**Reviewer #1:**

The submitted manuscript by Agiadi et al. is a valuable contribution to study the paleo-environmental evolution of the Island of Rhodes during the Pleistocene as it uses a multi-proxy approach to identify changes in productivity and temperature. In addition, the authors estimated the fish distribution depths at Lardos to assess the response of fish to the environmental changes. The study provides unique SST and SSS data of the Island of Rhodes that contribute to the understanding of climate change in the eastern Mediterranean Sea. Therefore, I believe the manuscript is a valuable contribution to the field and worth being published.

Thank you for carefully reviewing our manuscript and for this assessment.

However, there are some aspects that should be clarified and adapted before publication, which is why I suggest a major revision.

For once I believe the many proxies used in this study are rather confusing and not all contribute to the understanding of the paleo-environmental conditions. Much of the discussion focuses on the d13C of benthic and planktic foraminifera, while ostracods and sponge spicules for example seem not to contribute to the understanding. In that regard, a summarizing figure would additionally be needed, where the reader better can follow the discussion.

We agree and we will include in the revised manuscript a summarizing figure, on which we can also discuss more the results from the ostracods and sponge spicules.

The authors suggest capturing the MPT and use the age model provided by Titschack et al., 2013. However, in 2024 the age model for Lardos has been refined by Eichner et al. towards a younger age. I suggest that the authors also check and discuss whether they are really in the time frame of the MPT by also considering the refined age model from 2024. In this regard, I would suggest changing the title because in the discussion the MPT is not mentioned at all. From the discussion it is not clear if the MPT did influence the study area or not.

Regarding the title, we will change it to "Pelagic ecosystem responses to changes in seawater conditions during the Middle Pleistocene in the Eastern Mediterranean".

On the other hand, we are not convinced by the age model published by Eichner et al. (2024). Titschack et al. (2013) built an age model of the Lardos section (which has since been applied to several studies of the studied section; e.g., Agiadi et al., 2018; 2023; Porz et al., 2024, all quoted in the manuscript), which was based on the $^{234}U/^{238}U$ dating of three *Desmophyllum pertusum* coral fragments collected from two debris flows cropping out in the lower part of the Lardos section. The two fragments from the lowermost debris flow level (Figure 1 of the manuscript) yielded mean ages of 755.2 ± 15.5 kyr and 756.0 ± 17.5 kyr. The one fragment from the uppermost debris flow level (which is located ~2 meters above the part of the section we analyzed in our study) yielded a mean age of 688.9 ± 15.5 kyr. This age model of Titschack et al. (2013), as that of Eichner et al. (2024), is well supported by the published calcareous nannofossil biostratigraphic data as well as the continuous occurrence of *Stilostomella* sp. in the lower part of the section (Titschack et al., 2013; Eichner et al., 2024). It has been further validated by Agiadi et al. (2023), who provided a new $^{234}U/^{238}U$ dating for an additional *D. pertusum* fragment collected from the lowermost debris flow level, which yielded a mean age of 744 kyr (95% confidence intervals).

The revised age model proposed by Eichner et al. (2024) entirely relies on peak-to-peak correlations between their benthic foraminiferal $\delta^{18}O$ data and the global ocean LR04 record (Lisiecki and Raymo, 2005), but excluding the use of the coral ages by Titschack et al. (2013) as tie points (the fourth $^{234}U/^{238}U$ age published by Agiadi et al. [2023] was not cited in that study at all). Eichner et al. (2024) therefore considered that the coral fragments analyzed by Titschack et al. (2013) from two different debris flow levels, had been systematically reworked, or hypothesized that the $^{234}U/^{238}U$ dating by Titschack et al. (2013) did not meet the necessary criteria and an open system could have biased the estimated ages to older ages.

In our opinion, there is no tangible evidence for the reworking of these coral fragments collected from different levels in such type of upper bathyal environments and hemipelagic

deposits. We find in addition very unlikely that all four coral fragments collected from two different levels yielded nearly constant age offsets of ~50–60 kyr. Consequently, we favour Titschack et al.'s (2013) age model and are confident that the studied sediments were deposited during the MIS23–MIS18 interval. In our revision, we will add to the chronostratigraphic background of the studied section to clarify why we used Titschack et al.'s (2013) age model instead of Eichner et al.'s (2024) one.

In the discussion I am missing a critical view on the data provided by the study. It is not really clear what these regime shifts are and some of the suggested changes are not as clear to me as suggested in the text. Further the authors should discuss the data to a much greater extend, especially chapters 4.1.1-4.1.4. I also suggest changing the structure of the discussion and instead of describing the regime shifts individually, discuss them as a whole. What are the changes and what is responsible for it?

Our aim in this work was to evaluate the effect that paleoceanographic changes had on different marine organisms. In order to do so, we first needed to establish when these paleoceanographic changes took place, in the study area. We used the Sequential T-test Analysis of Regime Shifts (STARS) algorithm developed by Rodionov (2004) to detect rapid, statistically significant shifts in each paleoceanographic parameter (SSS, SST, productivity) separately. This algorithm uses the t-test to determine whether sequential records in a time series represent statistically significant departures from the mean value observed during the preceding period, and outputs a value for the regime shift index (RSI), which represents the sum of the normalized anomalies indicating the shift magnitude. The STARS algorithm is capable of detecting statistically significant shifts even at the edges of a time series, which is why it has been the preferred statistical method in many previous studies (e.g., Wahl et al. 2014; Yuan et al. 2020; Harning et al. 2021; Barron et al. 2022; Agiadi et al. 2024, BG; Ikeda et al. 2024; Lu et al. 2025).

