General Comments:

This paper provides the community with a highly significant data set for the Cretan Sea, and useful context for understanding how the values/vertical structures vary over the period of study, and in relation to previous studies, both in the Cretan Sea and in the surrounding seas. The data seem of high quality and are provided for the reader. Overall conclusions about the heating anomaly in one year, the trends in temperature and salinity over the 6 years, and the changes in deeper layers below the intermediate water are supported by data and well-described.

We thank the reviewer for the positive assessment of our work. We have carefully addressed all specific comments below (our responses in blue italics).

While all of this is excellent, the 3 overall conclusions do not include a convincing discussion of the statistical significance or representativeness of the measurements. As in nearly all observational studies, there was undersampling in both space and time which makes discussion of the implications of the gaps critical. Neither is there a discussion of processes or mechanisms that may provide insight into the details of how the events/trends happened, and how it may help future studies better design sampling strategies to capture relevant mechanisms.

I accept that all of this is challenging, if not impossible, to perform and to include in a single paper. However these points need to be raised and discussed at the very least, and the authors should be careful not to claim quantitative conclusions without proper caveats and error analysis.

We thank the reviewer for this important observation. We agree that the original manuscript did not include a sufficiently clear discussion of the statistical significance and representativeness of the measurements, and we have substantially strengthened this aspect in the revised version.

Regarding statistical significance, full ordinary least squares (OLS) trend diagnostics (slope, standard error, 95% confidence intervals, p-values, and R^2) have been evaluated for all trend estimates. In addition, sensitivity analyses—including the exclusion of winter 2022, restriction to deep missions, and seasonal subsampling—together with robust regression tests, have been performed to assess the stability of the results. These analyses are described in detail in our response to Reviewer 1 (Comment 6; see https://doi.org/10.5194/egusphere-2025-6542-AC1).

With respect to spatial representativeness, the glider transect spans the full east–west extent of the Cretan Sea (23.66–26.15°E), providing a quasi-synoptic view at ~4 km horizontal resolution. We acknowledge, however, that this sampling cannot fully resolve the two-dimensional variability of the basin. This limitation will be explicitly stated in the revised discussion, emphasizing that the derived trends reflect multi-year changes along the transect rather than basin-wide climatological rates.

Finally, the consistency between the glider-derived trends and those from the independent E1M3A mooring over the same period provides additional confidence in the robustness of the results.

Specific Comments: (individual scientific questions/issues)

The main conclusion seems to be that the anomalous heating event was detected and in line with other studies. The magnitude of the event was described well and compared to other studies, but no further insights were provided (such as representativeness spatially, role of and impact on mesoscale dynamics and structure). A second conclusion about the rates of change of temperature and salinity is qualitatively convincing, but not quantitatively robust (statistically, representativeness spatially) nor did it provide any insight as to details of how/where this came about exactly (spatial extent, exact timing, or episodes not captured by measurements, and no discussion about those). The third conclusion about the role of intermediate water formation on deeper layers was not well-explained in terms of possible processes or convincing in terms of the measurements that might support.

No other tools or methods (like model simulations) were used to support the arguments for any of the above.

Technical Corrections and instances of specific comments:

TMW used in abstract without definition

You are right; We have added in the abstract TMW's definition as "Transitional Mediterranean Water'.

Line 33: typo 'Theoharis'

Replaced by 'Theocharis'.

Line 48: it is not clear if EMT events occur in the Cretan Sea or outside, or both.

We thank the reviewer for pointing this out. The Eastern Mediterranean Transient (EMT) was an extreme thermohaline event that occurred in the late 1980s to early 1990s in the Eastern Mediterranean Sea. It was characterized by an abrupt shift in deep water formation from the Adriatic Sea to the Aegean Sea alternating the deep circulation in the region. During this period, intense production of very dense waters occurred in the Aegean, resulting in the formation of Cretan Deep Water (CDW) with exceptionally high density in the Cretan Sea. This dense water spread into the eastern Mediterranean outflowing from the Cretan Straights, displacing older deep waters of Adriatic origin and uplifting the entire water column by several hundred meters. The EMT significantly affected the Eastern Mediterranean basin by increasing temperature and salinity in deep layers, altering deep and intermediate water masses, and modifying large-scale circulation patterns. It also led to the formation of Cretan Intermediate Water (CIW), which propagated westward and partly replaced the Levantine Intermediate Water (LIW) in parts of the basin. The EMT event was created inside the Aegean Sea, forming very dense waters that accumulated in the Cretan basin before being exported towards the open eastern Mediterranean.

