The author proposed a revised version of the manuscript that addresses the comments raised by both reviewers. The manuscript is now more balanced and clearer, and its overall quality increased significantly. As mentioned in my first review, the manuscript presents a precious dataset together with an analysis of time series of temperature and currents from deep observatories across the mediterranean Sea. The contribution of the article is useful, and I would therefore recommend it for publication after a few minor changes and clarification that I described below.

For a better clarity I will follow the numeration used by the authors to answer the first round of my review. My new comments will be written in green, while the author's response will be in black.

Main comments (of the first round of reviews)

1. *We agree that the use of the term "cross-correlation techniques" could be misleading, then, following the Reviewer's suggestion, we have replaced "cross-correlation techniques" with "regression analysis" to more accurately describe the methodology used. Regarding the interpretation of $R^2 = 0.28$, In the revised manuscript, this has been changed to "moderate agreement" better explaining the context of the measurement setup. These clarifications have been incorporated into the revised manuscript, as follows:*

   "For instance, the horizontal velocity component (U) measured by the punctual current meter and by the ADCP deployed at GEOSTAR-SN3 during the 2003–2004 mission showed moderate agreement ($R^2 = 0.28$, Figure 3). This result should be interpreted in the light of the differences in functioning, and positioning: the ADCP samples the open water column, whereas the punctual current meter operates within the observatory frame. Also, the current-meter record, acquired at 2 Hz, was smoothed using a 1-hour moving average and sampled at hourly intervals to allow comparison with the ADCP data. "

   Accordingly, L232 has also been revised to "moderate consistency".

   For what concerns the other component, see attached Fig.3_V: [attached to the author's answer report]

The author modified the manuscript in a suitable way. I thank the author for providing Fig.3_V in their answers.

2. *On the figure 4, I was wondering if the result is the final one or just after one step of QC because a large majority of "Interesting/Suspicious Data" is present. I would have also added a horizontal space between bars when moving to another site for readability (bigger horizontal space between bar 5 and bar 6, between bar 11 and bar 12, and between bar 16 and bar 17).*

The author modified Figure 4, which is now more readable and consistent with what a reader would have considered as “Good data”.

3. *We agree that the Rate of Change (ROC) test and a classical outlier detection based on mean ± 3 standard deviations are conceptually different and should not be described as equivalent. The ROC test evaluates the difference between consecutive observations, requiring that the rate of change between sample* $n-1$ *and* n *remains below a threshold defined as a multiple of the standard deviation (e.g.,* $N\_DEV \times SD$*). Importantly, this standard deviation is computed over a moving time window to account the natural variability. Both the number of standard deviations (N_DEV) and the time window (TIM_DEV) are operator-defined parameters. In this framework, the standard deviation is used to characterize the local variability of the time series, allowing the threshold to adapt to changing environmental conditions. Therefore, describing this approach as equivalent to a mean ± 3 SD method would be misleading, since the latter identifies outliers based on their deviation from a central value and does not account for temporal sequencing. We have therefore revised (in blue) the manuscript as follow:*

   Rate of change *– This test inspects the time series for changes between consecutive observations that exceed a defined threshold. The observed quantities can change substantially over short periods in some locations, hindering the value of this test, so the thresholds have been chosen carefully and tailored based on the sensor’s operational limits and location. The threshold is defined using the local variability of the signal (e.g., a multiple of the standard deviation computed over a moving time window). Unlike classical outlier detection methods based on deviation from a mean value, this test evaluates the temporal consistency of the measurements and flags, rather than removes, suspect data. In the case of dissolved oxygen measurements, because of its dynamic nature, this test does not involve outliers’ removal but flagging.*

The author modified the manuscript in a suitable way.

