Review of Agiadi et al. "Pelagic ecosystem responses to changes in seawater conditions during the Middle Pleistocene Transition in the Eastern Mediterranean"

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.

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.

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.

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?

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.

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.

The reference list was not checked for completeness. My specific comments and suggestions can be found in the following in the attached pdf:

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
- Line 61: "no consistent global patterns" – related to what? Also, I would not assume global changes in productivity. Please check.
- Line 73: "many areas worldwide" – but only two references are provided?

- 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.
- Chapter 2.1. a map showing the island and the LBF outcrops including Lardos would be helpful here.
- Line 111: The lithology is shown in figure 1 and should also be shortly described in the text.
- Line 117: Is the total section of 30 m all comprising of LBF?
- Line 121: Eichner et al. 2024 should be mentioned here
- chapter 2.2 Paleodepth estimates – Please refer to paleo-water depth.
- Line 131: Calgon normally dissolves lime? Please explain why this step has been made
- Line 132: Has this temperature an effect on the isotope 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.
- Line 145: If the bulk isotope measurements are not presented, this information could be removed?
- Line 157: Why this fraction?
- Line 165: No diagenetic overprint of the otoliths found in the section that could influence the isotopic signal?
- Line 171: This is somewhat a repetition from the sentence before. The references could be added to the sentence before.
- Line 175: The otoliths not the fishes?
- Line 177: MeOH, please explain.
- Line 183: Bulk data?
- Line 192: Data are shown in VPDB, correct?
- Line 240: How can C/N ratios be used to detect changes in sedimentation rates? Please explain.
- 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.
- Line 263: What is the rationale behind the size fractions?
- Line 269: Was A. tepida also used in this study?
- Line 272: Please shortly explain why ostracods and sponge spicules are counted. Used as proxies for what?
- Line 277: I assume you mean planktic forams?
- 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.
- Line 294: Is the adjustment plus or minus?
- Line 298-299: It is the modern relationship? It has not changed through time?
- Line 308-313: These methods are not widely applied and some explanations are needed here.
- Line 314: Why all the correlations are measured here? Some make sense, others possible not.
- 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?

- Line 322: Are you talking about benthic or planktic foraminifera?
- Line 322-323: Are they only representing the thermohaline gradient in the Aegean? And what are these certain assumptions? Please specify.
- 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?
- 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.
- Line 359: Why not show the data in a figure?
- Line 360: Why those values were measured -to support the age model?
- Line 369: Not shown and could possibly be removed from the methods and results?
- Line 376: Why the correlation was measured here? What is the message?
- 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?
- Line 392: Eichner et al. (2024) also refined the age model of Titschack et al. (2013). This should be noted here.
- Line 393: Changes in paleo-water depth could also be related to tectonics.
- 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.
- Line 409: This sentence could be removed. The dataset is mentioned in the "data availability" section
- Chapter 3.5: I wonder whether the TOC values reflect the TOC during deposition. Weathering could be a problem (oxidation of organic matter).
- 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
- Line 490: Why all these correlations?
- Line 440 and 449: Why this differentiation between the size classes? Is this important for the story to tell?
- Line 500: It is only a local signal - this aspect should be highlighted here
- Line 502: There is no data available?
- Line 505: The reader does not know what and where site LC07 is. It should be described, also including water depth.
- 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.

- Line 509-510: What is the system?
- Line 514: Logical break between two sentences. Sapropels were not topic of this paragraph, why mentioning here?
- 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).
- Line 516: Not sure whether time intervals mentioned here agree with the regime shifts shown in the figures.
- 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.
- Line 527: d13C of G. ruber looks like it is more or less the same in MIS 23 and 22.
- Line 529: Are deep sea and shallower habitats comparable so that the authors can use the results from deep sea studies for their study?
- 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.
- Line 531: What is the climate proxy here? d18O? And which kind of the climate system is here referred to?
- Line 535-536: and how is that? Please discuss your results more.
- Line 540: of planktic foraminifera. For benthic it is different
- Line 547: and d13C G. ruber? What does this mean?
- Line 549-550: Are those two locations really comparable like this?
- Line 552: Why was it expected that they correlate with d13C of U. peregrina? What is the causality behind?
- Line 552: "also" in relation to what?
- Line 555-556: But what where the causes of the higher productivity?
- Line 558: Is it enough to base a regime shift on 4kyrs and only a few samples?
- Line 559: How is the EASM related to the study area? Where is the link to the data of the study?
- Line 562: The shifts can be interpreted in what kind?
- Line 563-564: What is the amplitude in the global records or Mediterranean (if available)?
- Line 567: Are these isotope ratios really so easy to interpret?
- Line 572: What is the interpretation of these observations?
- Line 576-577: Not sure what the authors mean here.
- Line 578: Are the ostracod and sponge abundances higher? Or why are they confirming?
  Line 583: Not sure if it is safely to assumed.
- Line 586: (and therefore lower δ13C) – could be measurement error?
- Line 643: What do you mean with negatively affected?
- Line 645: Please also summarize chapters 4.3 and 4.4 here