Regime shifts are rapid reorganizations of the ecosystem that are reflected in the analysed parameter. Therefore, the cause may be different in each case depending on which parameter was analysed. This is the reason why we feel that we should discuss each one separately. Nevertheless, you are right, and the manuscript can benefit much from providing such explanation (above) in the methods, and elaborating more in the discussion. The additional schematic figure should also help with that.

We propose to revise subsections 4.1.1–4.1.4 to clarify how the paleoceanographic conditions in the study area changed from one interval to the next, as denoted by the determined regime shifts. Specifically, the interval 923–858 kyr is upward bound by a regime shift at 851 kyr detected by $\delta^{13}C_{ruber}$, which we attribute to increased primary productivity after this point. This regime shift is immediately reflected during the interval 851–798 kyr in the foraminifera biomasses, and the ostracod and sponge spicule counts. The next regime shift takes place at 788 kyr and is reflected in $\delta^{13}C_{peregrina}$ and the ostracod weighted-average body size, which is interpreted as decrease in secondary, benthic production after that point. Finally, the last regime shift in the study area takes place at 760 kyr, is detected in TOC, $\delta^{18}O_{peregrina}$ and $\delta^{18}O_{ruber}$, and is attributed to the shallowing of the study area.

Barron et al. (2022). An 11,300 yr record of paleoclimatology and paleoceanography of the central California coast in a gravity core from Pioneer Seamount. Quaternary International 621: 74–83.
Harning et al. (2021) Response of biological productivity to North Atlantic marine front migration during the Holocene. Climate of the Past 17(1): 379–396.
Ikeda et al. (2021) Regime shift of skeletal d13C after 1997/1998 El Nino event in Porites coral from Green Island, Taiwan. Scientific Reports 14: 23332.
Lu et al. (2025) Tipping point-induced abrupt shifts in East Asian hydroclimate since the Last Glacial Maximum. Nature Communications 16: 477.
Wahl et al. (2014) An 8700 year paleoclimate reconstruction from the southern Maya lowlands. Quaternary Science Reviews 103: 19–25.
Yuan et al. (2020) Phytoplankton responses to climate-induced warming and interdecadal oscillation in North-Western Australia. Paleoceanography and Palaeoclimatology 35(3): 3219PA003712.

Please be more precise when talking about depth. You should specify in the whole manuscript if you are talking about water depth/section depth/ fish depths etc. It is not entirely clear.
Thank you for pointing this out. We will correct in the revised manuscript, to specify when we are referring to depth in the water column where the fish were living in (fish depth distribution), section depth (height), or depth of the sea bottom at the time (paleodepth).

The authors should make clear that Agiadi et al., 2024b is a database with the data generated for this study and not an earlier publication where the data was already described in. Why not refer to it in the journals section "data availability" where you can add the doi. At the moment it is just confusing.
We submitted the dataset to Zenodo, to make it openly accessible immediately, and then we cited that in the text. We can rephrase the in-text citation to reflect that it is indeed our dataset that was produced for this particular work.

Specific comments:
Line 59: benthic and planktic foraminifera? Oxygen is also an important factor. Specify if you mean water, surface or bottom water sea temperature –
We propose to change this to 'In general, foraminifera are very sensitive to parameters of ambient seawater, oxygen, such as food supply, and sea temperature, which control community composition and pelagic biomass production (Gooday and Jorissen, 2012; Schiebel et al., 2018), while for the planktic foraminifera, light intensity also plays an important role.
Gooday A.J., Jorissen F.J. (2012) Benthic foraminiferal biogeography: controls on global distribution patterns in deep-water settings. Annual Review of Marine Science 4: 237–262.
Schiebel et al. (2018) Advances in planktonic foraminifer research: new perspectives for paleoceanography. Revue de Micropaleontologie 61(3–4): 113–138.

Line 61: "no consistent global patterns" – related to what? Also, I would not assume global changes in productivity. Please check.
We meant that no consistent responses of foraminifera biomasses and isotopic compositions have been found on a global scale.

Line 73: "many areas worldwide" – but only two references are provided?
We can add to these the following references to cover other parts of the world as well:
Cronin et al. (2014) Quaternary ostracode and foraminiferal biostratigraphy and paleoceanography in the western Arctic Ocean. Marine Micropaleontology 111: 118–133.
Zarikian (2016) Pleistocene Deep Sea ostracods from the Bering Sea (IODP expedition 323). Deep Sea Research Part II: Topical Studies in Oceanography 125–126: 96–106.
Zarikian et al. (2022) Ostracod response to monsoon and OMZ variability over the past 1.2 Myr. Marine Micropaleontology 174: 102105.

Line 93: What are the hypotheses for the study? Even though it is possible to shortly mention the results here, it would be better to introduce the hypotheses and the aim of the study.
The hypothesis is that glacial–interglacial variability in paleoceanographic parameters would have affected the fish diel vertical migration (DVM) patterns, particularly during important climatic transitions such as the MPT, and this is key to quantifying the contribution of DVM to the biological carbon pump in the past.