We will add a comprehensive description in the revised manuscript to help the reader in addition to the citations regarding the EMT event.

Line 60: thermohaline pump" mechanism ….are you implying this is in contrast to the BiOS? please explain.

We thank the reviewer for the comment. The 'thermohaline pump' mechanism has been proposed as the internal process that controls the competitive functioning of the Adriatic and Aegean Seas as dense water *sources. It constitutes an alternative to the BiOS interpretation of the observed quasi-decadal upper thermohaline circulation variability of the Eastern Mediterranean, as it considers the reversals of the of the northern Ionian upper circulation not related to the Northern Ionian Gyre (NIG) but to the advection convection pumping mechanism called the 'thermohaline pump', driven by alternating, anti-correlated dense water formation in each of the two dense water formation sources (i.e. the Adriatic and Aegean Seas). This theory considers NIG reversals as a result of alternating AW paths in the eastern Mediterranean.*

We will add the explanation in the revised manuscript.

Line 65: use of 'where' is confusing since just after that you say exported from. Clarify that exported from Cretan Sea."…Aegean Sea, where the export of …"

"Furthermore, Velaoras et al. (2015) linked the appearance of TMW in the Cretan Basin to water mass exchanges driven by DWF in the Aegean Sea. Here, the export of dense Aegean waters through the Cretan Straits is balanced by the inflow of TMW."

Line 83: dynamics are not really discussed in this paper. The current fields in 3D are not presented or discussed. One 2-D plot mentioned, with illegible vectors, and inadequate discussion.

We agree that the dynamic context could be strengthened. The ADT and geostrophic velocity fields (Fig. 6d) are included to provide qualitative mesoscale context for the representative transects, not as a quantitative dynamic analysis. A full 3D current field analysis is beyond the scope of the present study. We will move the ADT panels to an Appendix (as noted in our response to Reviewer 1, Comment 2) and strengthen the qualitative description of the mesoscale influence on each representative transect. We will also improve the legibility of the vectors. A dedicated quantitative mesoscale analysis is planned as follow-up work.

Line 101: Define "mission"; is it a single transect?

We thank the reviewer for pointing this out. Each glider mission consists of a continuous deployment lasting 30 - 45 days, during which the glider typically completes the full east-west transect two to three times. For the trend analysis, a single representative transect per mission is selected using specific time windows such that each mission contributes one layer-mean value to the trend calculation. The 14 missions thus correspond to 14 time points in the interannual analysis. This will be clarified in the Methods' section.

Line 105 and after: Details on glider specs, calibration of which sensors when, summary of QC methods and quantitative assessment of data quality (e.g. how much each flag was used, typical problems found and how dealt with)…lots missing here. Doesn't TEOS-10 depend on regional composition, requiring local salinity samples to be used?

The six-year dataset was collected using five Alseamar SeaExplorer gliders (SEA015, SEA031 rated to 700 m; SEA030, SEA048, SEA062 rated to 1000 m). All gliders were equipped with a Seabird Glider Payload CTD (GPCTD) paired with an SBE 43F dissolved oxygen sensor. The GPCTDs sampled at 1 Hz (SEA062, SEA031, SEA030) or 4 Hz (SEA048, SEA015); however, subsampled data at 30 s resolution were used in this study. Comparison between raw and subsampled datasets indicated that the latter is both suitable and more manageable for the present analysis, and it corresponds to the standard data distributed to aggregators and Global Data Assembly Centers (GDACs).

