

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).

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 manuscript is generally well written and very comprehensive, both in the inclusion of relevant prior work and previous observational evidence. The described mechanisms for eutrophication enhancement are not new in themselves but have not been shown before to play a role in this area. As such, the manuscript provides a good overview of the area and its eutrophication status, some of its local governing mechanisms and thus allows for more detailed management of the area with respect to related water quality issues. In this respect I do miss some discussion on natural occurring eutrophication (given the local topography I would expect some in the deep basins), the physical controls of the area in terms of nutrient mixing and the expected future changes in eutrophication due to these factors and the sedimentary processes. For me, this would put the presented results more in context, which can help prioritise possible management actions. Although I fully acknowledge that the presented work is a detailed scientific study that does not aim to provide a basis for marine policy. Detailed comments are provided below.

Recommendation

Moderate revision, some graphs could be adjusted and text should added but no new figures or analysis are needed.

Detailed Comments

1. Line 47: a reference to figure 1 would be helpful here.
2. Line 51: “The *Marmara Sea ecosystem*”
3. Line 59: with a permanent pycnocline at 15-20 m I would like to know what the effect of shipping is in the region, as the whole area is a busy thoroughfare (as far as I know) and ship wakes can reach to these depths (e.g. Nylund et al, 2020). Does the traffic volume affect the pycnocline and vertical mixing?
4. Line 60: “Thus a major oxygen source”
5. Section 2.1: can the authors say anything about how representative the year 2019 was for the area? And I miss a description of the sediments themselves here: are they sandy or muddy, is the medium grain size known, are there differences between the three areas in terms of sea bed composition?

6. Line 97: "*CTD probe that was coupled to*"
7. Line 131: "*indication a significant fraction of the TN pool*"
8. Lines 232-236: please rephrase, too long and grammatically incorrect.
9. Line 237: "*In all sites*"
10. Line 239/241: "*Porewater diffusive PO₄, NH₄ and Si fluxes*" is repetitive, please rephrase
11. Line 244: are the increases in the diffusive fluxes of reactive iron and manganese derived from figure 7 or from unpublished results?
12. Line 246: "*mixing across the basin, intensifying during winter*"
13. Line 248: "*The correlation between*", I see no correlation parameters provided anywhere for this statement.
14. Figure 5: please provide a 0 line for the NO_x plots
15. Line 269: if these results are not shown please state so clearly, otherwise indicate the relevant table or figure.
16. Line 274: "*core samples and decreased*"
17. Line 280: "*İzmit Bay*", and they had higher TOC levels compared to what? Previous studies, or the third region under study?
18. Line 285: "*results further show*"
19. Lines 285-287: I don't quite see the evidence for this statement as the manuscript does not contain accumulation rates or previous results for İzmit Bay.
20. Line 287: "*results also report*"
21. Line 294: "*results altogether show*" and it seems only stations IZ-30 and IZ-2 have elevated levels. Of TOC and TN, not the other stations in the same bay. Also please provide a reference for the Baltic statement
22. Line 300: I see no difference in figure 6 between the TOC and TN levels of southern Marmara and Çınarcık Basin
23. Line 304: "*caused accumulation of excess amounts of*"
24. Figure 7: the markers for stations IZ-2 and 8 are practically identical
25. Line 336: "*nitrogen (NO₃) through denitrification*"
26. Line 343: what is N.A.F.?
27. Line 355: "*core samples in this study were taken by*"
28. Line 377: the figure 7 results for Mg seem the same everywhere?
29. Line 390: as there are no observational data for primary production anywhere in this manuscript (only Chla) I suggest the authors rephrase this statement
30. Line 393/396: the manuscript has not introduced actual sedimentation rates before, so this information should be presented in the discussion. No new evidence should be included in the conclusions.
31. Line 396-400: overly long sentence making it unclear what the "*respectively*" actually refers to. I suspect it refers to the areas (Çınarcık Basin/İzmit Bay vs southern Marmara Sea) but on initial reading it seems linked to sink and source.

32. Line 411: as this refers to primary production I would assume that the process of higher nutrient fluxes to the deep water is gravity driven, not gradient (diffusive) driven
33. Lines 428-431: as states way above I would like to see some perspective for this, in the discussion section. How important is this process compared to for instance climate change impacts on nutrient inputs, increased stratification, etc.? And is there any natural (background) eutrophication occurring in this region? It doesn't have to be long or extensive, but as marine management is touched on here I think it is relevant.

References

Nylund, A. T., Arneborg, L., Tengberg, A., Mallast, U., & Hassellöv, I. M. (2020). In situ observations of turbulent ship wakes and their potential implications for vertical mixing. *Ocean Science Discussions*, 2020, 1-29.