Chapter 2.1. a map showing the island and the LBF outcrops including Lardos would be helpful here.
We can add that, of course.

Line 111: The lithology is shown in figure 1 and should also be shortly described in the text.
We describe this in line 120: "The total section comprises 30 meters of marine marls and silts. In this work, we focus on the 2.4 m lowest marls of the section…". We can cite again the figure at that point.

Line 117: Is the total section of 30 m all comprising of LBF?
The section exposes the upper part of the Lindos Bay Formation and the lowermost part of the Arkhangellos Formation. The part we studied is at the base of the section and belongs to the Lindos Bay Formation.

Line 121: Eichner et al. 2024 should be mentioned here
We do not agree with and have not adopted the age model of Eichner et al. (2024). We will elaborate on this point in the revised manuscript, as detailed above, and of course refer to their study.

chapter 2.2 Paleodepth estimates – Please refer to paleo-water depth.
We use "paleodepth" to refer to the estimated depth of the sea bottom at each time. We will clarify this in the revised manuscript, also to differentiate from fish lifetime-average depth and depth in the water column.

Line 131: Calgon normally dissolves lime? Please explain why this step has been made
That was an error: we used hydrogen peroxide. We will correct. Thank you.

Line 132: Has this temperature an effect on the isotope signal?
We followed the commonly used protocol of drying at 60°C. This temperature is not expected to affect the isotopic signal.

Chapter 2.3: the chapter needs some reorganization. I would suggest to start with one of the stable isotope measurements, e.g. bulk samples and describe the process completely until the end and then start with the next, e.g. foraminifera, ostracods and otolith. It is very confusing to jump from one organism to another and back.
Yes, we see your point and we agree. We will reorganize the text here, as you suggest. Thank you.

Line 145: If the bulk isotope measurements are not presented, this information could be removed?
We have provided the isotopic results for the bulk as well, but indeed we do not obtain much information from them for this work. We will therefore move this part to the supplement.

Line 157: Why this fraction?
In the photosymbiotic species *Globigerinoides ruber*, there is a positive correlation between test size and Chlorophyll-a content, and the symbiont abundance of single *G. ruber* is scaled to the growth of the foraminifer. This affects the $\delta^{13}C$ composition of the shell through the lifetime of the foraminifera. To limit such an effect on the $\delta^{13}C$ composition, all analyzed foraminifera were picked from the 250–355 µm size fraction.

Line 165: No diagenetic overprint of the otoliths found in the section that could influence the isotopic signal?
In general, otoliths suffer little from diagenetic alteration, mostly maintaining their microstructure, chemistry and mineralogy, and remain aragonitic even after millions of years (Dufour et al. 2000; Prasanna et al. 2021). Nevertheless, we selected otoliths preserving well all of their morphological characteristics without secondary coloration and/or signs of bioerosion or encrustation that may point toward extensive diagenetic alteration (Agiadi et al., 2022). We will add this information in the revised text.

Agiadi et al. (2022) The taphonomic clock in fish otoliths, Paleobiology 48, 154–170.
Dufour et al. (2000) La diagenese des otolithes par la comparaison des donnees microstructurales, mineralogiques et geochimiques: Application aux fossiles du Pliocene du Sud-Est de la France, Bull. Soc. Geol. France 171, 521–532, 2000.

Line 171: This is somewhat a repetition from the sentence before. The references could be added to the sentence before.
We will revise.

Line 175: The otoliths not the fishes?
Yes, corrected.

Line 177: MeOH, please explain.
MeOH stands for methanol

Line 183: Bulk data?
The isotope measurements were similarly performed for all carbonates (foraminifera, ostracods, otoliths and bulk), but the information on bulk will be shifted to the supplement.

Line 192: Data are shown in VPDB, correct?
Yes, these are V-PDB, as explained at line 192.

Line 240: How can C/N ratios be used to detect changes in sedimentation rates? Please explain.
The C/N ratio is considered in order to distinguish between marine and terrestrial organic matter, not the sedimentation rate. The changes in the sedimentation rate, should also be considered when evaluating TOC to infer primary productivity through. We understand that the sentence originally was a bit confusing and we will rephrase.

Line 252: Is Agiadi et al., 2024b the right reference here? Isn't it the data set? It reads as if the equation was invented in the study. Same in line 259.
The reference to Agiadi et al. (2024b) is to indicate where the BIT raw results are provided. We will clarify.

Line 263: What is the rationale behind the size fractions?
These are the size fractions used by Schiebel and Movellan (2012) and Movellan et al. (2012) to establish the equations (Eqs. 7 and 8) that allow us to calculate the foraminifera biomasses. We will revise the sentence to clarify this.

Line 269: Was A. tepida also used in this study?
No, we did not estimate biomass for *A. tepida*, but instead used the equation established for this species to estimate the relative changes in benthic foraminifera biomass from all the species in our samples. Naturally, this equation may not hold for other species, but at the moment there is no alternative equation available for any other benthic foraminifer species. We will add this limitation to the text.

Line 272: Please shortly explain why ostracods and sponge spicules are counted. Used as proxies for what?
We explain in the Introduction that ostracods are indicative of changes in oxygenation, whereas sponges are sensitive to pH, temperature and oxygenation. Indeed, we will discuss more their results.

