Review of the manuscript *Improved understanding of eutrophication trends, indicators and problem areas using machine learning*, by Deep S. Banerjee and Jozef Skákala, submitted to **Biogeosciences**

Manuscript overview

The manuscript presents a machine-learning based method to create 3D fields (surface, time) from sparse surface observations of marine nitrate. The authors subsequently compare the predictive capability of the new method with those of an ecosystem model simulation available on Copernicus and with the remaining observations. They conclude that the new method of generating time-dependent surface fields of nitrate is better at predicting the selected variable than the chosen ecosystem model, and as such deem this method a good option to complement sparse assimilation data in order to improve said ecosystem model forecasts.

Review overview

The manuscript is generally well written, but starts with 3 claims in the abstract with which I do not agree. Just because an area is nitrate limited does not make it prone to eutrophication, and both the general trend of marine nitrate in NWES and the weak link between winter nutrients and spring bloom intensity have been reported on before and are not new. This does not mean that the authors should not report these findings, but for me they do not represent results worthy of listing in the abstract. I feel the main objective is to create a 3D field (horizontal, time) for assimilation into the operational model to improve forecasting, which is a worthy aim.

But this is not stated as the main aim, and therefore I feel that the manuscript lacks a clear objective and a transparent narrative. It also does not seem to include all necessary figures and references. The authors refer in many cases to supplementary materials which I could not find on EGUsphere, so I haven't assessed them. But a manuscript should be readable without the background information included in supplementary materials. The authors do not make it clear enough in the text when each model is used (the reanalysis product or the machine learning model). The method itself has merit, and could be a valuable addition to sparse observational data, but there is no mention of (or comparison to) methods to derive nitrate surface values from satellite observations. It may perform better than the applied ecosystem model, but that is but one model and doesn't necessarily mean the method is better than what process-based models can do. Nevertheless, it is remarkable that a machine learning method without direct advective processes performs better than a full 3D hydro-biogeochemical model in representing dissolved nutrient patterns. There a lessons here for ecosystem model developers I feel.

Detailed comments are provided below. I missed references to the more recent OSPAR assessments, and noted some small linguistic issues.

Recommendation

Major revision. I would want to see the manuscript again.

Detailed Comments

1. Line 3: "nitrate observations are difficult to obtain", do the authors mean that it is difficult to find the data or that it is difficult to measure? In any case, I would argue that the observational data is not difficult to find, nor is it particularly difficult to measure once a sample has been

taken, but that observational data is sparse due to funding cuts to monitoring programs, leading to a strong coastal bias of existing observations.

2. Line 8: this sentence indicates that questions will be presented that the new nitrate fields can answer which apparently existing methods cannot. But this is followed by three statements, not questions.

3. Lines 8-14: these three statements are relatively detailed, and are not repeated with the same detail in the conclusions. I would argue that the abstract should reflect the subsequent manuscript in what is deemed important and what not, and thus reflect the presented conclusions. Most detail should be included in the relevant sections, followed by the conclusions, followed by the abstract.

4. Lines 7-9: the first statement seems odd, as nutrient enrichment alone is not enough for an area to be classified as a problem area under OSPAR's Common Procedure (OSPAR, 2022). As such, the OSPAR problem areas do not necessarily indicate areas where winter nitrate concentrations are above threshold levels (areas with direct or indirect eutrophication effects without nutrient enrichment can also be classified as problem areas, while areas with nutrient enrichment but no direct or indirect effects can be classified as non-problem areas). Nor do they indicate areas where nitrate is the limiting nutrient. The assertion therefore seems far-fetched to me. Particularly as most coastal areas are prone to eutrophication issues given sub-optimal policy.

5. Lines 11-12: the second statement lists bi-decadal trends in nitrate that respond to policy-driven reductions in riverine inputs. But the riverine reductions have been extensive reported by OSPAR and others (see e.g. Axe, 2022), all that the presented new fields do is follow the nutrient input trends encapsulated by the marine observations. Therefore, not specifically anything new. The statement can be used to bolster confidence in the presented nitrate fields, but this does not merit mention in the abstract. This may be partly due to the fact that the authors fail to acknowledge the most recent OSPAR quality status report (OSPAR, 2023) anywhere in the manuscript.