4. *Thanks for this comments that one more time evidencing a possible misleading. Our intention was not to imply a strict equivalence between the sensor tilt test and a rate-of-change test applied along the vertical. In the QUARTOD framework, the tilt test is specifically designed to detect changes in sensor orientation that may affect current measurements. In our case, the sensors are mounted on a fixed and stable seafloor platform, where significant tilt variations are not expected under normal operating conditions. Therefore, the relevance of a dedicated tilt test is reduced. Instead, we proposed a complementary approach based on inspecting the vertical consistency of the velocity profiles (i.e., gradients along depth). While this does not directly measure sensor orientation, abrupt or non-physical vertical variations can indicate potential issues, including misalignment, platform disturbance, or measurement artifacts. We acknowledge that this approach is not equivalent to a tilt detection test and should not be interpreted as a replacement in a general sense. We have revised (in blued) the manuscript to clarify how this method is used:*

   Sensor Tilt *(for current meter and ADCP) - Current sensors must be aligned within an expected range of tilt angles to properly measure horizontal and vertical currents. For*

fixed mounted sensors, as in the case of seafloor observatories, significant variations in tilt are not expected, and the applicability of this test is therefore limited. In this context, we examined the vertical consistency of ADCP velocity profiles (i.e., gradients along depth), which can help identify non-physical variations potentially associated with instrumental issues or platform disturbances, although this approach does not represent a direct substitute for tilt measurements.

The author modified the manuscript in a suitable way.

5. *The sentence referred to a previous version of the manuscript that included a table reporting the operating limits of the sensors, which was subsequently removed. As a result, this sentence is meaningless and has simply been removed.*

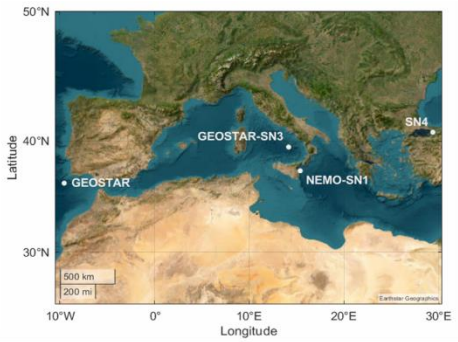
Ok.

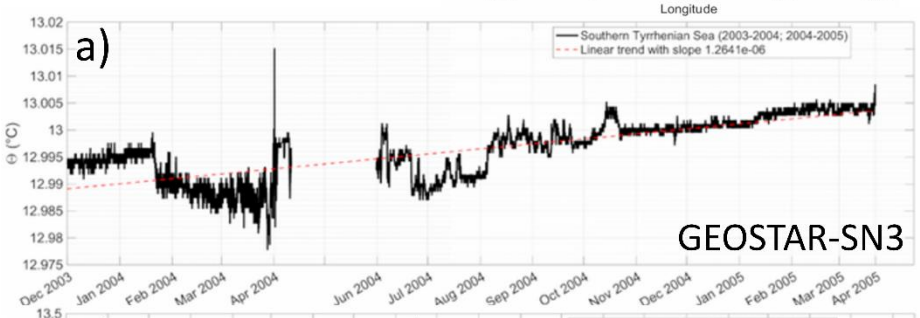
6. *The hodographs were generated using the JLAB hodograph routine, part of a toolbox specifically developed for oceanographic applications, which plots the eastward velocity component against the northward component, i.e., a Cartesian U-V hodograph. Figure 7 has been improved following reviewer suggestion.*

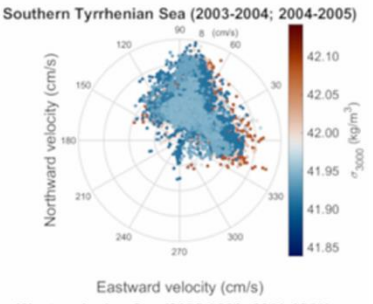
The author responded clearly to my comments concerning convention used for the hodograph. The unity of x and y axis were changed to m/s.