Line 277: I assume you mean planktic forams?
We write "Foraminifera $\delta^{18}O$ records reflect both the temperature and the ambient oxygen isotopic composition of seawater ($\delta^{18}Osw$) in which the shells precipitated." This is true for both planktic and benthic foraminifera, but in the case of benthics the $\delta^{18}O$ values give

insight about the deep water temperatures and sea-level/ice volume changes (Hoogakker et al. 2024).

Hoogakker et al. (2024) A review of benthic foraminiferal oxygen and carbon isotopes. Quaternary Science Reviews 342: 108896.

Line 285: Can you use a species-specific equation for another species? You could provide the equation in the manuscript. Please also explain a bit more why it's the most accurate. Also, if you can remove the temperature trend, then you could compare this trend with the calculations from Tex86H.
We should not use a different equation. We explained that *Orbulina universa* low-light paleotemperature equation of Bemis et al. (1998) is widely regarded as the most accurate for estimating paleo-SST in symbiotic planktonic foraminifera from subtropical settings in deep time (Williams et al., 2005). We can add the equation in the text, of course. $TEX_{86}^{H}$ is directly used here to remove the temperature control similarly to the protocol earlier studies (Kontakiotis et al., 2022; Vasiliev et al., 2019) (lines 282–285).

Bemis et al. (1998) Reevaluation of the oxygen isotopic composition of planktonic foraminifera: experimental results and revised paleotemperature equations. Paleoceanography and Paleoclimatology 13(2): 150–160.
Williams et al. (2005) Efficacy of $\delta^{18}O$ data from Pliocene planktonic foraminifer calcite for spatial sea surface temperature reconstruction: comparison with a fully coupled ocean–atmosphere GCM and fossil assemblage data for the mid-Pliocene. Geological Magazine 142(4): 399–417.

Line 294: Is the adjustment plus or minus?
The 0.48‰ is subtracted. We will change the text to say instead "by subtracting 0.48‰"

Line 298-299: It is the modern relationship? It has not changed through time?
Yes, this is a modern relationship. We do not know if it changed through time, and this obviously is a limitation. However, this is a fundamental assumption in paleoceanography. It is far beyond the scope of this manuscript to discuss the robustness of such assumption, especially for an area such as the Aegean Sea, where hardly any data for testing the robustness of the present relationship is available. We will note this in the limitations of the study.

Line 308-313: These methods are not widely applied and some explanations are needed here.
We will elaborate in the revised manuscript, as explained above: Regime shifts are rapid reorganizations of the ecosystem that are reflected in the analysed parameter. We used the Sequential T-test Analysis of Regime Shifts (STARS) algorithm developed by Rodionov (2004) to detect rapid, statistically significant shifts in each paleoceanographic parameter (SSS, SST, productivity) separately. This algorithm uses the t-test to determine whether sequential records in a time series represent statistically significant departures from the mean value observed during the preceding period, and outputs a value for the regime shift index (RSI), which represents the sum of the normalized anomalies indicating the shift magnitude. The STARS algorithm is capable of detecting statistically significant shifts even at the edges of a time series, which is why it has been the preferred statistical method in many previous studies (e.g., Wahl et al. 2014; Yuan et al. 2020; Harning et al. 2021; Barron et al. 2022; Agiadi et al. 2024, BG; Ikeda et al. 2024; Lu et al. 2025).

Line 314: Why all the correlations are measured here? Some make sense, others possible not.
We tested these correlations to detect covariance between temperature or productivity, the isotopic signals in the fossil skeletal material of the different organisms and their biomasses or abundances.

Chapter 2.9 Lifetime-average depth – again it would be good to specify what depth you are talking about. A lot of assumptions are made in this chapter. Can the authors be sure that they get correct estimates?

These mesopelagic fishes are living in the water column, at mesopelagic zone depths during the day and at euphotic zone depths during the night. The precise time they spend in each zone each day depends on the species, the season, the ontogenetic stage. Our approach allows us to estimate the average depth for their whole lifetime, for each species, and at each time (height along the section). Our estimates are obtained by modeling the $\delta^{18}O$–depth relationship for glacials and interglacials within the time interval and in the area of study. This is a simple model, whose main assumptions are that the thermal gradient remains the same within each MIS and that the water column is always well mixed. We then use these results to discuss potential changes in fish diel vertical migration and implications for the carbon pump in subsection 4.4.

Line 322: Are you talking about benthic or planktic foraminifera?
We use both planktic and benthic foraminifera values here: the surface represented by the planktic and the bottom by the benthic.

Line 322-323: Are they only representing the thermohaline gradient in the Aegean? And what are these certain assumptions? Please specify.
Yes, these values of the foraminifera represent the conditions in the study area in the southeastern Aegean. The assumptions that we make to estimate the fish lifetime-average depth are elaborated on already in the paragraph following this introductory sentence.

