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Reply on RC1

Katia Mallil et al.

Author comment on "The Levantine Intermediate Water in the western Mediterranean and its interactions with the Algerian Gyres: insights from 60 years of observation" by Katia Mallil et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-120-AC1, 2022

First of all, we would like to thank you very much for all your interesting remarks that helped us improve the manuscript, especially regarding the statistical significance of the trends.

Please find below the responses to each of the comments.

Best regards.

Comment on os-2021-120

Anonymous Referee #1

Referee comment on "The Levantine Intermediate Water in the western Mediterranean and its interactions with the Algerian Gyres: insights from 60 years of observation" by Katia Mallil et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-120-RC1, 2022

Review of "**The Levantine Intermediate Water in the western Mediterranean and its interactions with the Algerian Gyres: insights from 60 years of observation** "

By Katia Mallil, P. Testor, A. Bosse, F. Margirier, L. Houpert, H. Le Goff, L. Mortier, and F. Louanchi.

The authors describe the Levantine Intermediate Water (LIW) in the western Mediterranean using in-situ data gathered over more than 50 years with a particular interest given to the Algerian Gyres region. The mean and variability of the LIW temperature and salinity are assessed. Regarding the variability of the LIW, the data suggests a significant cooling of the LIW in the late 70s early 80s and a rapid warming after 2012. Salinity trends are also described here (although in-situ are sparse). I found this work interesting, well-organized, and well-written. The amount of available in-situ data itself deserves publication.

While the analyses/trends would deserve more statistical significance (in particular with respect to the number of sin-situ data), I would recommend this article for publication after a careful revision of the following comments.

I wish the manuscript had included the origin and physical mechanisms of the cooling and warming. I hope it could be part of an additional study combining the large in-situ dataset presented here and some non-assimilated ocean model experiments.

Comments:

Line 10: This does not seem to be a statistically significant trend for the period 1960-2017. In fact, you say warming for the period 1960-2017, but if you choose the period 1965-2012, it looks like a cooling trend. See below for more comments.

Thank you for bringing up this point, indeed, the obtained time series are very irregular and the value of the obtained trends depend highly on the chosen period, especially for temperature, however throughout our study, the evolution tended toward warming in the long term. We tried computing the trends for the period 1965-2012, these turned out to be on average slightly positive +0.0007 ±0.0017°C/year and +0.0013 ±0.0005/year. We agree that the significance of the trend over such a long period of time is small. While the salinity trend is clear and monotonous, the variability of the temperature is also impacted by decadal signals. We slightly modify our statement about the temperature trend. "While there is no significant temperature trend over the full study period, the more recent period 1988-2017 has been characterized by a clear temperature increase of 0.005±0.003°C/year, accelerating to a rate of 0.05±0.04°C/year between 2013 and 2017."

Line 15: What does "active" mean in "active thermohaline circulation" ?

We qualified the thermohaline circulation as "active", because of the variety of processes involved, including deep convection. Also, the residency time of waters in the Mediterranean is estimated to be of about 100 years, which is an order of magnitude smaller than the residency time associated with global thermohaline circulation. We suggest to use instead the term "dynamic" also refer recent changes in deep convection that were well documented (Margirier et al 2020, Somot et al 2018).

Line 24-25: Could you move these lines a couple of lines after? This paragraph is well written but you mention successively the MOW, then the LIW, then the MOW, and back to the LIW.

Thank you for pointing this out, it has been reorganised following recommendations of #Refree 2. After mentioning MOW, we proceeded to introduce LIW as an important constituent of it, describing its origin and properties. After that we indicated that the southwestern part of the Mediterranean was less documented than the other parts of the basin especially regarding LIW.

Line 29: “Levantine Intermediate Water “ -> LIW

This have been sorted, thank you.

Line 37: If it does not make Figure 1 too busy and unreadable, could you show these Algerian gyres in Figure 1? Or perhaps having a new figure that shows the mean circulation of the region, including the Algerian gyres, Sardinia Eddies, bathymetry …. In particular for readers (like me) who are not familiar with this region.

Thank you for the suggestion, we will add a simple schematic figure to indicate the circulation features.

Line 50: furtheR

Sorted, thank you.

Line 53: What are the findings of “Testor et al. (2005a)” ?

Testor et al. (2005a) have confirmed, using the numerical model, that the Sardinian eddies present a core of LIW at intermediate depths with characteristics close to those found in the Sardinian LIW demonstrating their transport efficiency. A sentence was added to clarify that.

Line 60: 0.005°C/year. Can it be called “warming”? I’m guessing that number is not statistically significant. Same for the salinity “trend”. If the references listed here found these values to be significant, please add the information to the text.