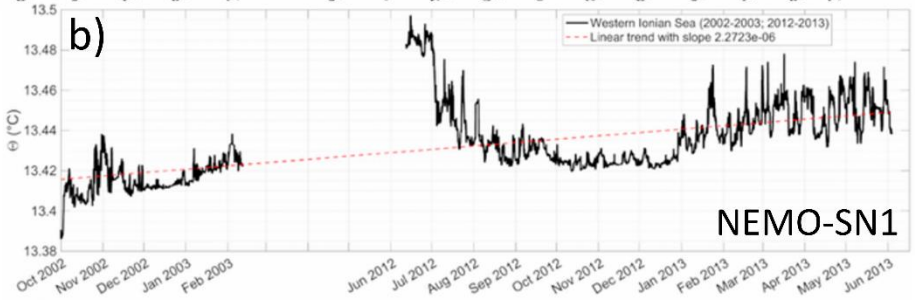
Nevertheless, despite the improvements made to Figure 7, it is still hardly readable in a printed version of the manuscript. The legend of the different time series are small, as well as tick labels, and colorbar label. I understand the difficulty that the author is facing to present two time series and a map in the horizontal. I propose an alternative on the next page for the layout which would make the different elements easier to read.

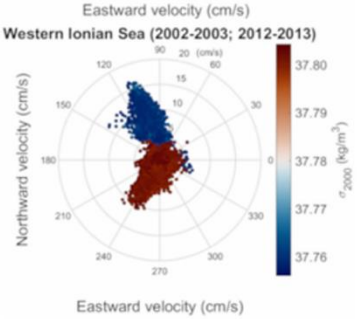
I also detected that σ_{2000}, σ_0 and σ_{3000} are used in this figure. I would recommend the author to change *"σ_{2000}, calculated with reference pressure of 2000 dbar"* (L.425) to *"σ_0, σ_{2000}, and σ_{3000}, calculated with reference pressure of 0, 2000, and 3000 dbar respectively"*.

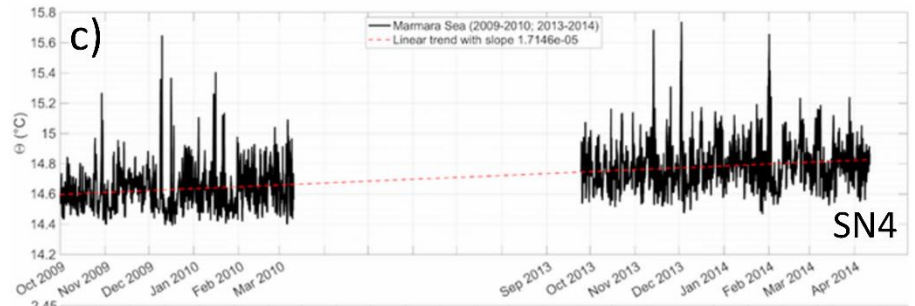

50°N
40°N
30°N
Latitude
10°W
0°
10°E
20°E
30°E
Longitude
GEOSTAR-SN3
SN4
GEOSTAR
NEMO-SN1
500 km
200 mi

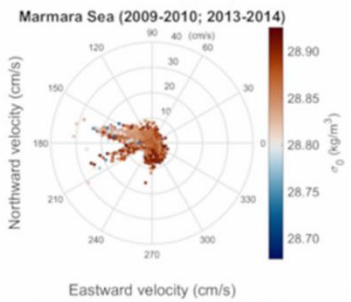

a)
Southern Tyrrhenian Sea (2003-2004; 2004-2005)
Linear trend with slope 1.2641e-06
Θ (°C)
GEOSTAR-SN3

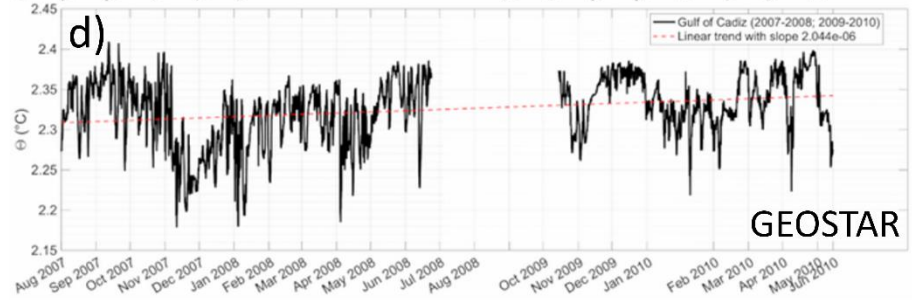

Southern Tyrrhenian Sea (2003-2004; 2004-2005)
Northward velocity (cm/s)
Eastward velocity (cm/s)
σ3000 (kg/m3)

