Dear Sir/Madam,

We tried to do our best for all the suggestions from the editor and reviewers. All changes in the manuscript were marked. According to comments, you can find the point-to-point replies below.

Best regards,

İsmail Akçay

Reviewer 1

Detailed Comments

Review of the manuscript *Impacts of recent eutrophication and deoxygenation on the sediment biogeochemistry in the Sea of Marmara* , by Akçay et al, submitted to Biogeosciences (egusphere-2025-1255). Second review round

Manuscript overview
The manuscript presents a quite detailed overview of the chemical state of surface waters and sediments (pore water and solid state) with respect to eutrophication in the Marmara Sea. It lists new observational results and puts them into perspective using previously published work and own analysis. As three areas are considered in more detail, each with their own distinct eutrophication status, the manuscript draws both local and regional conclusions, which together form a good overview of the Sea of Marmara and its eutrophication issues. It is determined that in very eutrophied parts the sediment biochemistry is enhancing the problems through feedback of nutrients from the sediments in hypoxic or anoxic conditions.
Review overview

The authors have made changes to their manuscript in a rather minimalistic way, and some language concerns remain, particularly regarding repeated sentences. The figures and tables are same, though minor improvements have been made. My request to show/state whether the sample year was in any way representative for the region has been ignored, possibly due to the lack of a clear baseline in the current shifting climate system. Nevertheless, I feel this should be touched upon more, as all presented measurements and flux estimations are based on a single year of observational evidence. I am glad the authors now at least refer to climate change as another possible cause for increased eutrophication.
In all, I am happy with the current manuscript as it provides a good overview of the fieldwork and observation al findings, and presents crucial validation data for ecosystem models that try to incorporate seabed storage and release of nutrients (which is indeed vital for simulating eutrophication effects). Once these processes are satisfactorily included the models can then be used to test mitigation strategies for remediate actions. Some remaining, mainly linguistic, comments are included below.

Recommendation
Accept after minor revision.

1. Line 20: I agree that the results show this observation (twice as much TOC in the sediments at the hypoxic site than at the oxic site), but am slightly worried that the formulation used now indicates that eutrophication/anoxia is good for carbon storage. Though true, this may not be the message the authors want to give.
R: This sentence has now been deleted from the abstract.

2. Line 60: I'm glad this issue has been included but "*occur at temporal and spatial scales*" is completely meaningless: everything occurs at certain temporal and spatial scales. I would prefer to see an indication of whether and where the authors think this might important, given that most ship traffic (https://www.marinevesseltraffic.com/SEA-OF-MARMARA/ship-traffictracker) seems concentrated in the Northeast part of the basin where the largest eutrophication issues exist.
R: This sentence is revised.

3. Line 74: "*of this phenomenon on ecosystem* functioning*, including* within the *sedimentary system*".
R: It is now corrected.

4. Line 166: I do not agree with the statement that lower Sechi Disc Depth values indicate eutrophication, as no information is provided about suspended particulate matter or CDOM in the area. The decrease in euphotic depth maybe due to coastal erosion, for all we know. The authors should substantiate this conclusion.
R: This sentence is deleted.

5. Line 170: "*observations …* are *in line … pointing out the combined effect*"
R: Correction is done as suggested.

6. Lines 173-175: It is unclear to me whether this observation is from this work or from the listed references.
R: This is clarified now in the revised version.

7. Line 174: "*nutrients from* the *lower layer*"
R: It is corrected.

8. Line 177: don't you mean Fig. 2 here for the Chla values?
R: Corrected; it is Fig. 2.

9. Line 179-182: please rewrite in better English and this sentence would probably improve with being 2 sentences instead of 1.
R: These sentences are condensed and rewritten in the revised version.

10. Line 182: "*Enhanced primary productivity …* has *led to* the *development*"
R: Corrected.

11. Fig 2: Secchi with a capital (as it is a name) and I would still much prefer to see the winter values as well, in a similar figure.

R: It was corrected. Summer distributions were much more pronounced, this is why they were presented.

12. Line 233: "*oxygen*-depleted *core samples*"
R: Corrected.

13. Line 240: "acted *as a source*"
R: Corrected.

14. Line 241: "*by* the *denitrification process*"
R: Corrected.

15. Line 257: "*increasing markedly in winter*", but Table 3 does not contain separate information for summer and winter estimates. I assume the range provided does that, but without information on whether the high value represents winter or summer the remark here is not substantiated.
R: This sentence is revised.