6. Lines 12-14: the third statement in the abstract indicates a weak link between winter nutrient levels and spring bloom intensity. Again, this is nothing new, the same was reported for this area by van Leeuwen et al (2023) as being very model dependent. The fact that an NN model could reproduce this would be worth mentioning, except that it was the re-analsyis model product that yielded this conclusion. Which is to say, this particular re-analysis model (NEMO-ERSEM) does not have a clear link between winter DIN and Chla, other models can.

7. Lines 32-33: there is no reference provided for the claim that nitrate monitoring and predicting provides an essential management tool. Most monitoring programs focus on more than just nitrate.

8. Line 35: "*Thames, Rhine and Loire*", a bit surprised to see the Thames in this small list. In terms of discharge the Thames is quite small compared to other rivers influencing NWES, e.g. the Seine, Elbe, Meuse, Humber, Weser. See for instance OSAPAR (2023) and Sonesten et al, 2022, their Table 1.5.1: the Thames is not even included here.

9. Line 37: "*play another a role*", should be plays as exchange is singular.

10. Line 39: "*German Bights*", I only know one German Bight.

11. Line 39-41: there is no reference for the claim that certain areas of NWES experienced increases of land-based nutrient inputs during the 1980's. The assertion need to be validated as in general nutrient inputs started declining during the 1980's following implementation of eutrophication policy during that decade. See for instance Radach & Pätsch (2007) (their figure 4), Soetaert et al (2006) (their figure 6F) and Lenhart et al (2010) (their figure 3).

12. Line 43: a reference to the latest nutrient input reports from OSPAR would help here (Axe et al, 2022 or Sonesten et al, 2022).
13. Lines 45-48: this seems to me to be the main objective of the manuscript, yet this is not reported as such in the abstract. It is mentioned in the conclusions, though.
14. Lines 45-48: Why are satellite nutrient algorithms not mentioned/used? Like Yu et al (2021), Durairaj et al (2015) and especially Chen et al (2023)? I can imagine the proposed methods having advantages over satellite estimates of surface nitrate. This should be discussed.
15. Line 55: "*This is to our knowledge by far the most complete*"
16. Lines 57-59: again, these are not questions and grammatically the sentence is severely impaired. Please rephrase.
17. Lines 85-90: again, why are satellite-based products of surface nitrate not mentioned at all?
18. Lines 90-91: naturally, this is what assimilating data into operational models does.
19. Lines 93-94: please specify exactly which products were used. Now that information is only provided in the acknowledgement.
20. Lines 97: in general the authors cite old references where appropriate, which I fully support. But here the ERSEM model is cited without reference to the first publications of the model, i.e. Baretta et al (1995). I understand the used model will be different from the one from 1995, but credit needs to be given where credit is due. The cited reference may describe the applied model, but it did not create it from scratch I imagine.
21. Line 99: "*into the model*", I would suggest a clear delineation between the applied biogeochemical model, the NN model (same as the ML model) and observations. In several places the word "model" is used without specifying which one is meant.
22. Lines 107-112: I miss an address for the Global River Discharge Database and a clear source of the riverine data used. As far as I can see, the provided reference does not give access to the used data (though a link is specified in the data statement) so it is unclear what kind of update was applied. If atmospheric deposition of nutrients was not considered then please state so clearly. And given the references to OSPAR throughout the manuscript, why not use the riverine database as presented in OSPAR (2022-895) and Van Leeuwen et al (2023)?
23. Lines 116-117: I can understand the limitations, but several studies have shown that riverine nutrients do not behave like this, as they are transported by the currents and usually form coastal river plumes. See for instance Lenhart & Große (2018), Painting et al (2013) for the (simulated) marine footprint of North Sea rivers. I think this merits more attention as it is likely the reason why the NN model is less adept at representing coastal areas, which is exactly where eutrophication issues are most prevalent.
24. Line 124: atmospheric deposition is listed here as an important source, yet is not included, correct?
25. Line 130: "*were considered at the same times than the predicted nitrate*", you mean the same times as the predicted nitrate?
26. Line 131: Please explain what SHAP stands for
27. Line 131-132: that the structural input features are the most important is not surprising, given that they determine the physical circulation missing from the NN model.
28. Figure 1: why are there no river input points in Denmark? They may not have very large rivers but loads can be high due to high agricultural run-off.