All GPCTDs underwent routine calibration and maintenance. Sensor performance was evaluated for each deployment and recovery through CTD casts following the glider yo dive (yo is a sequence of a dive (descending part) and the following climb to the surface (ascending part)). Intercomparisons were performed using a Seabird SBE19plus CTD, calibrated at the Hellenic Centre for Marine Research (HCMR), with differences remaining within manufacturer-specified accuracy limits.

Data were quality controlled following procedures adapted from Argo quality control protocols (Wong et al., 2025) and further described in (EGO Gliders Data Management Team, 2025b). Thermal lag effects were identified in individual profiles as salinity discrepancies (~0.05 psu) between downcasts and upcasts within the same yo, primarily in the upper ~100 m during summer missions. These effects do not affect the present analysis, which focuses on deeper layers.

Thermodynamic variables were derived following the TEOS-10 framework. While TEOS-10 does not require locally measured composition, it accounts for regional variability through Absolute Salinity corrections based on basin-scale anomaly fields.

A concise summary of these data handling and quality control procedures will be included in the revised manuscript.

Line 119: what type of interpolation was used? Vertical bin averages used or other filters? Smoothing used for plots?

We will clarify in the Methods' section the following: Each glider profile was linearly interpolated onto a uniform 1 m depth grid. No vertical smoothing or filtering was applied prior to interpolation. Horizontally, profiles were averaged into 4 km bins along the glider track using means of all profiles within each bin. Figures correspond to the gridded means without additional smoothing, unless explicitly stated.

Line 130: A more detailed chart of time coverage would be interesting (time series of number of profiles per day for the duration, and again with all months collapsed to see coverage per month). Or a table of dates and length of each mission. (instead of Fig. 3)

We thank the reviewer for this suggestion. We will add a supplementary table providing the date, duration, season, and maximum depth of each of the 14 missions, which gives more detailed time-coverage information than Fig. 3 alone.

Line 163: what signifies these 'continuous exchanges' exactly, from looking at the data?

In the context of our data, the "continuous exchanges" are inferred from the interannual variability of salinity in the subsurface and intermediate layers, which exhibit alternating periods of higher and lower values. These fluctuations are consistent with the recurrent presence of different water masses in the Cretan Sea, reflecting episodic inflows of saltier Levantine Intermediate Water from the east and relatively fresher Atlantic Water from the west.

We will revise the text to explicitly link this interpretation to the observed variability in the salinity profiles.

Line 178: The T-S diagrams are not convincing with regard to spatial gradients described.

You are right, by mistake we did not mention in the sentence the citation of the following diagrams where the spatial gradient in the surface waters is evident (in reality we are referring to fig. 6); The following sentence should replace the original one: 'Cretan Surface Waters (CSW) are typically influenced by LSW *and fresher modified AW/BSW inflows producing occasionally the evident spatial gradients in surface temperature and salinity as depicted in the autumn transect diagrams of fig. 6a and b1'*

Line 199: "deep layers of salinity transects.." which depths, salinity values, and which transects specifically? Quantitatively.

We will add the following sentence: ' The TMW mass typically appears in the summer salinity transect of 2019 in fig. 6a4 between 600 and 1000 m as a minimum with values [38.89 38.922].'

Line 200-205: this section gives a 1-D description of a 2-d image...why? No thoughts on spatial structures for temp/density? No thoughts on time variability within the mission (10 days) or any biasing by internal waves or other non 1-D process? The dynamic topography maps could be used to explain the horizontal structure seen (location of eddies in both plots pointed out). As it is the figure is not legible, and the text does not help the reader make this connection, if there even is one.

We thank the reviewer for this insightful comment and agree that the current description does not adequately interpret mesoscale structures influencing the glider transects. In the revised manuscript, we will expand the discussion to better account for the spatial structure of temperature and density, including a qualitative interpretation of mesoscale features in the Cretan Sea.

