Reply to Reviewer #2

We thank reviewer #2 for the detailed comments in a very friendly manner. We addressed all points with our answer given in Italics following the original comment.

The authors reported original results using a regional physical-biogeochemical ocean model to study the Mediterranean Sea circulation and biogeochemical cycle during the LGM. The regional model was forced by the data of Earth System model taking into account topography and bathymetry changes, which is highly appreciated. Several interesting findings were shown, in particular the sensitivity test attesting the importance of the sill depth in the Straits of Gibraltar and Sicily that explained a large part of sluggish Mediterranean circulation at the LGM. The authors succeeded in reconciling apparent contradiction of proxy reconstruction showing reduced primary production in the surface and increased organic deposition on seafloor during the LGM by the interplay of water column stratification, zonal advection and slower respiration in glacial cold waters. Also, the potential influence of seasonality changes during the LGM on paleo-temperature reconstruction was discussed.

I enjoyed reading this manuscript. The scientific theme fits perfectly with the topics treated by Climate of the Past and this is the first attempt of simulation of biogeochemical cycle in the Mediterranean Sea under glacial conditions. I wish to see this work published on Climate of the Past.

The manuscript is well structured with clear figures and tables. I am not a modeller and I have only limited comments / suggestions.

My first point is scarce comparison between proxy reconstruction and simulation results. Only SST reconstruction based on planktonic foraminiferal assemblages (Hayes et al., 2005) was used for comparison (Figs 8 and 9). The authors mentioned possible bias of reconstruction due to seasonality changes at the LGM taking alkenone as an example but the comparison between model results and UK'37-SST is not presented except for Fig. 9. I would suggest to add a map presenting the comparison using the data provided by Cacho et al. (2002) and Essallami et al. (2007) by updating the MARGO database used in the present manuscript. It will be interesting to examine whether the sites with large SST offset between UK'37-SST and delta T32 correspond to the areas of marked delta peakiness (Fig. 12).

We updated the data set on UK'37-SST shown in Fig. 9 which is primarily used to evaluate our model results against proxy estimates. However, we refrain from a further assessment between the simulated biological production temperature (T_org) and UK'37-SST. Alkenones are produced by haptophyte algae which have a distinct seasonality affecting the recorded signal (Tierney & Tingley, 2018 ,doi.org/10.1002/2017PA003201). Our simulated primary production does not distinguish between plankton species. Thus, the simulated T_org signal is different to that from proxy data. The focus of our study on T_org is to investigate potential signal differences between T_org and simulated SST which occur in an ideal model framework.

The second point is about remineralization rate of organic matter and dissolved oxygen concentration. Slower remineralization rate in glacial cold water is proposed to increase transfer efficiency of detritus (Teff). Together with cold sea surface that increase oxygen solubility, generally high dissolved oxygen is expected but some cancelation due to sluggish ventilation is possible. Therefore, the distribution of dissolved oxygen concentration is highly interesting but only vertical profiles from the selected areas are shown in Fig. 2. I would like to see transect of dissolved oxygen under different simulation scenarios at least in supplementary information.

For completeness, we included the transect of oxygen in the Appendix Fig A4.

I would suggest to accept this manuscript after minor revision.

Minor/specific comments

Line 28 and throughout the text. “Gibraltar” would be replaced by “the Strait of Gibraltar”.

done

Line 28. “ZOC”. Do you mean “zonal overturning circulation”?

done

Line 33. “Proxy data from foraminiferal shells” should be replaced by “Water mass proxy data recorded in foraminiferal authigenic fraction”.

done

Lines 155-156, about the nutrient supply from shelf areas during glacial sea level low stands. Was the similar effect considered inside of the Med Sea?

We refer here to a sensitivity study by Palastanga etal, 2013 which addressed the impact of additional nutrient supply from continental shelf areas in a global setup. According to their paper, they applied the changed nutrient supply globally, which should include the Med Sea. However, the resolution of their global model is very coarse, i.e. it does not have any ocean points less than 200m which could be considered as shelf areas, and the Mediterranean Sea is only represented by a couple of grid cells – I doubt that they looked at regional results at all. In our setup, potential changes in water-sediment interactions due to sea-level low stands within the Med Sea are considered.

Line 160. About the use of the present-day nutrient concentration for the LGM simulation. I understand the reasoning of the authors but some sensitivity tests by modifying nutrient concentration will be interesting to examine the robustness of the results.