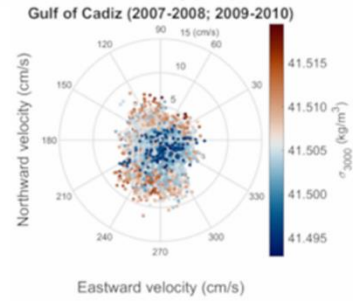

b)
Western Ionian Sea (2002-2003; 2012-2013)
Linear trend with slope 2.2723e-06
Θ (°C)
NEMO-SN1


Western Ionian Sea (2002-2003; 2012-2013)
Northward velocity (cm/s)
Eastward velocity (cm/s)
σ2000 (kg/m3)


c)
Marmara Sea (2009-2010; 2013-2014)
Linear trend with slope 1.7146e-05
Θ (°C)
SN4


Marmara Sea (2009-2010; 2013-2014)
Northward velocity (cm/s)
Eastward velocity (cm/s)
σ0 (kg/m3)


d)
Gulf of Cadiz (2007-2008; 2009-2010)
Linear trend with slope 2.044e-06
Θ (°C)
GEOSTAR


Gulf of Cadiz (2007-2008; 2009-2010)
Northward velocity (cm/s)
Eastward velocity (cm/s)
σ3000 (kg/m3)

7., 8., 9. *We thank the reviewer for this comment, and we understand the caution suggested that is needed when interpreting trends derived from relatively short time series. Our intention was not to claim a robust detection of long-term climate change, but rather to highlight that the observed positive temperature trends across all monitored deep sites are consistent in sign and in agreement with the broader warming patterns reported in the literature, including IPCC (2013). We acknowledge that, for several sites, the available time series are relatively shorter and therefore may reflect local or decadal variability, as discussed later in the text. However, the coherence of the signal across multiple independent deep-sea observatories, combined with the known sensitivity of the Mediterranean basin as a climate hotspot, supports the relevance of these observations. We have revised (in blue) the manuscript to better clarify these limitations and to temper the interpretation accordingly.*

While the available time series are relatively short for each site and might therefore reflect regional or decadal variability, all temperature trends measured between 2001 and 2013 at all deep layers are coherently positive and broadly consistent with the warming patterns reported in the literature (IPCC, 2013).

I appreciated to read the revised version of the manuscript concerning the point 7, and I think the text is now more balanced.

For the comments 8 and 9, the manuscript still appears to present an ambiguity.

In L.444 (previously L.404 – comment 8) I am still concerned with what the author wrote *"providing a coherent and reliable estimate of a decadal-scale warming process"*. This interpretation does not appear balanced enough: the trend is derived with a time series constituted of 5 months between 2002 and 2003 (October 2002 to February 2003) and one year from June 2012 and June 2013 which question its reliability for the detection of a decadal-scale warming process. The manuscript mention changes in density and current direction, as well as changes in deep water masses properties. I encourage the author to clarify whether the dynamic in the at NEMO-SN1 is due to a decadal warming scale process, a change in the deep-water mass, or a combination of both.

Before answering, please read the comment for the answer 10.