Indeed the trends are small and based on few observations of a dynamical system. But still, these trends are however significant. The deep water properties were thought for a long time to have constant temperature and salinity, before oceanographers discovered that deep convection was renewing them and abruptly modify their TS properties. In that context, bethoux et al. (1990) have shown an increase of T of 0.12 °C between 1959 to 1989 in the deep water (>2000) using historical observations observation. Then from volume and heat conservation calculations, an increasing trend of 0.005 °C/year in the intermediate layer have been deduced. Bethoux et al. (1996) have compared in situ T and S measurements of intermediate layer from historical data covering the 1950-1973 period (Nyffeler et al. 1980) and measurements acquired in 1991 and 1992, and have shown an increase of temperature of 0.0068 °C/ year and an increase in salinity of 0.0018 / year. Sparnocchia et al. (1994) have also reported a significant increase in LIW core temperature in almost all the areas of the Western Mediterranean, based on data from 1950 to 1987 (eg: 0.0091°C/ year in Ligurian Sea and 0.0065°C/year in Sicily channel).

These trends are synthesized in table 1 from Vargas et al. (2009)

Line 70-71: Sect. -> Section

In fact, it is in the OS instructions for authors that sections should be referred to by Sect.

Figure 1 caption: "total number of data" -> "total number of profiles"

Sorted, thank you.

L100: any references for the "WMDW having small natural variability within a year (0.01)". What is the percentage of data going through the correction of salinity offset?

After checking, it turns out that the correction of salinity offset algorithm failed to correct the detected outlying profiles in the absence of proper reference . We will try to adapt the parameters, otherwise this part will be discarded. Thank you for pointing this out. The natural variability of ~(0.01) in salinity over a year was documented by Houpert et al (2016) during deep convection year, which can be considered as an upper bound, as years of weak convection will not affect the properties of the deep waters very much.

L119: Reference for choosing the range "28.95-29.115 kg.m^{-3}"?

We did not use literature to determine this range, instead, we used reference data such as quality-checked cruise and glider data to set a broad range encompassing the layer of LIW.

L136: Do you mean "western" Algerian Gyre?

Yes, thank you. The mistake have ben corrected.

L133-145: Except for EAIg and WAIg, how do you choose the other regions ? Are they related to bathymetry?

We chose the regions for our study to be relevant in respect to the circulation features. As suggested above and by reviewer 2, we will add a figure with the general circulation and the bathymetry that will help to understand our choices.

Line 154: Can you indicate the LIW in Figure 3?

I am not sure I understand your request here, the sentence in line 154 have

been rephrased to clarify what I meant, " but one can identify a marked patch of LIW at about 400 km during each cruise (Fig. 3) >> but one can identify a patch of higher temperature and salinity within the LIW layer, starting at about 400 km from point A, during each cruise (Fig. 3). A ellipse will be added in Fig. 3 around the marked patch to help the reader. Please let me know if it is not what you meant, Thank you.

Line 155-161: It would be clearer to plot the 2010-2008 and 2014-2008 differences (with a red-white-blue colorbar) to visualize the "trends".

Great suggestion, a third column in figure 3 or an appendix figure would be great to visualise the evolution. Thank you.

Line 169: You might want to overlap SSH contours from altimetry to clearly see the anticyclone in Figure 5

indeed, thank you for the suggestion. We will have a look at the CMEMS SSH product to add this information if it shows well the observed pattern.

Line 172-174: Does the LIW exhibit seasonal variability? If so, how did you compute the climatological values?

LIW does not exhibit any seasonal variations, once formed in the Levantine basin, it is not in contact with the air, except in very specific areas of deep convection (NW Mediterranean Sea, Aegean, Adriatic, ...). In the study region, the LIW characteristics are mainly modified by mixing with the adjacent waters, and vertically by the interior turbulence.

Figure 6: That would be interesting (not for this manuscript, just a thought) to add to Figure 6 the thickness of the LIW.

In our study, we focused on detecting the core of the LIW, but for sure this would be an interesting perspective to look at the thickness of the LIW. Thank you for the idea.

Line 180-182: How do you compute the climatology. Did you just compute the mean of all your data? If so, and as you mentioned, the mean value will strongly depend on the number of data per year. Why not do the mean of monthly mean data? (Or if there is little climatology do the mean of yearly mean data).

Yes, the climatology is a mean of all the data. Indeed, doing a mean of yearly means will reduce significantly the bias, we will prepare new climatology following your suggestion, thank you.

Line 188: Are the cyclonic Algerian Gyres some types of mode water eddies ?

No the Algerian Gyres are large scale cyclonic circulation features, mainly barotropic with a velocities of about 5cm/s within which the Anticyclonic Algerian Eddies live.