The ADT panels currently included in Fig. 6 (panel d, one per season) will be moved to a new Appendix A as illustrative reference material. In the main text, we will retain a concise qualitative description of the mesoscale context for each representative transect, including the dominant eddy polarity inferred from the ADT field, to better link the observed structures with the regional circulation. We also acknowledge that a fully quantitative analysis linking ADT variability to isopycnal heave and two-dimensional structure of temperature and salinity would provide further insight; however, this is beyond the scope of the present study and will be addressed in a dedicated follow-up analysis. Finally, the figure will be revised to improve clarity and readability, and to more clearly highlight the connection between the observed structures and mesoscale dynamics.

Table 1: does this table show anything not also shown in time series in Figure 7 below? No need for table in my opinion.

We agree, Table 1 will be removed in the revised manuscript.

Line 244: please explain how 'spreading from east to west' is to be inferred from the figure. What do you mean?

We will revise the text to provide a more accurate description: "...warmer and fresher surface waters are advected to compensate for the downward displacement of denser waters into deeper layers.'

Line 244 (fig 8). at 26 deg from 29 to 30 March, huge changes seen in upper 600 m, and only a few more days for observed changes at 25.6 deg. Surely advection played a role.

We agree, and in the revised manuscript we will explicitly acknowledge the potential role of lateral advection, in addition to local vertical processes, in shaping the observed structures along the transect in the Cretan Sea.

We also note that the interpretation of Fig. 8 requires caution, as the apparent spatial variability reflects the combination of transect orientation and temporal evolution. Specifically, at ~26°E the glider sampled *the region around March 20 (not March 29) during the east-to-west transect and again around 6 April on the return leg, while the changes observed near ~25.5–25.6°E were recorded a few days prior to 6 April.*

Line 245: How can you be sure that the difference in density (fig. 9a) is vertical convection and not horizontal advection of the denser water while the glider was absent?

We cannot conclusively attribute the observed density differences (Fig. 9a) to vertical convection, as horizontal advection of denser waters cannot be excluded. Dense water masses are formed in various subbasins of the Aegean Sea and may subsequently be transported and accumulated in the Cretan Sea. Therefore, the observed signal likely reflects a combination of processes, and its interpretation should be treated with caution.

Nevertheless, the presence of relatively dense waters during the observation period may be consistent with intensified winter conditions in 2022, although this cannot be firmly established from our data alone. Our intention is to highlight that the glider recorded a notable signal, which, when considered alongside observations from the broader region Teruzzi et al. (2024) and Potiris et al (2024), may suggest enhanced formation and/or redistribution processes during that period. Winter 2022 is not the primary focus of the present study and should be treated as one in another study supported with in depth analysis.

Line 259: Again, how can you be sure the difference in density, (fig 9b) is not horizontal advection of denser water into that area since the previous year? Any evidence from other sources?

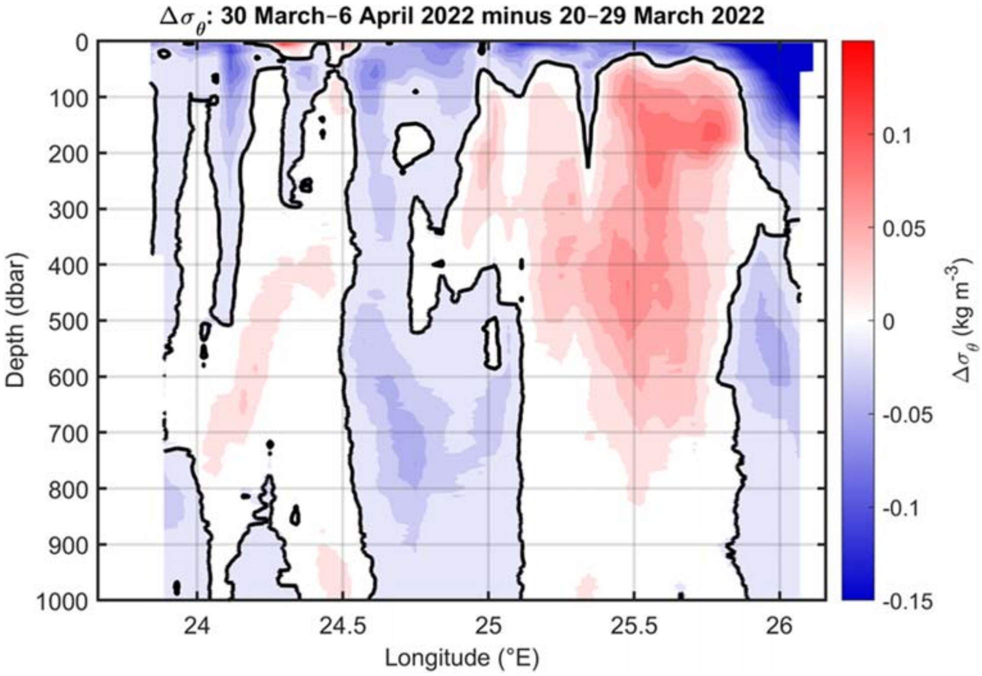
The same as above. The manuscript will be revised to explicitly clarify these points.