Technical corrections:

- Line 30: delete recent
- Line 33 & 34: add a comma before triggered and after (An et al., 2024)
- Line 43: However should be at the beginning of the sentence

- Line 46: Western Mediterranean
- Line 47: what is this point?
- Line 48: “surface water” and unit is missing
- Line 53: add comma after consequently
- Line 71: Jorissen et al., 1995 should be cited here
- Line 74: add comma before including
- Line 75: delete sea
- Line 107: Figure 1a is not marked in the figure; delete “the”; add s for limestones, replace “substratum is” with “are”
- Line 109: add comma after motions
- Line 114: How deep is deep-water?
- Lien 114-115: “providing a valuable reference point” – please rephrase
- Line 119: thickness of samples? And please add more details on how the surface was refreshed.
- Line 125-129: Sentence too long, please rephrase.
- Line 140: and food
- Line 142: delete from; add to after counts
- Line 142: please add van Hinsbergen et al., 2005
- Line 143: replace as with since they are; end of sentence: and depend on food
- Line 160: For how long?
- Line 199: … The biomarker analyses followed standard grinding (…) as described by e.g., Besiou et al. (2024)
- Line 210 add a T for SST
- Line 212-213: PTFE; HPLC, UHPLC - Please write out
- Line 215: what is SbiK-F
- Line 220: what Analyst Software? Reference is missing
- Line 248: delete “-“ in maximum
- Line 249: replace was with were
- Line 250: replace for with of
- Line 257: add parentheses
- Line 305: Unit is missing.
- Line 308: reference for the Shapiro-Wilk-test is missing
- Line 314: replace was with measures were
- Line 320: Replace “The above” with “All”
- Line 345: remove parentheses
- Line 360: Add The; ruber in italics
- Line 361: delete of foraminifera
- Line 361 -363: Higher/Lower values compared to what?
- Line 372: Please add a new paragraph for d13C
- Line 376: There is a significant…
- Line 377: rho and p in italics
- Line 413: higher temperatures compared to what?
- Line 416: unit is missing
- Line 417: S is missing
- Line 431: TN was not mentioned in the methods
- Line 503: replace is with was; Add value for the warmer SST you mention
- Line 504: please rephrase a little, “as observed today” is a little confusing
- Line 525: stable “isotope” high values…
- Line 539: replace in with at

- Line 540: Compare with line 528. This is partly repetitive here
- Line 546: delete “at Lardos” and at to the end of the sentence.
- Line 558: and MIS 18 to 19 transition
- Line 573: reference?
- Line 595: “fluctuate between much more positive…” please rephrase.
- Line 631: delete “here”
- Line 632: the Island of Rhodes, not the complete Eastern Mediterranean.
- Line 632-634: This statement should be in the introduction
- Line 635: time interval equals MPT?
- Line 637-368: delete “at this time”

Figures:

- As mentioned above, the authors should consider to make a synthesis figure and add the regime shifts to the plots.
- 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.?
- Figure 2: those red lines for the regime shift are not visible. For the SST_{TEX} I would assume lower temperatures during glacials compared to interglacials.
- Line 459: total
- Line 460: ontogenic shifts can be excluded?
- Figure 3 and 4: Paleo-water depth. Why are they shown twice?
- Line 483: Ooliths should also be mentioned.
- 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.
- Line 520: based on the colors, there are only three records. Why are there so many mentioned in the captions?