Line 190: from table 1, the "overall increase" is true only for Albo and AlgC

You are right, the trends of the whole study period are not very clear and significant. We thus rephrased: "The evolution of potential temperature of the LIW as seen in Fig. 8 is showing a slight overall increase over the 1960-2017 period, however not monotonous and significant only for Albo and AlgC regions (Table 1)."

Line 193 and 202: Does your analysis start from 1969 or from 1960?

For the trend analysis, almost all the 60s data were discarded because of the scarcity of the data. However, in the climatology analysis, all data have been taken into account.

Line 194: Θ -> Θ & S

Sorted, thank you.

Line 195: By "error" you mean standard deviation?

We meant, confidence interval, the sentence is redundant, it has been rephrased. Thank you for pointing it out.

Table 1 and 2: Rather than using grey values for R^2<0.5, you should use grey values for **non statistical** significant values.

You are right, the R^2 values indicate how much of the variance of the data points is explained by the linear regression. It is thus fair to exclude data of low R^2 since the linear trend is not explaining an important signal.

To assess the significance of the trends, p-values were computed. It turns out that none of the grey data (R^2<0.5) are statistically significant. In addition to that, some trends with R^2>0.5 were also not statistically significant, probably because of the low number of data points used for the regression. In the revision of the manuscript, we will thus only consider trends with p-values inferior to the acceptable 0.05 threshold. Thank for this suggestion.

Line 197: "… different phases best fitted …". I am not sure I understand. Why do you change the time period in each phase? For e.g. Why do you choose 2009-2017 for SSar? This is twice as long as the 2014-2017 period.

Indeed, the recent increase in temperature (already documented in the Sicily channel, see Shroeder et al 2017) arrived from the Easter Mediterranean entering the study area by the Ssar region. This is why we observe an accelerated increase in temperature already from 2009 in Ssar box. This is the motivation for having a different time period between the regions. We will explain this physical reason more precisely in the text.

General comment: The number of points used in this analysis is very small, in particular for salinity for the period 1960-2010 or for temperature for the periods 1960-1970 and 2000-2010. In most years between 1960-2010 you have less than 5 data points per year for the 8 regions, meaning that most of the regions do not have data and the rest have 2-5 data points. This would be even more problematic if there is a seasonal cycle. How do you take this lack of in-situ data into consideration in your calculations?

First, we would like to clarify that the histogram that is represented in figure 8 represents the mean number of data used to compute the annual mean in each polygon, so there is on average 5 data points in each region. It is true that this number is not high, but over the whole period the number of data point become significant to make some statistics and compute some trends.

We have already justified why the seasonal cycle should not be an issue.

Line 202-203: Only half of the region shows that "increase". Also, Table 2 shows some misleading values. For e.g, the salinity increase for SaMi for the period 1967-1977 (R^2=0.8). It seems however that there is no data between 1970-1997 from Figure 8b.

You are right, we will check this. With the computation of the number of points per sample in the tables and p-values, we should be able to track those mistake. This is link to the comment about significance of the trends, only p-values could tell us how significant the trends are. R^2 can be wrongly high when the sample is small…

Line 207: Only 3 out of the 8 regions show a "significant" freshening. Yet, the conclusion is that there is a freshening.

Indeed, we will reassess our conclusions for this period, the signal is indeed not that clear.

Line 209: “propagates”. How do you arrive at that conclusion? You look at yearly averaged, does it take several years for the signal to propagate from SSar to the other regions?

If we anticipate the results presented in the paper (fig 9), it takes indeed 1-3 years for the signal to propagate from the Ssar to the other region. If it was advected by horizontal currents, the signal would spread faster, but the cross-shelf exchange is mainly driven by horizontal diffusion by mesoscale eddies and thus takes more time than the typical advection speed of currents.

Line 211: If we assume that $R2<0.5$ is not significant (I recommend using grey values for statistical significance), then only 2 out of the 8 regions have significant temperature trends (one warming and one colling). Yet, the conclusion here is “tending towards increase for most areas (6 regions over 8)”

Thanks for pointing this out, we will re-assess the trends once more information will be provided in the table.

Line 215: “WAlg between 2013 and 2014 “. This is only a 2 year long period (i.e. 2 points).

You are right, this is not significant and we will remove it from the text.

Figure 8: This figure is great and clean. The separation between bars (and points) which represent each year is however not consistent. It would be also easier to read if the vertical dotted lines also correspond to 1 year.

This has also been pointed out by reviewer2, it will be fixed. Thank you.

Line 225: “monthly averaged data”. I felt already that there were very few data points for the yearly averaged time series (bars on Figure 8a). This section now uses monthly averaged data. Do you have enough data for that?

We will carefully check and document the number of data points presents in the regions considered.