Line 323-325: Depth regarding water depth at the bottom or water column? If talking about lifetime-average depth of fish and by looking at your figure 8 I would imagine water column and again it is not clear if benthic or planktic d18O were used. Unfortunately, I still do not understand how the life-time depth was estimated. It is not clear to me how the d18O signals of foraminifera can be used to calculate the position of fishes in the water column. Did you use the relative abundances of the planktic species or did you use any other weighting of the signals?
We estimated the average depth that the fishes inhabited in the water column throughout their lifetime using the $\delta^{18}O$ of their otoliths. The $\delta^{18}O$ of otoliths and of foraminifera shells all reflect the $\delta^{18}O$ of ambient seawater. The planktic foraminifera species *Globigerinoides ruber* lives in the water column 10–50 m depth, and therefore its $\delta^{18}O$ corresponds to those depths. The planktic foraminifera species *Globoconella inflata* lives at 100–300 m depth in the water column, so its $\delta^{18}O$ should correspond to 100–300 m. The species *Uvigerina peregrina* is benthic, and therefore its $\delta^{18}O$ should correspond to the sea bottom (the paleodepth). We used all these foraminifera values to create a model of how $\delta^{18}O$ changes with depth in the water column during glacials and during interglacials separately. This regression model, which is shown in Fig. 5, predicts the depth in the water column for each $\delta^{18}O$ value. Thus, we used this model to obtain the depths corresponding to the fish otolith $\delta^{18}O$ values that we had measured. Because otoliths grow throughout the lifetime of the fish, and they biomineralize day and night, their $\delta^{18}O$ values are actually lifetime-averages. Consequently, the depths obtained from the model are also fish lifetime-average depths.

Chapter 3.1: These depths refer to local or regional sea-level coupled to the global sea level? What about tectonic motions? These reconstructions are only used to support the age model? if so, please note this accordingly in the methods section und add this sentences to the age model section in the results. See also below.
We agree. Vertical motions are not in the scope the MS and our reconstructions are indeed used to support the selected age model. Following the reviewer, we will note this in the methods and add a sentence in the age model section of the results.

Line 359: Why not show the data in a figure?
They are reported in the table, and we can show them in the figure. If we plotted them, however, we would obstruct the visualization of the biota-derived isotopes. The isotopes on bulk material included also the carbonate contribution of abiotic components, these being a

function of the sedimentary influx. We shift in the revised manuscript the bulk isotopic data to the supplement.

Line 360: Why those values were measured - to support the age model?
We measured the isotopic compositions of the foraminifera to reconstruct the thermohaline gradient and estimate the fish lifetime-average depth.

Line 369: Not shown and could possibly be removed from the methods and results?
Done

Line 376: Why the correlation was measured here? What is the message?
Otoliths grow throughout the lifetime of the fish and their size is correlated with fish body size. The significant correlation between otolith weight and otolith $\delta^{13}C$ for both fish species here reflects the expected relationship between the metabolic rate and the age of the fish, and confirms the validity of our approach.

Chapter 3.3: The results section should start with the age model as figure 1 shows ages. It is often difficult to refine an age model based on d18O alone. Is there further support for the refined age model - marker species, for example? It is also not entirely clear if the age model has simply been extended for the parts the authors digged deeper or if the age model of Titschack et al. was changed. To me it looks like the authors agree to mostly with the previous publication. How about the absolute dates of the dated corals. Were they considered?
Yes, the coral ages were considered (see above) and we followed the age model by Titschack et al. (2013) that we refined based on our new *G. ruber*, *G. inflata* and *U. peregrina* oxygen isotope analyses and paleodepths reconstruction. In the short time interval recovered and studied, there is no calcareous plankton bio-event that could be used as an anchor point to further support our revised age model. Following the reviewer, in our revision, we will start the results section with the revised age model.

Line 392: Eichner et al. (2024) also refined the age model of Titschack et al. (2013). This should be noted here.
We did not adopt the model of Eichner et al. (2024), but we will clarify this here, as discussed above.

Line 393: Changes in paleo-water depth could also be related to tectonics.
We agree and will change the text accordingly

Line 406-408: How the sedimentation rates were estimated should be moved to the methods section. Can you simply interpolate? In fact sedimentation rates are missing in the results section. Please add.
We agree and will add the estimated sedimentation rates

Line 409: This sentence could be removed. The dataset is mentioned in the "data availability" section
Done

Chapter 3.5: I wonder whether the TOC values reflect the TOC during deposition. Weathering could be a problem (oxidation of organic matter).
The reported TOC values reflect are an unknown function of preservation, oxidation and dilution driven by the sediment influx. It is almost impossible in paleo studies to precisely account for the contribution of each (i.e., preservation, oxidation and dilution). We will note this in the revised manuscript.

Line 470: As you include so many d18O values in the study, you should always name which one you are talking about, as you have done in the methods section

Thank you. Yes, we will change in the revised manuscript.

Line 490: Why all these correlations?
We performed these correlations to detect any covariance between temperature and productivity, and the biomasses of foraminifera and ostracods.

Line 440 and 449: Why this differentiation between the size classes? Is this important for the story to tell?
The different patterns in the size classes reflect growth and shell size of foraminifera, which may be affected by nutrient availability, temperature. It is important for us to see if there are intervals where the size of the shells increases overall. We will elaborate on this point in the Discussion.

Line 500: It is only a local signal - this aspect should be highlighted here
This a regional record, that is affected by general global trends with influence of the regional paleo-circulation at the time of the sediment's deposition.