We performed one extreme sensitivity study by prescribing a tenfold nutrient concentration of PO4 and NO3 compared to present day at the western boundary for the LGM simulation of GLAC-1D. Comparing these results with the LGM simulation with PI values at the western boundary, named here LGM-CTL, we see a significant accumulation of nutrient in the upper 200m (higher than 100 times LGM-CTL in the eastern basin). However, net primary production is higher by only up to 12 times LGM-CTL which is a result of a significant higher standing stock of zooplankton (>30 times) that controls phytoplankton concentrations. Below 200m, we find an almost linear response of the nutrients (~ 10 times LGM-CTL values which corresponds to the prescribed perturbation). Detritus also shows 10 times higher LGM-CTL *values down to ~400 m. Below that depth, detritus starts to accumulate because remineralization is slowed down due to the lack of oxygen (less than 5% of the LGM-CTL). At the end of the 1000 years of simulation, the deep basins nearly turned anoxic. It is interesting to find that this extreme, highly unrealistic sensitivity study shows a nearly linear response of the nutrient concentrations to the western boundary at the depth of the LIW and below in the eastern basin. However, we don't see a possibility to provide limits for the open boundary concentrations, besides that ten times higher values produce deep sea anoxic and are therefore very unlikely.*

Lines 185-186, "PEM = precipitation + river runoff - evaporation". The first letter of the words does not correspond to the abbreviation.

We change it to PR-E = precipitation + river runoff - evaporation

Line 187 and throughout the text, "1000 m3 s-1". The authors may use always "Sv" or "mSv" because small fluxes like +0.06-0.076 Sv and +0.04-0.11Sv (line 194) are shown with Sv unit.

We follow the convention to use 1000 $m^3 s^{-1}$ for freshwater fluxes in atmospheric sciences and Sverdrup (Sv) for oceanic fluxes, as is common in oceanography. We introduce Sv when it is used the first time.

Lines 206-207, "slightly weaker...other modelling studies". Add the range of flux obtained by other studies to be more precise.

We provide a range (0.1-0.3Sv) which is taken from the four mentioned references.

Line 212, "Nile damming". Do you mean "the Aswan High Dam in 1964"? Please revise.

Revised.

Lines 233-234, "Hamann et al., 2008" is missing in the list of reference. The authors may use the map provided by Venkatarathnam and Ryan (1971) that presents a detailed distribution of calcium carbonate in the eastern basin sediments.

Thanks for pointing out the missing ref. of Hamann etal 2008. We also included the reference to Venkatarathnam and Ryan (1971).

Line 247, "Kuhlemann et al., 2008". The data provided by this reference is not used for the data model-comparison. Please revise.

It is correct that the data model-comparison which is shown in Fig. 8 is only based on Hayes et al. (2005). We still keep the reference of Kuhlemann et al 2008 to cite a second independent estimate of the overall SST change in the LGM.

Line 253. The authors may cite Fig. 8 in addition to Fig. 9.

Done

Line 260, "Fig. 7d". Isn't it "Fig. 7b"?

Correct. Now changed to 7b.

Lines 304-305. Fig. A1e does not exist. Please revise.

Correct. It is Fig. A1c.

lines 316-317. Why is phosphate concentration higher in the Alboran Sea during the LGM? Is this because of enhanced river discharge?

Yes, the river discharge to the Alboran Sea doubles (triples) in GLAC-1D (ICE-6G) during the LGM. In addition, the maximum of the vertical nutrient profile is shifted to a shallower water depth in the Strait of Gibraltar, which enhances the nutrient availability similar to what we found for the Strait of Sicily (see Fig. 15). The impact of the changed dynamical conditions in the strait on the nutrient distribution is visible for the entire Alboran Sea.

Lines 395-396, about the agreement between high organic matter accumulation and increased abundance of benthic foraminifera sensitive to food supply during the LGM. A map presenting simulated organic matter content in sediment (Fig. 6) and changes benthic foraminifera abundance will be interesting to show the trend.

We include information on the relative change of benthic foraminiferal numbers between LGM and PI from Schmiedl et al (2010,2023) in Fig. 6. Data are only available for the eastern basin and show a relative mean increase of ~7 over all locations (ranging between 0.1 to 30), which is of the same order of magnitude ballpark as our simulated relative increase of the deposition flux.

Line 451, "can not" should be replaced by "cannot".

Done

Fig. 1. It will be helpful to add latitudes and longitudes scale since the white line indicates the zonal transect is used Fig. 13 that is *presented with longitudinal scale.*

We added contour lines of longitude and latitude to Fig.1 for a more convenient comparison of Fig. 13.

Fig. 4. I am curious to see the result of zonal stream function of PI-Straits at least in supplementary information.

We added the zonal stream function of PI-Straits (Fig. A1) to the appendix.

References

Cacho, I., Grimalt, J. O., and Canals, M.: Response of the Western Mediterranean Sea to rapid climatic variability during the last 50,000 years: a molecular biomarker approach, J. MARINE SYST., 33–34, 253-272, 2002.

Essallami, L., Sicre, M. A., Kallel, N., Labeyrie, L., and Siani, G.: Hydrological changes in the Mediterranean Sea over the last 30,000 years, Geochem. Geophys. Geosyst., 8, Q07002, 2007.

Venkatarathnam, K. and Ryan, W. B. F.: Dispersal patterns of clay minerals in the sediments of the eastern Mediterranean Sea, Marine Geology, 11, 261-282, 1971.