10. *We thank the reviewer for this important comment. It is important to clarify that this study does not aim to provide a comprehensive assessment of long-term climate change. Rather, its objective is to highlight the presence of a consistently positive temperature signal in the deep layer across multiple sites, and to emphasize its potential relevance. In this case, we distinguish between processes acting at different temporal scales. The changes highlighted by the hodograph, and the shift in current direction, reflect a reorganization of deep circulation, likely associated with BIOS-related variability. At the same time, the observed temperature increase represents a local long-term tendency, which, although derived from relatively short time series, is coherent across sites. These processes may be interconnected: modifications of deep thermohaline properties, can influence the density structure of water masses and potentially contribute to circulation changes at larger spatial and temporal scales.*

I thank the author for this clarification. I understand the intention of the author to distinguish the long-term trend observed across the different sites and the Ionian Sea BiOS-related variability. The author's answer *"These processes may be interconnected: modifications of deep thermohaline properties, can influence the density structure of water masses and potentially contribute to circulation changes at larger spatial and temporal scales"* is appropriate. But the text is less balanced (*"reliable", "coherent", "real change"*). I would suggest that the author use his response to refine my comments for the answer 8 and 9, explicitly distinguishing decadal variability in the Ionian basin and long-term temperature trend.

11. We thank the reviewer for this detailed and constructive comment. We agree that some statements in this section were overstated and have revised the manuscript to provide a more balanced description of the SSA/OI approach and its limitations. We acknowledge that the performance of SSA-based gap filling depends on the data structure and gap distribution, as highlighted by Kondrashov and Ghil (2006). In our case, the relatively short time series and the presence of a long gap do not allow for a rigorous assessment of reconstruction accuracy. The SSA/OI method was therefore not intended to provide a quantitatively validated reconstruction, but rather to obtain a continuous time series suitable for estimating large-scale, low-frequency variability and associated trends. This clarification has been added to the text. Regarding the statement that SSA "does not require a priori assumptions," we agree that this was imprecise. While SSA is non-parametric in the sense that it does not assume a predefined model, it does require methodological choices, such as the selection of the embedding dimension and the number of modes retained. We have revised the text accordingly. Concerning the selection of the six dominant modes, this choice was based on the fraction of explained variance, retaining the leading modes that capture the majority of the signal while excluding higher-order components dominated by noise. We now have explicitly stated this criterion in the manuscript and provided a reference to similar approaches (e.g., Fig. 8a in Marullo et al., 2011 https://doi.org/10.1175/2011JCLI3884.1) to support this choice.

I appreciated the modification of the manuscript by the author which is now more balanced. I would still suggest the author to also introduce this balance in the conclusion as I wrote in my earlier comment. In *"the reconstruction of longer and more continuous records increase the resolution and robustness of spectral estimates…"* the "robustness" may not guarantee by reconstruction especially with SSA/OI and in *"the application of advanced analytical methods […] minimized spectral leakage and enhances the reliability of frequency domain diagnostics",* the article did not show a leakage reduction quantitatively, neither a quantitative validation for the *"reliability"*.

Secondary comments

12. I thank the author for the modification of the manuscript. He may have missed on L.109 where sensor efficiency is still referred as storage efficiency.
13. Ok.
14. I thank the author for the modification of the manuscript. He may have missed on Fig.3 caption where it is still referred as "horizontal (U) velocity"
15. Ok

16. Ok
17. Ok
18. The author may have missed to change the manuscript, as he mentioned “done” in the answer. I kindly ask the author to check it.
19. Could the author tell me where he added the reference for Table 3?
20. The author answered that “The reference to the models has been removed” but it is still present in the revised manuscript I downloaded
21. Ok
22. Ok
23. I thank the reviewer for the change, which makes the sentence clearer
24. Ok
25. No change appears in the revised manuscript for the Fig.5 while the author wrote “improved” as an answer.
26. to 32. Ok

Other small comments (editorial corrections, rephrasing, missing reference in the list)

33. The author decided to keep the manuscript in its original version.
34. to 37 Ok
38. To 42. The author decided to keep the manuscript in its original version.
43. To 45. Ok
46. The author decided to keep the manuscript in its original version.
47. Ok
48. I would suggest the author to add a space missing between “Tyrrhenian” and “Sea” in the table 3.
49. Not changed, I would suggest the author to correct this mistake.
50. Not changed, I would suggest the author to correct this error.
51. Ok
52. 53. The author decided to keep the manuscript in its original version.
54. 55. Ok
56. 57. The author decided to keep the manuscript in its original version.
58. Ok
59. Reference not added. I would suggest the author to check the manuscript.