Line 502: There is no data available?
Not more than we present in this manuscript.

Line 505: The reader does not know what and where site LC07 is. It should be described, also including water depth.
LC07 location is shown in the map of Fig. 1. We will cite the figure at this point, and add the information about the depth. Thank you.

Line 506-507: Unfortunately, the legend of figure 7 is missing, so that one cannot check on the temperature differences. I also wonder why the authors decided on comparing the SST record of the eastern Mediterranean Sea with the Atlantic Ocean. The Mediterranean Sea has a completely different character than the Atlantic Ocean. Comparing the interglacial-glacial temperature differences of both does not indicate that the MPT is responsible for this change. Indeed, I would rather agree that the semi-enclosed configuration and the special conditions in the Mediterranean Sea are responsible.
Thank you for pointing this to us. The embedded version of the figure was wrong. We will correct this.
We do agree that the semi-enclosed configuration and the special conditions in the Mediterranean Sea are responsible for the SST and SSS, and further more influencing the biotic response. The aim of the figure and also of this paper is not to detect the effects of the MPT on temperature, but to investigate the responses of marine organisms to paleoceanographic changes (SST, SSS, primary productivity) in the Middle Pleistocene, specifically within the time interval of the MPT. We will clarify this in our Introduction and in the Discussion.

Line 509-510: What is the system?
We refer to the present-day configuration of the Lardos in the Eastern Mediterranean, restricted, highly saline and very warm system.

Line 514: Logical break between two sentences. Sapropels were not topic of this paragraph, why mentioning here?
We mention sapropels here, because it could be expected that we find them in such a Mediterranean setting.

Line 514: New paragraph here. It is hard to follow here since all proxies are merged for interpretations. It would be helpful having a synthesis figure showing all mentioned data in one graph (see also below).
We will separate a new paragraph as suggested, and add a synthesis figure.

Line 516: Not sure whether time intervals mentioned here agree with the regime shifts shown in the figures.
We have not illustrated the regime shifts on the figures (only the MIS), but we can indicate them of course.

Line 523: What's about a synthesis figure to support the statements here? It is hard to compare different figures. In a synthesis figure, it could be mentioned which record is a proxy for what.
We will add the synthesis figure, as suggested.

Line 527: d13C of G. ruber looks like it is more or less the same in MIS 23 and 22.
Thank you. Corrected. It should be "higher in MIS 21, and lower in MIS 22 and MIS 23".

Line 529: Are deep sea and shallower habitats comparable so that the authors can use the results from deep sea studies for their study?
We do not use data from other studies. In Line 529, we mention that "Benthic foraminifera accumulation rates from deep-sea drilling sites are often used to estimate changes in productivity (Diester-Haass et al., 2018)." This is a general statement, but we don't use data from Diester-Haass et al. Instead, we believe that "Our results suggest that productivity during the 923–864 kyr interval fluctuates with climate, as reflected by the values of $\delta^{13}C_{ruber}$, $\delta^{18}O_{ruber}$ (Fig. 3) and the foraminifera biomasses (Fig. 4)." We will add "at Lardos" in this sentence, to make it clear that this an interpretation of our data.

Line 530: I really do not see the d13C of G. ruber is fluctuating that much. It is peaking at 864 ka but in the older parts it has variations in the same range as U. peregrina and G. inflata.
In this sentence "Our results suggest that productivity during the 923–864 kyr interval fluctuates with climate, as reflected by the values of $\delta^{13}C_{ruber}$, $\delta^{18}O_{ruber}$ (Fig. 3) and the foraminifera biomasses (Fig. 4)." we missed adding "$\delta^{18}O_{ruber}$". Indeed, during this 923–864 kyr interval $\delta^{13}C_{ruber}$ and foraminifera biomass, which are considered here proxies of primary productivity and secondary production, respectively, follows well $\delta^{18}O_{ruber}$ that is mostly influenced by climate.

Line 531: What is the climate proxy here? d18O? And which kind of the climate system is here referred to?
Yes, $\delta^{18}O_{ruber}$. We are referring to the Eastern Mediterranean.

Line 535-536: and how is that? Please discuss your results more.
BIT indicates the relative influx of organic matter coming from terrestrial sources. The increased influx of terrestrial organic matter brings with it nutrients and increases secondary production, which is reflected then in the sponge weighted average size.

Line 540: of planktic foraminifera. For benthic it is different
We agree. For benthic depends on the amounts of carbon residing in the bottom waters too. We will amend the sentence in the revised manuscript.

Line 547: and d13C G. ruber? What does this mean?
Yes, $\delta^{13}C_{ruber}$ show a drastic decline in this interval, which reinforces our interpretation of enhanced organic carbon burial.

Line 549-550: Are those two locations really comparable like this?
There are scare records to compare within the Mediterranean (see map of Fig. 1) and this site in the Iberian margin is one of the closest ones on the Atlantic side. Even though it is clearly not comparable to Lardos, we believe it is worth mentioning this information, for context. Even though the Mediterranean is not directly influenced by global events, it is interesting

(and possibly significant) that on this particular case, the same cooling trend is detected both in the Mediterranean and in the Atlantic.

Line 552: Why was it expected that they correlate with d13C of U. peregrina? What is the causality behind?
This passage will be deleted in the revised manuscript. Higher planktonic foraminifera biomasses are observed in this interval, in contrast to ostracod body size, which decrease.