Line 261 (and fig. 9): convective events not well-justified. Difficult to understand which contour is 'zero' change, and if there is really positive change of density at any particular depth (all some shade of yellow). Spatial structure and advection could also be major reason for difference between years, since cannot be sure of rest of the Cretan Sea, outside this transect.

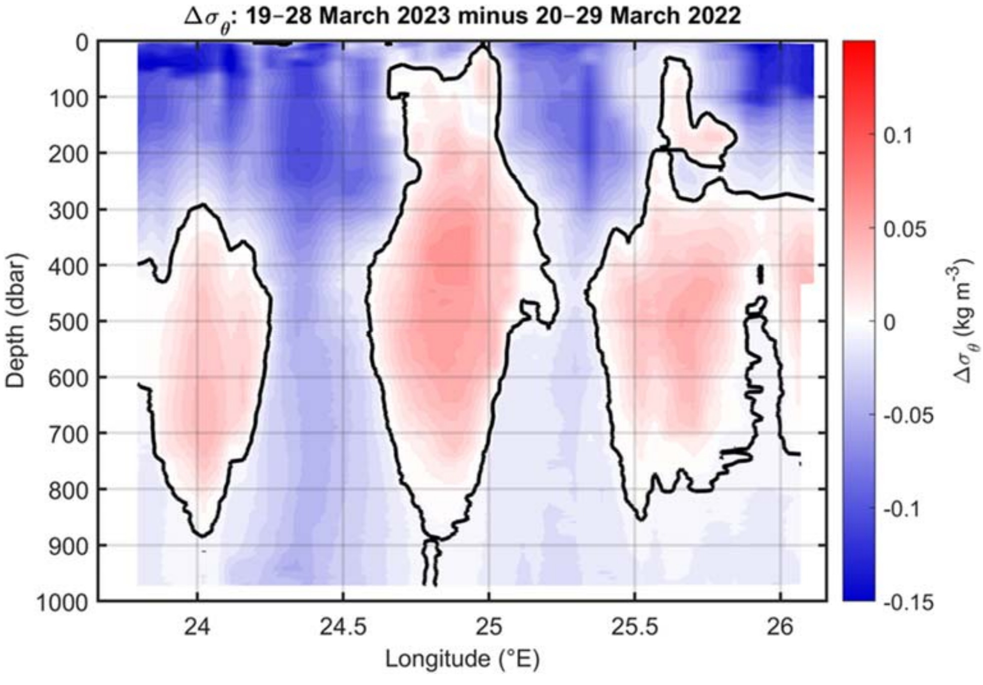
We thank the reviewer for these comments. We have revised Fig. 9 to address all points raised.

Regarding the colourmap, the original jet colourmap has no natural centre, making it difficult to identify the zero-change contour visually. In the revised figure we use a diverging blue-white-red colourmap centred explicitly at zero, where blue indicates no change, and red indicates an increase in density. The zero contour is additionally highlighted as a bold black line on both panels.

Regarding the colour scale, the original scale (-+0.3kg m^-3) was too wide, we have tightened the scale to -+0.15 kg m^-3 which makes the density changes of 0.01 -0.06 kg m^-3 reported in the text clearly visible.



