

**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 observational 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.

## Detailed Comments

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.
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-traffic-tracker>) seems concentrated in the Northeast part of the basin where the largest eutrophication issues exist.
3. Line 74: "*of this phenomenon on ecosystem functioning, including within the sedimentary system*".

4. Line 166: I do not agree with the statement that lower Secchi 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.
5. Line 170: *"observations ... are in line ... pointing out the combined effect"*
6. Lines 173-175: It is unclear to me whether this observation is from this work or from the listed references.
7. Line 174: *"nutrients from the lower layer"*
8. Line 177: don't you mean Fig. 2 here for the Chla values?
9. Line 179-182: please rewrite in better English and this sentence would probably improve with being 2 sentences instead of 1.
10. Line 182: *"Enhanced primary productivity ... has led to the development"*
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.
12. Line 233: *"oxygen-depleted core samples"*
13. Line 240: *"acted as a source"*
14. Line 241: *"by the denitrification process"*
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.
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.
17. Line 319: *"transport of nutrients to the surface layer"*
18. Line 320: *"inflow, human-induced"*
19. Line 340: *"were mainly the result of"*
20. Lines 353-359: I feel this can be stated much more concisely, now it seems like repetition.
21. Line 372: *"processes caused redistribution ... bottom water, leading to"*
22. Line 410: *"has been observed in "*
23. Line 410-414: *"Çınarcık Basin which, having higher primary production in terms of Chl-a, resulted in higher"*
24. Lines 418-421: repetition of lines 403-406.