16. Line 303: TOC and TN values for the Southern Marmara Sea look similar to me as those for Çınarcık Basin (might even be slightly higher for TOC at the core top), but are lower than those of İzmit Bay. So I do not see any evidence for the statement that TOC and TN values in the southern Marmara Sea were lower than at the other two sites. Lower than İzmit Bay, for sure, but not lower than those of Çınarcık Basin. Unless the authors have used unpublished results or analysis to come this conclusion, in which case they should state so clearly.
R: These sentences were rewritten.

17. Line 319: "*transport of nutrients* to *the surface layer*"
R: That is corrected.

18. Line 320: '*inflow, human-induced*'
R: That is corrected.

19. Line 340: "*were mainly the result of*"
R: That is corrected.

20. Lines 353-359: I feel this can be stated much more concisely, now it seems like repetition.
R: These sentences are rewritten.

21. Line 372: "*processes caused redistribution … bottom water, leading to*"
R: Corrected.

22. Line 410: "*has been* observed *in* "
R: Corrected.

23. Line 410-414: "*Çınarcık Basin* which*, having higher primary production in terms of Chl-a, resulted in higher*"
R: Corrected.

24. Lines 418-421: repetition of lines 403-406.
R: This sentence is deleted.

Reviewer 2

Detailed Comments

Review of egusphere-2025-1255-manuscript-version2I: Impacts of eutrophication and deoxygenation on the sediment biogeochemistry in the Sea of Marmara by Akçay et al.
This paper presents important new data on the chemical state of the water column and pore waters and solid state geochemistry of sediments, in three areas: Çınarcık Basin, Izmit Bay and the southern Marmara in the Sea of Marmara. Using these data, the authors address to effects of eutrophication and its feedback on the redox conditions and benthic cycling of nutrients from the sediment to bottom waters under hypoxic or anoxic/hypoxic bottom water conditions of the Çınarcık Basin and Izmit Bay and under oxic conditions in the southern Marmara.
Considering the siginifican new data on the water column and recent sediments, I support the publication of this manuscript. However, I have concerns about some of interpretations and conclusions regarding the diagenetic processes, which are listed below:

(1) Very little information are provided on the lithology of cores (i.e. grain-size parameters, total inorganic carbon, colour). Moreover, there is inconsistency between the brown colour reported for the upper part of the cores and the interpretation of anoxic/dyscoxic bottom water depositional conditions, based on the geochemical data of cores in the Çınarcık Basin and İzmit Bay cores.

R: The relevant sentences were rewritten, only TC and TOC values were discussed in the manuscript. Additionally, the general colors of the core samples are now presented.

(2) While cores a given area display different geochemical properties, the interpretations and discussion are generalized for the area as whole. For example, while pore water data for different cores in the İzmit Gulf or Çınarcık Basin display different redox conditions, the reader is given the impression that the deposition took place under anoxic conditions at all sites.

R: Some regions in the İzmit Bay displayed anoxic conditions, but generally Çınarcık Basin has suboxic conditions during the study period. And, some texts in the manuscript are revised according to these important suggestions.

(3) The temporal changes in the SMTZ depth in the Sea of Marmara and their relation to the eutrophication are based on comparison of pore water geochemistry of cores from previous studies from widely different locations in the three areas. In the Sea of Marmara, the SMTZ depth spatially varies from seafloor (0 mbsf) to 7.5 mbsf according to the previous studies (Halbach et al., 2004; Çağatay et al., 2004; Tryom et al., 2010; Ruffine et al., 2018), while the shallowest SMTZ observed the cores of this study is 45 cmbsf in the İzmit Bay core IZ-30 (Fig. 7). Even in this core, the suboxic/sulfate reduction boundary is located at 20 cmbsf, and therefore, none of the multi cores shows evidence of deposition anoxic/suboxic bottom water conditions.