$\Delta\sigma_\theta$: 30 March–6 April 2022 minus 20–29 March 2022



$\Delta\sigma_\theta$: 19–28 March 2023 minus 20–29 March 2022

Concerning the role of advection, we acknowledge that the observed density differences cannot be attributed unambiguously to convection alone. Horizontal advection of denser water from outside the transect area and inter-annual changes in mesoscale circulation could also contribute to the observed patterns. We will clarify in the text adding the following:

"The density differences shown in Fig. 9 reflect changes along the glider transect and cannot be unambiguously attributed to vertical convection alone. Contributions from horizontal advection and inter-annual mesoscale variability cannot be excluded. The consistency of the signal across the full east-west

extent of the transect and its agreement with independent regional evidence of anomalous deep mixing during winter 2022 (Teruzzi et al, Potiris et al) supports a predominantly convective interpretation, but further investigation, combining glider observations with model simulations is needed to fully separate these processes."

Line 273: unclear how this calculation is done. Rewrite.

*We will revise the relevant paragraph as follows. For each depth layer (200 - 600 m and 600 - 1000 m), a mission-mean value was computed by averaging all in-situ temperature and salinity profiles from a single representative transect per deployment, horizontally binned at 4 km resolution. One transect per mission was selected to ensure equal weighting of each deployment irrespective of mission duration. Linear trends were estimated by ordinary least squares (OLS) regression of the form y(t) = b0 + b1*t, where t is decimal year and b1 is the rate of change per year. Statistical significance was assessed at the 95% confidence level; results are reported as slope, SE, 95% CI, p-value, and R^2. Given the short record (N=14 missions, 2017 - 2023) and uneven seasonal sampling, these estimates should be interpreted as multi-year tendencies rather than long-term climatic trends. Full trend statistics, sensitivity analyses, and MATLAB code are available here: https://doi.org/10.5281/zenodo.19347841 .*

Line 276-286: this text fits better in the introduction.

We agree and will move lines 276 - 286 (the water mass description of intermediate and deep layers) to Section 1 (Introduction), where they will provide useful context for the reader before the results are presented.

Line 288: Below not 'Bellow'

This will be corrected.

Line 294 and Fig 10: why standard deviation and not standard error to take into account of number of observations? (of course assuming a normal distribution is not ideal in either case but the reader at least should have an idea of the number of observations used to make the average).

The error bars in Fig. 10 represent the standard deviation of layer-mean values across all 4 km bins within each mission transect and are intended to reflect the spatial variability (horizontal heterogeneity) along the transect during the mission rather than the statistical uncertainty of mission mean. A larger standard deviation therefore indicates stronger mesoscale or frontal structure along the transect. We will clarify this in the revised figure caption: "Error bars represent the standard deviation of layer-mean values across 4 km horizontal bins within each mission transect, reflecting the spatial variability along the transect rather than the uncertainty of the temporal mean."

Line 302: how well does the glider CTD sample the upper 2 m given the geometry of the sensor, the time response and reliability of the pump? Has there been any comparison with the CTD casts? With remote sensing? How many points given 30 sec sampling period could be expected in each? This kind of information about the sensor and general calibration/comparisons should be in the methods section and any comparisons relevant to trends made here.

The upper 2 m of the glider sampling is collected during the drifting mode where the glider drifts in the surface between ascend and the following descending. During that mode the glider is transmitting the previous yo collected data while at the same time is sampling between 0 -2 m 'standing' in a ~45 deg *position (~45 deg pitch) and drifting. From a glider and GPCTD operational standpoint, there is no indication that data quality is affected at this stage. These data correspond to 10 - 20 samples for each profile considering a mean 10 min time that the glider stays on the surface between each dive and they are well compared with the CDT casts that are performed during glider deployment and recovery (see reply on comment for Line 105) and in very good agreement with the sublayer (>2m) samples. All the necessary information will be added in the methods section in the revised manuscript.*

Line 306: "delayed restratification" How is this shown? In figure? Or is it from a reference?

