Reviewer #2:

The submitted manuscript provided information about the paleoenvironmental changes in marine records in the Eastern Mediterranean basin over the Middle Pleistocene Transition, and in particular between Marine Isotope Stages MIS 23–18. The multiproxy approach used to analyzed the marine record from Rhodes Island allowed the authors to produce SST data as well as changes in productivity signal. Analyses on change in biomass have been used to reconstruct the fish distribution depths during the MPT time interval.

It is clear evident the huge amount of data produced by the authors and it is not easy to combine all the information derived from these proxy records, but I think that the present version of the manuscript is really confused and most of the manuscript is really a description of results and it is completely missing a deep discussion as well as a global view of the MPT problem and the impact on marine ecosystem. In fact, in my opinion, the Middle Pleistocene Transition is not the target on this manuscript. The study record falls well within the changes occurring during the MPT. In the study record is missing the onset of the MPT as well as the end. So that I would like to suggest changing the title of the manuscript.

Correct. The MPT is not the target of the manuscript. The target is the pelagic ecosystem response to changes in seawater conditions. This manuscript does not aim to put emphasis on the here-produced SST, SSS but on the ecosystem response, one step further that the common paleoclimate studies. All data presented here are new: SST, SSS, d18O, d13C, counts. We will amend the title to "Pelagic ecosystem responses to changes in seawater conditions during the Middle Pleistocene in the Eastern Mediterranean" to avoid any confusion.

The manuscript is well written but it is missing a real discussion and in the manuscript the authors consider only part of the data (i.e. they focused on d13C signal and d18O is strongly underestimated).

The $\delta^{18}O$ signal is included in the SSS estimates, not that much in the classical $\delta^{18}O$ interpretation. We do that only because we also provide sea surface temperature data that can further used to constrain the SSS, via the $\delta^{18}O$. But we can provide a more traditional explanation of the $\delta^{18}O$ and the relation to the paleoclimate: temperature in open marine systems vs hydrological budget changes in restricted basins. Most importantly, the $\delta^{18}O$ records are used to reconstruct the fish responses to the SST, SSS, productivity changes. Again, we will make clearer the aim of our work.

Concerning the age model: I have several doubts on the chronology of the study record. The authors referees to the Titschack et al. (2013) and they improve the chronology according to the new stable isotope data. But the proposed chronology is based only on visual correlations of the news stable isotope data with LR04 global (Lisiecki and Raymo, 2005) and Mediterranean (Konijnendijk et al., 2015) benthic δ18O stacks, and with that occurring in the planktonic foraminifera Mediterranean δ18O Medstack (Wang et al., 2010). There are no data on calcareous nannofossils and planktonic foraminifera to support the proposed chronology. In addition, for the Mediterranean planktonic record I would like to suggest to use Grant et al. (2022) recently published for ODP site 967 (eastern Mediterranean). In my opinion it is necessary to improve the chronology of the study record. At present this issue is really weak.

Analyses of calcareous nannofossils were carried out on the studied sedimentary interval by Titschack et al. (2013) and Eichner et al. (2024) and confirmed that the lower part of the Lardos section deposited during the Middle Pleistocene transition. That said, the sedimentary deposits of the lower part of the Lardos section studied here were deposited in the interval MIS23-MIS18, which does not contain calcareous nannofossil nor planktonic foraminiferal calibrated bio-events that could have been used as anchor points for our revised age model. Following the reviewer, in our revision, we will use Grant et al. (2022) $\delta^{18}O$ *G. ruber* record from the eastern Mediterranean to improve the chronology of the studied deposits.

Chapter introduction: the authors report as follows: Such warmer and saltier conditions were last observed in the area in the Late Miocene (Besiou et al., 2024; Kontakiotis et al., 2022; Vasiliev et al., 2019) and it becomes intuitive that seawater conditions during the MPT may have also been very different from those in the Western Mediterranean. This issue is questionable. The Late Miocene is forced by a different climate regime and a possible connection with the MPT is very difficult.
We agree and we will remove this passage and reference to Late Miocene.
We also have to consider that very few studies go for the assessment of coupled SST-SSS and these are concentrated on the Miocene. Even the configuration was different, the Mediterranean was and it still is a semi-enclosed restricted basin.

