

## ***Interactive comment on “Transformation of Levantine Intermediate Water tracked by MedArgo floats in Western Mediterranean” by M. Emelianov et al.***

**C. Millot (Referee)**

Claude.Millot@ifremer.fr; f4eut@winlink.org

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The paper focuses on CTD profiles through the LIW layer get from MedArgo floats and proposes a fully objective method to classify the profiles into a limited series of clusters. Even though I think that the paper could be markedly improved, I strongly recommend its publication for two major reasons: 1) any kind of vertical profile (and even horizontal ones provide they are repetitive), collected with devices that are autonomous or handled from ships, and whatever the parameter, could be analysed in such a way, 2) analysing a larger set of data spread over time will allow addressing objectively the temporal variability.

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My first general comment concerns the criterion used to select the data set “in the range 29.0-29.1” in density anomaly units. I think that, even though this criterion is one of the simplest, it is far from being the most adequate, mainly because it leads to consider parts of a vertical profile that could concern also large portions of either AW above or TDW/WMDW below. If the authors want to focus on LIW, I think that they should, for instance, i) locate the depths of the theta and S maxima that characterise LIW, ii) define the mean of these 2 depths as the depth of the LIW core and iii) consider some depth intervals above and below the depth core (or even some dtheta or dS above and below). Even if this might represent some non-negligible work, I strongly advise the authors to perform it.

My second general comment is basically a personal opinion about circulation so that the authors can disagree with it, which will not reduce the interest of their work and my recommendation to publish it. Even though the point I want to emphasise maybe concerns the writing only, I provide the authors with these remarks essentially because they seemingly rely on some of my previous papers. I personally disagree with writings such as “LIW is spread following the general circulation”, “LIW is entrained by the alongslope Algerian current”, “LIW sunk down to some level”, “incorporated to the circulation”, etc. Indeed, I think that, except where Mediterranean waters (MWs, i.e. LIW and other ones as well) can be entrained away from the continental slope (as by the Algerian eddies), they first naturally tend to spread (i.e. themselves) from where they are produced and are then maintained along the slope by the Coriolis effect. Therefore, I would have said something like “LIW spreads and circulates”, “All waters in a concentration basin (the MWs and AW as well) circulate counterclockwise along the slope: within both the western and the eastern basins, AW and the MWs circulate together (AW does not entrain LIW); through the major passages (the strait of Gibraltar and the channel of Sicily), they flow in opposed directions”, “LIW sinks down or sank down (not sunk down)”, “it is part of the overall circulation”, etc. Also, I do not make differences between the circulations in the eastern vs. the western one that are as large as indicated by the authors. To have a correct idea of our own understanding, the authors

can have a look at our last paper about the circulation in the whole Mediterranean Sea ([http://www.ifremer.fr/lobtln/OTHER/Millot\\_Taupier\\_handbook.pdf](http://www.ifremer.fr/lobtln/OTHER/Millot_Taupier_handbook.pdf)).

I also strongly advise the authors to improve figures such as 2(b) and 5(b) in essentially 2 ways. One way is to use symbols that would be more representative/meaningful of the mixing stages (circles having different sizes, or grey tones, or  $\ddot{\text{E}}$  etc.); also, the clusters should be numbered “logically”, for instance 1 being the less mixed and 5/7 the more mixed. Another way is to plot, on these figures or on other ones, the envelopes for each cluster, or all profiles considered in a given cluster; indeed, information is provided neither on the differences between the profiles within each cluster nor on the differences between “neighbouring” profiles belonging to different clusters.

As my last major comment, I would like to address the specific problem of the bathymetry and of the location of the MWs veins. I think that important information to be given somewhere concerns the limitation of the method by the bathymetry and/or what is done with “incomplete profiles”. Indeed, if we agree that LIW is flowing alongslope as a vein  $\ddot{\text{E}}$  its “overall core” (i.e. where the overall  $q$  and  $S$  maxima will be encountered) will always be located “just on the bottom”: therefore, it will never be possible to get profiles crossing the “overall core”, i.e. just to get profiles from the core upwards. Similarly, let us assume that the bottom is at  $\sim 1000$  m and that the density there is still in the range 29.0-29.1 (i.e. not larger than 29.1): any profile there will be “incomplete”, even if the (external part of the) vein is crossed, hence even if  $q$  and  $S$  maxima are encountered. I thus think that a specific analysis (or at least comment) should be made for those profiles which are incomplete, i.e. that are located over the upper part of the continental slope.