We clarify that we refer to destratification rather than restratification and will revise any ambiguous phrasing accordingly. The delayed destratification is inferred from Fig. 10d, which shows that surface temperatures in autumn 2023 were approximately 2°C higher than those observed in autumn 2020. This anomalously warm surface layer is consistent with the record-breaking marine heatwave of summer 2023, documented by Copernicus Climate Change Service (C3S, 2024), and may have delayed the onset of surface cooling and convective mixing in the Eastern Mediterranean Sea. This delay is also supported by the transect structure observed during the autumn 2023 mission in the Cretan Sea (not shown).

We will revise the text to make this interpretation explicit: "The elevated surface temperatures observed in autumn 2023 (Fig. 10d) suggest a delayed onset of destratification relative to previous years, consistent with the anomalously warm summer of 2023 (C3S (2024), Martellucci et al. (2025))."

Line 308: "potential to disrupt patterns" can you be more specific? What did you have in mind exactly, and is it supported by something we can see in the figure? Or supports other work you can reference? Or just general comment?

We agree that the original phrasing was too vague and will revise it accordingly. Our intention was to refer to the delayed destratification associated with the prolonged summer conditions in 2023, which is suggested by the persistence of elevated surface temperatures into October–November. This delayed transition may influence the timing of seasonal mixing processes and, potentially, the evolution of water-mass properties.

More generally, in the Eastern Mediterranean Sea, persistent heat anomalies may enhance upper-layer stratification and thereby affect vertical mixing and water-mass characteristics; however, such processes cannot be directly resolved from the present analysis and would require dedicated temporal and process-oriented observations to be reliably evaluated. The original statement will be revised to reflect this more cautious interpretation.

Line 313 (fig 10): so is it true that there are 14 transects (each time point in the figure is one transect with its averages by layer) or 14 missions? 1 transect = 1 mission?

For the inter-annual trend analysis in Section 4, one representative transect was selected per mission (14 missions = 14 time points in Fig. 1), so each data point corresponds to one mission mean derived from one transect. A full mission typically contains 2-3 complete transects; we select one per mission to ensure equal temporal weighting irrespective of mission duration. We will clarify this in the revised Methods.

Line 314: you mean plots a, b, and c (not a, b, d)?

This will be corrected - the reference will read 'panels a, b, and c'.

Line 324: why not examine by density and not depth (evaluating the T and S changes over a range of densities representing the water mass)

We agree that analysing temperature and salinity by density would better isolate water mass changes. However, in this study we chose to examine variability in depth coordinates to provide a more direct representation of the observed variability and to facilitate comparisons with previous studies and different platforms. We note that isopycnal (or water mass oriented) analysis will offer additional insight into water-mass transformations and will be considered in our future work.

Line 340 (fig 11): I don't believe this figure adds much information. Replot with different axis of some of fig. 10 lines. Slopes of lines in table 2.

We will improve Fig. 11 in the revised manuscript, by i) labelling the trend slope an uncertainty on each panel, ii) ensuring consistent y-axis ranges between the intermediate and deep layer panels to facilitate visual comparison, and iii) adding the N=14 data points more clearly. We will retain Fig. 11 as we believe it provides an accessible visual representation of the six-year trends.

Line 388: what do you mean 'selected to be correlated'? Why those years?

We thank the reviewer for pointing this out. We will rewrite this sentence. The winter seasonal climatology for the period 2000 - 2015, the most recent climatological period available in the Iona et al (2018) atlas, was used as a reference state. Winter glider missions (2018 - 2023) were compared against this reference to quantify the departure of recent hydrographic conditions for the pre-2017 baseline.

Line 400: Please explain. How can a change in annual mean, relative to climatology be 'consistent' with winter convection? You mean consistent with a particular change in winter convection? What is the process?