Subchapter 1 Study area, sampling and chronostratigraphic background: the chronology has to be improved and supported by data. When the authors reported as follows: 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 already studied by Titschack et al. (2013), it is necessary a new figure where it is reported clearly the stratigraphic interval analyzed in this manuscript respect to the published ones.
We agree and will provide, as a supplementary material, a new figure to illustrate where the studied stratigraphic interval is located with respect to the published ones.

Subchapter 2.3 Oxygen and carbon isotope analyses: the stable isotopes on bulk can be removed from this paper. They are not useful. The authors produced stable isotopes on different matrixes
We will remove the bulk data to the supplement.

I do not understand if all the data plotted in the figures of this manuscript have been acquired for this work or derived by a compilation from different sources. Please clarify this issue
All date presented here are new. We will clarify this in the revised manuscript.

Figure 1: The authors proposed in Fig 1 two different figures. Please it is necessary to separate these informations. In figure 1 is missing the location of Montalbano Jonico section.
We will add (a) and (b) for the different panels. Montalbano Jonico is already indicated as "M.J." but we will move the letters to the left so that they are more clearly visible.

Fig 2: in the observing d18O G. ruber, G. inflata as well as in U. peregrina patterns is not clear the position of warm and cold MIS in time domain. I think that there is a problem with chronology. MIS 21 (warm) shows in G. ruber signal a strong cold event, at the base of this MIS, followed by a warm pick event, followed by a cold phase along the MIS21. This pattern is strongly different to the signal reported in the reference curves. Also, the signal in MIS 22 (cold) is the same reported in MIS 23 (warm). How can you separate these MIS? Again, in U. peregrina it is not clear the signature and the signal is flat. Are you sure of the chronology? Please try to control the age model.
The boundaries of the MIS are already provided in the literature, and we do not redefine these here. The Mediterranean signal is much different from that of the global ocean. In addition, there are stadials and interstadials that are not necessarily captured in our record, but this is still a unique record for the Eastern Mediterranean at the highest achievable resolution.

Chapter discussion: Basically, the authors have to write this chapter. I think that it is necessary to specify how the authors define the boundaries of the identified regimes, and it is necessary to discuss seriously what are the climate/oceanographic changes those impact on the different time interval and secondly the impact of these changes on the marine ecosystem.
In my opinion without the quantitative distribution of marine proxy records (i.e., calcareous plankton, ostracods and others) it is very difficult to use the term ecosystem in this manuscript.

Our focus is on the secondary production and the responses of the marine organisms beyond primary producers. In fact fishes form a major component of the marine ecosystem. In addition, we provide non-classic proxies such as ostracod and sponge spicule biomass and body size measures.
Our Discussion at the moment comprises four subsections:
4.1 provides our interpretation of the results regarding regime shifts and the biotic responses to them;
4.2 discusses the thermohaline gradient that comes out of our model;
4.3 examines the responses of the different organisms to the SST, SSS and productivity changes, namely, foraminifera (benthic and planktonic), ostracods, sponges and fishes;
4.4 presents our interpretation of the changes in the lifetime-average depths of the fishes based on the model estimates and what these may mean about the fishes capacity to inject carbon from surface to deep waters.
We can add an introductory paragraph to the Discussion to explain the above structure that will perhaps help.

SubChapter 2 Thermohaline gradient: this subchapter is a descript of results
Our model outputs the thermohaline gradient during glacials and interglacials in the study area. The interpretation is that this is steeper during interglacials because the water in the lower part of the water column does not heat up as fast as the surface water during warm periods. We can explain this further in the revised manuscript.