Line 226-229: Need to be rephrased.

This has been rephrased: “In Fig. 9, the cooling signal across the Algerian basin

is tracked in time. The map shows in solid gray arrows the along-slope circulation, as shown in Millot and Taupier-Letage (2005b), the transparent red polygons with the numbers showing the time in months needed for the signal to travel from south Sardinia (SSar polygon) to the other areas in the Algerian basin."

Section 3.4: This is an interesting section. Are the "advection times" observed in other studies (could be during different periods of time). Can the 15months lag between MAIg and WAIg be explained physically? Is the surface circulation similar to the one at the LIW depth?

The circulation at LIW depth is clearly less energetic than at the surface, especially in the Algerian Basin where lots of intense Algerian Eddies have surface intensified velocities of ~(0.5m/s). The velocities in the LIW layer is ~(0.01-0.1m/s). The two regions are separated by approximately 200km, which corresponds to a propagation speed of 5 mm/s. There is no direct route to transport the LIW signal by the mean circulation and the eddy-induced horizontal transport act at an effective speed much smaller than the actual observed current velocities at the LIW depth.

Line 266: "have" -> "has".

Sorted, thank you.

Line 266: Is "Millot (1999)" support the "slow accumulation over time of LIW in the interior of the Algerian basin"? If so, move the reference up in the sentence.

Millot (1999) does not explicitly talk about slow accumulation over time, it is only said that averaging without precautions may mislead into thinking there is a direct route from southwest of Sardinia towards the Algerian basin. This will be checked carefully and rephrased. Thank you for pointing this out.

Line 283: Add the period of time of the cooling discussed here.

The period 1978-1986 have been added to the texte, thank you.

Line 292: "the" -> "The"

Sorted, thank you.

Line 292-295: What are the propagation speed of those eddies. Is it consistent with the lag correlations in Figure 9?.

The zonal velocities of large anticyclonic eddies detected and tracked from

altimetry maps for the 20 year as estimated by Escudier et al. (2016) are of about 3 to 6 cm/s, (or ~2.5 to 5 km/day). if we use this number to estimate the propagation time of the signal, we obtain a transit time of ~1.5 to 3 months to cross 2° of latitude, which is consistent with the result we obtained for the SaMi region and the MAlg one. Testor et al. (2005a) have estimated an average translation velocity of the Sardinian Eddies to be of ~ 2 to 3 cm/s (or ~ 1.7 to 2.5 km/day) which gives us, 3 to 4 months to cross 2° of latitude, this is slightly larger than the result we obtained for SaMi and MAlg, but the order of magnitude is consistent. The interior of the Algerian gyres however, present much larger transit times, and that is because of the wiggly motion that most of the eddies have following eddy-eddy interaction and the cyclonic barotropic circulation close to the gyres centers.

Line 296: Again, is the 47 lag realistic from the circulation of the region?

If we use the velocities of the LIW in the Provençal basin (5.7 to 9.4 cm/s) obtained from Margirier et al. (2020), to compute a transit time of a signal traveling from SSar to SIbi (~2450 km), we would obtain ~10 to 20 months, which is smaller than the 47 months obtained in our analysis, but the interesting information here is obtained when comparing the 47 months of SIbi with the 29 months in Albo that could reveal that the Eddy transport helps the efficiency of the intermediate water mass transport from SSar to Albo across the Algerian basin, and/ or the differential effect of this eddy transport. North of the eastern Algerian gyre, the transport is very effective, hence the 2 and 4 months, but then, once the eddies start swirling around the gyres, they progress slowly to transport the signal in other parts of the basin.

Line 354: Why is it alarming? Impact of biology, ecosystem, …?

We have expended this last sentence: "A closer monitoring of water mass properties need to be sustained. It is crucial to maintain and reinforce existing surveillance systems as they can assess the direct impacts of climate change in the Mediterranean hot-spot. In the future, we can expect important modification of the water masses properties with major consequences: increase of temperature, stratification, collapse of deep convection in the NW Mediterranean Sea (Parras-Berrocal, et al 2022), thus affecting its profound functioning and the rich but fragile ecosystems that is hosts. It is reported in Lacoue-Labarthe et al. (2016) that an increased warming is likely to result in mass mortality of seagrass Posidonia oceanica (which is a very important habitat in the Mediterranean, and constitutes an important carbon sink), invertebrates, sponges and corrals ..etc. Invasive warm water species of algae, invertebrates and fish are increasing their geographical ranges. In addition to that, the proliferation of pathogens are expected, increasing the spreading of diseases."

Acknowledgments: Do not forget to add the funding agencies, if applicable.

We entered the information about funding separately in a dedicated space during submission. I suppose it will be included in the final version even though it is not visible in the pre-print