The relevant sentence will be revised as follows:

'As shown in Fig. 13b, the salinity deviation decreases gradually from 0.31 at the surface to −0.05 at 1000 m. The positive temperature and salinity deviations observed in the upper ~400 m may reflect enhanced vertical redistribution of heat and salt, potentially associated with wintertime mixing and intermediate water formation processes in the Cretan Sea. During CIW formation, surface waters are mixed and subducted to intermediate depths, transferring their thermohaline properties downward and leaving a coherent anomaly across the upper and intermediate layers. However, as these results are based on annual-mean fields, this interpretation remains indirect and cannot be unambiguously attributed to winter convection alone.'

Line 413: how can the salinity and depth of a water mass change by 'exchanges'? Does this mean lateral mixing with Med Sea masses? And the 2nd mechanism is vertical mixing ('in the layers of Cretan Sea..')? This needs more explanation.

We agree that the mechanisms were not sufficiently specified in the original text and will clarify this in the revised manuscript. Changes in the salinity and vertical distribution of a water mass in the Cretan Sea can arise from both lateral and vertical processes. In particular, Vervatis et al. (2011) attributed the deepening of the Transitional Mediterranean Water (TMW) core to the outflow of dense Cretan Deep Water toward the Eastern Mediterranean Sea through the Cretan Straits, highlighting the role of lateral advection and dense water export in modifying the vertical structure. Vertical processes, such as mixing and convection *within the Cretan Sea, may also contribute to redistributing water-mass properties, although their relative importance cannot be quantified from the present dataset. We will revise the text to explicitly distinguish between these processes.*

Vervatis, V. D., S. S.Sofianos, and A.Theocharis (2011), Distribution of the thermohaline characteristics in the Aegean Sea related to water mass formation processes (2005–2006 winter surveys), J. Geophys. Res., 116, C09034, doi:10.1029/2010JC006868.

Line 415: Salt fingering: Has any evidence been found of this in the CTD profiles? Shouldn't it be visible during this period if indeed the water mass was changing for this reason?

Evidence of salt fingering is observed in the shipborne CTD profile collected in the eastern Cretan depression on 19 March 2023 (Fig. 17), as explicitly described by Velaoras et al. (2025). In the glider dataset, thermohaline staircases are only weakly evident in the easternmost profile (two thermohaline steps identified), located in close proximity (~9.2 km) to the CTD station and sampled two days earlier (17 March 2023).

However, the limited detection of salt fingering signatures in the glider data likely reflects sampling constraints, as the glider measurements were acquired at a relatively coarse temporal resolution (~4 Hz) during that mission, which may not fully resolve the fine-scale vertical structure associated with salt fingering.

Line 428: Mean core depth: How were the limits of the 'core' defined? Max/min depth? Max/min density? Values?

We will add the following in the Methods: The TMW core was identified in each mission-mean profile as the absolute salinity minimum between 500 and 1000 m depth. The core depth was defined as the pressure level of this minimum; the core salinity and temperature were taken as the values at that pressure level. Only missions with profiles reaching at least 1000 m were included in this analysis (8 of 14 missions), with the exception of the eastern portion of the first mission (November 2017), in which the TMW was evident between 600 and 700 m and was therefore included. Standard deviations shown in Fig. 15 represent the spread of core values across individual profiles within each mission.

Fig 16 and 17: Why not show the 2 glider profiles nearest to these to avoid the smoothing by the averaging process?

This is a valid suggestion. For Fig. 16 we add the two individual glider profiles nearest to the E1M3A position from the March 2022 mission, alongside the mission-mean profile, to demonstrate that the mean profile represents the local hydrographic structure. For Fig. 17 we will similarly add individual profiles nearest to each CTD cast location.

Line 493: I am not clear on the mechanism for 'accelerating the downward shift' of TMW.

The conclusions section will be revised to align with the prioritized objectives outlined in Comment 1; (https://doi.org/10.5194/egusphere-2025-6542-AC1). We acknowledge that the present dataset does not allow us to identify or demonstrate a mechanism responsible for an accelerated downward shift of the Transitional Mediterranean Water (TMW) core. Instead, we frame this as an open question: whether the convective event during winter 2022 may be linked to the apparent deepening of the TMW core to ~1125 m in the Cretan Sea.