknowledges the scientific interest of the results presented in the paper on the characterization of the water masses, and points out the relevance of the discussion of the sampling strategies that is exposed. The referee also points out the extraordinary value of the analyzed data set and qualifies the analysis of careful, deep and detailed and the method (based on coloring of water masses according to their maximum density) of relevant. Referee #2 pointed out several formal issues that makes the paper difficult to read, this difficulty only partly lying in the complexity of the material presented. A large part of that difficulty is in the written text, the understanding of which required reading back and forth many times. The structure of the paper is also concerned, with a large number of figures some of them being complicate or difficult to understand. Referee #2 mentions a serious risk of readers to get discouraged, even abandon, or not to be fully understand the outcome of the paper. I share the view of the referee and consider that the paper must be seriously revised and that a significant synthesis effort is needed to make the paper clearer and the message it conveys much more evident for a large oceanographic community. I have a few comments regarding review #2. First, I think you somewhat misunderstood the objective of the open access review when you say that the review is essentially addressed to the editor. It is perfectly fine for the referee to address comments to the authors, otherwise, what would be the purpose of the discussion? In addition, the process gives you the opportunity to respond to the referee, which you did. It appears clear to me that the OS community working in the Mediterranean Sea has not yet reached a consensus regarding Names and Acronyms. Referee #2 is aware of that and explains the acronyms used in the review. Of course the acronyms you are using in the papers are fine and I do not have any requirements about this. I just consider that the discussion “my acronym is more le-

gitimate than yours” should be avoided here. What is important is that everyone knows what one is talking about. The revised papers should really minimize the discussion of acronyms. Referee #2 recommends avoiding explicit insistence of controversies. I do not insist on this because I do not have anything to add to what has already been said in the reviews of Part-1 and Part-2.

Finally, I have no specific recommendations regarding the specific comments provided by referee #2. Comments and suggestions for improvement or clarification have been proposed to almost every sections of the paper, suggestions with which you do not always agree (as clearly expressed in your response). Nevertheless, these comments have been written is a very positive approach, with a clear intention to help the authors to clarify their text and to make their hypotheses more visible and convincing. Such a detailed analysis of the paper is very rare in nowadays reviews and we should acknowledge the great contribution of the referee. I recommend that you give these comments the greatest attention.

Final decision regarding the review of the trilogy as a whole.

The open access review of the series of papers concluded that OS would reconsider your trilogy after a major revision. You informed me that you will re-handle the series of papers, will re-structure them in a series of 4 papers that you will re-submit. You know that I am not supportive of this decision. As you asked, I have been looking at the administrative way to deal with your decision. I have been discussing your proposition with the executive editor and the Copernicus Editorial Office. The conclusion is that the re-structuring you propose implies the submission of a new set of companion papers and will be considered as a new submission. In consequence, you will be asked to withdraw the previous papers and when your new papers will be ready, to proceed through a regular new submission. We advise that you should take full account of all the referee’s comments. This decision will be sent to you officially by the Copernicus Office with the details of the procedure to withdraw your trilogy. I sincerely think this solution is the best possible for you and for the journal. It gives you all latitudes to

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reorganizes your work according to your own view, and still benefit from the comments that emerged from present the review process. Your new series of paper may also benefit for a reviewer more aware than me of the challenge that the scientific issues you address represents, and having a better knowledge of the oceanography of the Strait. Also, if as I expect your papers get published in OS, (I see no reason for a different conclusion considering their scientific content), the paper will not be “tagged” by a very long review process (submitted 29 june 2017 – published ?? 2018) which can sometimes be detrimental to a paper.

Very sincerely

Bernard Barnier

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