The authors should consider that the SMTZ depth in the Sea of Marmara is controlled not only by the DO content of the bottom waters, but also other factors such upward methane flux and sedimentation rates. The upward methane flux is in turn related to tectonic activity and gas hydrate dissociation, which might be controlled by global warming of the bottom waters and hydrostatic pressure changes). The methane flux in the Çınarcık Basin and İzmit Bay would be locally different, being the highest near the active faults, where the SMTZ occurs at or near the seafloor (see recent studies by Bourry, 2012; Crémier et al. 2012; Çağatay et al. 2018).

R: Thank you for these valuable inputs. There is now a new paragraph in Section 3.4 with some of the references mentioned in the referee comment.

(4) The source of Mg and Ca: The authors consider the source of these elements to be the diagenetic dissolution of minerals in the sediment, but the downward decreasing profiles indicate that the source is the overlying seawater, and that the sink is carbonate deposition, most likely at or near the SMTZ. The source of K is also likely to be seawater and the sink is the clay mineral illite (via adsorption). Please see and cite some published papers on the topic.

R: The downward freshening was mentioned in the manuscript with the related references.

(5) The content of subsection 2.1 is related to the cores and has nothing to do with the heading (The study area).I suggest that the authors merge subsections 2.1 and 2.2 under the materials and methods section.

R: The sections 2.1 and 2.2 were merged.

(6) Terminology: In the text, and in particular subsection 3.3. Sediment Organic Matter Geochemistry, there are some unconventional terms, such as “undisturbed accumulations of particulates”. I wonder if this meant for mass flow deposition, which are mainly caused by seismic activity in the Sea of Marmara. Hence, possible presence of mass flow units in the studied multicores should be considered, especially in cores from the Çınarcık Basin and central basin of the Izmit Bay, where such units resulting from the recent earthquakes (e.g. 1999 Izmit Eartquake) have been previously reported (Sarı and Çağatay, 2006; Çağatay et al., 2012; Drab et al., 2015; Arslan Kaya, et al., 2022). This again bring out the importance of detailed lithological core descriptions.

R: The statement “undisturbed accumulations of particulates” was removed from the manuscript.

(7) The manuscript needs some improvement to the English language. For consistency, please replace “Marmara Sea” with “Sea of Marmara” (as in the title)

The above concerns and other details are marked in the annotated pdf file of the manuscript. I hope the authors find the suggestions and comments useful for revision of this interesting manuscript.

R: The language of the manuscript was improved, and “Marmara Sea” was replaced by “Sea of Marmara” for the whole text.

References

Arslan Kaya, et al. 2022. The effects of the 1999 Gölcük earthquake (Mw 7.4) on trace element contamination of core sediments from İzmit Gulf, Turkey. Natural Hazards. https://doi.org/10.1007/s11069-022-05717-w.

Bourry, et al. 2009. Free gas and gas hydrates from the Sea of Marmara, Turkey: Chemical and structural characterization. Chemical Geology, 264;197–206.

Çağatay, et al., 2012. Sedimentary earthquake records in the İzmit Gulf, Sea of Marmara, Turkey. Sedimentary Geology, 282:347-359.
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Crémière, et al., 2012. Methane-derived authigenic carbonates along the North Anatolian fault system in the Sea of Marmara (Turkey). Deep Sea Research Part I: Oceanographic Research Papers 66: 114-130.
Drab, L., Hubert-Ferrari, A., Schmidt, S., Martinez, P., Carlut, J., El Ouahabi, M., (2015). Submarine earthquake history of the Çınarcık segment of the North Anatolian Fault inthe Marmara Sea, Turkey. Bull. Seismol. Soc. Am. 105 (2a), 05 (2A), pp.622- 645.
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Jørgensen et al., 2004) Geochim. Cosmochim. Acta: Anaerobic methane oxidation and a deep H2S sink generate isotopically heavy sulfides in Black Sea sediments. Geochimica et Cosmochimica Acta, 68: 2095-2118
Lyons, T.W., 1997. Sulfur isotopic trends and pathways of iron sulfide formation in upper Holocene sediments of the anoxic Black Sea. Geochimica et Cosmochimica Acta, 61: 3367-3382.
Sarı, E., & Çağatay, M. N.,2006. Turbidites and their association with past earthquakes in the deep Çınarcık Basin of the Marmara Sea. Geo-Marine Letters, 26, 69-76.

R: Thank you for these suggestions – we have now cited three of them in the revised text and added them to the citation list.