Specific comments: - P.570, l.24: “westward path”. If the authors agree with my diagram, what I suppose, they should avoid using westward. Waters just spread and, because of the Coriolis effect, they circulate as veins counterclockwise alongslope  $\ddot{\text{E}}$  just “downstream” and in any direction within a basin  $\ddot{\text{E}}$  basically (i.e. except some Bernoulli effect near Gibraltar for instance) as long as the isobaths they follow pene-

trate into some passage leading them to outflow from that basin. - P.570, l.25: “more to the west than the most transformed one” is unclear. First, it must be specified that the sentence concerns LIW along the slope of Sardinia in the Algerian subbasin. Second it should be more convenient to say “that LIW as unmixed as the LIW generally found alongslope can be found nearby in the interior of a subbasin”. - P.571, l.12: the authors must specify what are the different areas of LIW formation in the eastern basin and provide adequate references. I do not want to influence them but in our reference I gave above, the authors can find hypotheses about why LIW could have been found away from the northern Levantine subbasin (that we assume is the sole place where it is formed). Basically, we assume processes similar to those already demonstrated for the Algerian subbasin: LIW forms roughly south of Rhodes then spreads and circulates alongslope, in particular south of Crete where it can be entrained by Ierapetra and transported, eventually till Egypt; then, LIW will circulate south of the Peloponese where it can be entrained by Pelops in general toward the interior of the Ionian subbasin. - P.571, l.16: I do not understand the notion of “suspended”. Any water lies, away from the zones of dense water formation, at a level corresponding to its own density: it floats and circulates above (denser) water below and below (lighter) water above. - P.571, l.20: “sink up”  $\rightarrow$  down? - P.571, l.21: I am not sure that evaporation is larger in summer everywhere in the sea; in particular, dry continental air masses entrained over the sea by violent northerly winds in winter probably induce a non-negligible evaporation; seemingly, cooling can also be induced in summer due to the northerlies. - P.571, l.25: This is again a personal opinion that the authors might not share but I do not think that “the picture becomes even more complicated in the western basin”. Processes driving the functioning of each basin and the circulation in it are, according to me, relatively similar and there are, in both basins, eddies either wind-induced or “current-instability induced” that can entrain part of the alongslope veins towards the interior of the basin. - P. 572, l.14, fig. 1: Obvious errors on theta and/or S should be corrected and erroneous points on the theta-S diagram eliminated. - P. 574, section 3: Basic information about the MedArgo characteristics are given only in the last lines

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of the Conclusion. I think that they should be given just at the beginning of section 3, furthermore it is essential to know which kind of information these floats can provide us with. - P. 574, l. 6, fig. 2: Erroneous locations (over land; why are locations in fig. 2 not similar to those in fig. 1?) should be corrected. - P. 574, l. 6: I think that differentiating “three main groups of clusters” is not obvious and not essential for the remainder. - P. 574, l. 10: I think it is not correct to deal with clusters located “west of islands” instead of specifying “in the interior of the Provençal and Algerian subbasins” since the specificity of these clusters is mainly to be away from the slope. - P. 575, l. 9: names that need to be specified and are not well known need to be mentioned in a figure - P. 575, l. 10: I did not understand what is “the thermohaline inversion often observed”. Please detail and comment. - P. 575, l. 15: I understand and agree that circulation and mixing are intensified in narrow passages, which should lead to homogenisation. But I am not sure that this will lead to the complete destruction of lenses, which is not what the authors consider for the distribution of LIW in the western basin, i.e. after LIW crossed the channel of Sicily. The authors must be careful in describing the various processes concerning homogeneity and properly describe their feeling. - P. 575, l. 17-18: one understands that the process invoked is double diffusion but this is not explicitly said; in addition, it is not explained why this process would lead to heterogeneities in the horizontal. This needs to be explained more carefully. - P. 576, l. 2: my own understanding of “patchiness” is “juxtaposition side by side” of classes. My own analysis of fig. 5 is “superposition” of classes that are “partly crossing each others”, which is normal since the analysis is spread over a relatively large time interval and, especially in the basin interior (i.e. away from the continental slopes), characteristics are dependent on mesoscale phenomena that are moving. Concerning fig. 5, let me emphasise that sub-cluster 1.3 is clearly considering deep features that are far from being related to LIW (hence demonstrating that the 29.0-29.1 criterion is not a good one). - P. 576, l. 6-7: I think that the overall description of the “background transformation” could be improved. I personally think about LIW (and other water masses as well) continuously changing roughly anywhere, due to continuous mixing under normal conditions, and

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encountering, in a discontinuously manner, changes here and there: in the narrow passages (always), when de-structured by mesoscale eddies (from time to time and during any season), when involved in the process of dense water formation (in some specific places during winter only). - P.577, l. 8: I do not think that profiling floats suffer some “drag” (horizontally in the authors’ mind). They are entrained horizontally by the water in which they are ascending or descending (and do not “feel” these horizontal motions; they only feel as a drag their vertical displacement).

To answer some of the aspects specifically mentioned by the Editor: 1) The paper addresses relevant scientific questions within the scope of OS 2) It presents novel ideas, tools and data 3) Substantial conclusions are reached 4) The scientific methods and assumptions are valid and can be more clearly outlined 5) The results are sufficient to support the interpretations and conclusions 6) The description of the calculations is almost sufficient, even though I recommend performing other calculations 7) The authors give proper credit to related work and clearly indicate their own new/original contribution 8) The title should clearly reflect the contents of the paper provided the criterion be changed 9) The abstract provides a concise and complete summary 10) The overall presentation is well structured and clear 11) The language must be markedly improved (even though I am unable to help the authors being not fluent enough in English) 12) Mathematical formulae, symbols, abbreviations, and units are correctly defined and used (I just recommend to check whether the use of psu is correct or not) 13) I think that fig. 2 and 5 mainly should be improved (see my comments above) 14) The number and quality of references are appropriate, although more recent ones could replace some.

I hope the authors will accept re-handling their work, hence providing a very nice paper.

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