29. Line 150-155: all the used long-term time series of nitrate are from the UK, is there a specific reason for this? Were there no other observational point sources with high enough temporal resolution?

30. Line 160: just to be clear, any data assimilation products that we were used in the re-analysis are used here as inputs for the NN model, but the re-analysis product itself is not used in that way. Correct?

31. Eq. 2: In equation 1 there was NN, Rean and Obs, now there is suddenly a Mod. Please use consistent naming, the Mod is obviously a model but which one? The NN or the one that created the re-analysis product?

32. Lines 174-177: The sentence is too long and grammatically wrong. Splitting it would help readability. And please remove "always", you simply mean all coastal stations.

33. Line 193: "*whereas* it *struggles to capture*"

34. Lines 203-206/Fig.3: It is obvious that the re-analysis model product is better at capturing the advective processes that govern coastal dispersion of dissolved riverine nutrients. As these are predominantly the areas that are designated as eutrophication problem areas by OSPAR I do not quite see how this NN model is better in identifying areas prone to eutrophication any better than the re-analysis model. And please explain which seasonal delineation was used in the main text.

35. Fig 5: This would be more illuminating as difference graphs.

36. Lines 206-207: again it is mentioned that the effective temporal resolution of the NN product is around 15 days. If this is so important then please include the relevant graphs in the main text. And given the fact that the product is aimed to be used as extra assimilation data for the re-analysis model, isn't 15 days a bit much for an operational setting?

37. Line 213: "*is used by OSPAR (in combination with other parameters) in its Common Procedure as an important indicator … (Axe, Topu, OSPAR).*"

38. Line 212: OSPAR uses elevated levels of winter DIN or DIP as a eutrophication indicator (OSPAR, 2022), it does not use particulate inorganic N and as such no total inorganic N.

39. Line 216: In the description of Figure 6 it should be made clear that this is the re-analysis product, not the NN model product. The finding that there is no obvious correlation between the winter dissolved nutrient concentration and spring bloom intensity is not new, and was also reported by Van Leeuwen et al (2023) for NWES. But the abstract repeats this claim in the context of the NN model, and indicates the NN model results gave rise to this results. Which it didn't, it was the re-analysis product that did.

40. Line 234-239: I do not agree. Most marine areas are either N or P limited, and introducing an excess of nutrients can always induce excessive growth in waters without light limitation. But that is not equivalent to eutrophication, and just being nutrient limited does not make an area prone to eutrophication (by that definition almost all oceanic waters would be eutrophication prone). For me, this is taking the presented work out of context. The achieved reductions in riverine phosphates have made the North Sea coastal zone in general P limited, not N limited (Philippart et al. 2007; Loebl et al. 2009; Burson et al, 2016; Grosse et al, 2017), which is also supported by several modelling studies (Skogen et al,2004; Lenhart et al, 2010). The mentioned areas are nitrate limited according to the reanalysis model, not the new NN model method, and correlations with other possible limiting factors are not shown. All mentioned "prone" areas are low in population density, have relatively low nutrient loads (according to fig. 1 except for Cork) and unspecified underwater light conditions. They also show no nitrate trend in Figure 7. Local eutrophication issues are definitely possible, and several small areas along the mentioned coastlines have indeed been classified as problem areas (Axe et al, 2017)

by OSPAR in the past. But to state that large parts of the Irish Sea, Scottish coastline and southern Ireland coastline are prone to eutrophication is a step too far for me. At least, I have seen no evidence for that claim in the presented manuscript.

41. Line 240, Fig 7: please make it clear that after Figure 6, which showed reanalysis results, we are again looking at NN model results.
42. Line 250: "*over* a *22 year period*"
43. Line 252: please remove the claim of having found areas potentially vulnerable to eutrophication. Finding nitrate limited regions in itself is also not particularly new, though worthy of reporting. It adds to the available information of which areas might be limited by which factors. But this refers to figure 6, which was the results of the reanalysis product, so I don't see why the authors state that the new product (the NN model) was used to find nitrate limited areas.
44. Lines 254-255: for me this is main point of the presented method, and it would be interesting to see the results. Particularly as the new assimilation fields do not include advective transport and the model used for the reanalysis does.
45. Fig. 7: please add clearly that the time series is from the NN model.

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