Line 552: "also" in relation to what?
This sentence will be deleted since the bulk data will be moved to the supplement.

Line 555-556: But what where the causes of the higher productivity?
We do not know this, neither for the Western nor for the Eastern Mediterranean. It may be related to the MPT, but within the semi-enclosed setting of the Mediterranean Basin, we could not make this assumption safely.

Line 558: Is it enough to base a regime shift on 4kyrs and only a few samples?
Yes, this is the main advantage of the STARS algorithm, that it allows detection of regime shifts even at the edges of the time series (see also our answer above).

Line 559: How is the EASM related to the study area? Where is the link to the data of the study?
This is an important component of global climate and affects southwestern Europe winter precipitation, and it was enhanced during MIS 18 (Sánchez Goñi et al., 2023). The data of that study is provided by Sanchez Goni et al. We discuss in this section the observed changes in our study area, Lardos, to see if there is any detection of a changes that could have been influenced by this global pattern shift. Our conclusion regarding the interval 760–756 kyr is that the observed regime shift at Lardos (recorded in TOC, $\delta^{18}O_{peregrina}$ and $\delta^{18}O_{ruber}$) are due to the shallowing of the study area, rather than climate.

Line 562: The shifts can be interpreted in what kind?
The shifts to lower TOC but higher $\delta^{18}O_{peregrina}$ and $\delta^{18}O_{ruber}$. However, SST and SSS ranges are similar to the previous interval. Therefore, we interpret these shifts to reflect the shallowing of the study area.

Line 563-564: What is the amplitude in the global records or Mediterranean (if available)?
Unfortunately, the amplitudes of $\delta^{13}C_{peregrina}$ and the ostracod weighted-average body size in the global records are not available to us. At Lardos, $\delta^{13}C_{peregrina}$ ranges from −0.89 (LR16) to −0.3 ‰ (LR10), and the ostracod weighted-average body size (valve width) ranges from 159 to 241 µm.

Line 567: Are these isotope ratios really so easy to interpret?
The isotopic composition of foraminifera generally does reflect that of ambient water. We base here our interpretation on the ecology of the three foraminifera species: *G. ruber* a surface-dwelling planktonic foraminifer, *G. inflata* a deep-dwelling planktonic foraminifer, and *U. peregrina* a benthic foraminifer. Assuming that these ecological traits have not changed, then the isotopic composition of their shells will reflect the oxygen isotopic composition of seawater, which is determined by temperature and salinity, and therefore the thermohaline gradient.

Line 572: What is the interpretation of these observations?
The steeper thermohaline gradient during interglacials means larger difference between surface and deep water temperatures and/or more stratified waters than during glacial periods.

Line 576-577: Not sure what the authors mean here.

Apologies for the confusion. What we mean is that the foraminifera biomass follows the paleodepth changes (although not precisely), but we do not anticipate greater accumulation of shells through transport. If the mechanism was transport, then we would still not have a greater biomass because the paleodepth is greater than the euphotic zone depth at all times. Therefore, we believe that the foraminifera biomass is actually controlled by primary productivity. We will modify this sentence accordingly.

Line 578: Are the ostracod and sponge abundances higher? Or why are they confirming?
Yes, they also increase with foraminifera biomasses (Fig. 4).

Line 583: Not sure if it is safely to assumed.
We rather propose higher secondary production during the interglacials.

Line 586: (and therefore lower δ13C) – could be measurement error?
No. The error in $\delta^{13}C$ is far less than the difference in $\delta^{13}C$.

Line 643: What do you mean with negatively affected?
Planktonic foraminifera biomass decreases during the MIS 20 and MIS 18 glacials.

Line 645: Please also summarize chapters 4.3 and 4.4 here
Of course, we will.

Technical corrections
Line 30: delete recent
Done

Line 33 & 34: add a comma before triggered and after (An et al., 2024)
Done

Line 43: However should be at the beginning of the sentence
Done

Line 46: Western Mediterranean
Done

Line 47: what is this point?
Partly

Line 48: “surface water” and unit is missing
This sentence is to be deleted in the revised manuscript.

Line 53: add comma after consequently
Done

Line 71: Jorissen et al., 1995 should be cited here
Yes, thank you.

Line 74: add comma before including
Done

Line 75: delete sea
Done

Line 107: Figure 1a is not marked in the figure; delete “the”; add s for limestones, replace “substratum is” with “are”

Done

Line 109: add comma after motions
Done

Line 114: How deep is deep-water?
Replaced with “hemipelagic”

Line 114-115: “providing a valuable reference point” – please rephrase
Replaced with “unique opportunity”

Line 119: thickness of samples? And please add more details on how the surface was refreshed.
We employed standard methods for sampling the outcrop. Each sample thickness was 2cm, and the outcrop was refreshed by clearing the first half meter of the surface. We can add this information to the text.

Line 125-129: Sentence too long, please rephrase.
Done

Line 140: and food
Done

Line 142: delete from; add to after counts
We deleted “from the counts” to correct the meaning of the sentence.

Line 142: please add van Hinsbergen et al., 2005
Added

Line 143: replace as with since they are; end of sentence: and depend on food
Done

Line 160: For how long?
Added this to the text

Line 199: … The biomarker analyses followed standard grinding (…) as described by e.g., Besiou et al. (2024)
We refer to earlier work as otherwise it would be a repetition of methodological information that is not new.

Line 210 add a T for SST
Done

Line 212-213: PTFE; HPLC, UHPLC - Please write out
Done

Line 215: what is SbiK-F
That is the Senckenberg Biodiversity Research Center that is the affiliation institute for the co-author Iuliana Vasiliev. We will expand the abbreviation in the text.

Line 220: what Analyst Software? Reference is missing
This is the software that comes with the mass spectrometer to obtain and analyze its outputs. We will add the information and reference in the revised manuscript.

Line 248: delete “-“ in maximum

Done

Line 249: replace was with were
This refers to the typical error

Line 250: replace for with of
We deleted “organic matter for coastal”

Line 257: add parentheses
Added

Line 308: reference for the Shapiro-Wilk-test is missing
Added

Line 314: replace was with measures were
Added “testing”

Line 320: Replace “The above” with “All”
Done

Line 345: remove parentheses
Fixed

Line 360: Add The
Added

Line 361: delete of foraminifera
We want to leave this as is, for clarity

Line 361 -363: Higher/Lower values compared to what?
Compared to the other foraminifera species. We will replace with “highest” and “lowest”

Line 372: Please add a new paragraph for d13C
Added

Line 376: There is a significant…
Added “a”

Line 377: rho and p in italics
Changed here and in subsection 3.9.

Line 413: higher temperatures compared to what?
Replaced with “increased” relative to the previous interval

Line 416: unit is missing
SSS is expressed without a physical unit

Line 417: S is missing
Added

Line 431: TN was not mentioned in the methods
Added

Line 503: replace is with was; Add value for the warmer SST you mention
Added

Line 504: please rephrase a little, “as observed today” is a little confusing
Done

Line 525: stable “isotope” high values…
We mean that the high values are stable.

Line 539: replace in with at
Done

Line 540: Compare with line 528. This is partly repetitive here
We would like to keep both explanations because they refer to different intervals.

Line 546: delete “at Lardos” and at to the end of the sentence.
The syntax is correct as is.

Line 558: and MIS 18 to 19 transition
Yes

Line 573: reference?
We mean here that water has lower thermal conductivity than air, and this is why the ocean water does not heat up quickly. We are not really sure if a reference is needed for this, since it is basic physics. The interpretation that this is the reason why the steeper thermohaline gradient during interglacials is expected, this is our own. Therefore, a reference is not needed.

Line 595: “fluctuate between much more positive…” please rephrase.
We added “same” before values to clarify.

Line 631: delete “here”
Done

Line 632: the Island of Rhodes, not the complete Eastern Mediterranean.
Actually, the study area was under sea, in the Eastern Mediterranean. We reconstructed the marine organisms’ response to paleoceanographic changes in the Eastern Mediterranean sea, not terrestrial conditions.

Line 632-634: This statement should be in the introduction
We have already referred to these previous studies in the Introduction, and here we use them to refresh the context and enter into the Conclusions of the present study. We can remove.

Line 635: time interval equals MPT?
We write “the time interval of the study”, which falls within but does not cover the entire MPT.

Line 637-368: delete “at this time”
Done

Figures: As mentioned above, the authors should consider to make a synthesis figure and add the regime shifts to the plots.
We agree, we will do both.

Figure 1: I would suggest to make to figures, maps and data, and add a map of the Island of Rhodes where the locations of the LBF-outcrops are marked. Add a legend to the lithology. I assume the interval LR23-25 are the corals dated by Titschack et al.? Yes, the reviewer is right the interval LR23-25 yielded two coral fragments dated by Titschack et al. (2013), as

well one coral fragment dated by Agiadi et al. (2023), all using $^{234}U/^{238}U$ radiogenic series. We will also add the requested figure.

Figure 2: those red lines for the regime shift are not visible. For the SSTTEX I would assume lower temperatures during glacials compared to interglacials.
Agreed, but the even in compilation of figure 7, we see that the data are different showing variations withing each warm or cold phase.

Line 459: total
Corrected

Line 460: ontogenic shifts can be excluded?
The $\delta^{13}C$ of the entire otoliths is a proxy of the mean metabolic rate of the fish covering its entire lifetime. To be able to control if the age (and therefore ontogenetic stage) of the fish played a role in the values we obtained, we excluded very young, juvenile specimens, and we also measured the otolith weight, which is plotted as the size of the circle (points in the plots).

Figure 3 and 4: Paleo-water depth. Why are they shown twice?
We show paleodepth in both figures for convenience of the reader, to facilitate our discussion with respect to stable isotopes, TOC, TN and BIT (Fig. 3) and biomasses, ostracods and sponge spicule counts (Fig. 4).

Line 483: Otoliths should also be mentioned.
We do already mention otoliths in the caption (both for the oxygen isotopes and the circle diameters).

Figure 7: A legend is needed here. Are the dashed lines the regime shifts? Why are temperatures above the plot? Unit is missing for SST.
We will replace this figure with the correct version. Apologies for the confusion.

Line 520: based on the colors, there are only three records. Why are there so many mentioned in the captions?
We will replace with the correct version of this figure.