We would like to sincerely thank the reviewer for their hugely valuable and detailed review that provided us with a lot of material and thoughts based on which we can improve our manuscript. We agree with all the major points by the reviewer and already invested substantial time to do the recommended changes. We would also like to apologize that the Supporting Information was not available to the reviewer, it was uploaded by us to the same repository than the main text, but due to the technical issues with the repository it took very long time until they made it available to the public. We believe it should be now accessible, but if there are still issues, we are happy to send the document separately. In our response below, we list all the reviewer specific comments in red, our responses in black and the suggested changes in blue font.

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.

We thank the reviewer for this comment, we will change the wording to

"nitrate observations on the NWES are costly to obtain"

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.

This will be now changed to

"With such a data-set we are able to demonstrate the following:"

followed by the statements.

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.

We thank the reviewer for this comment, we agree and have now expanded the Conclusions section by the following text:

"The areas of strong ecosystem response to nutrient loads were identified as south-east of the North Sea, coastline of Brittany and the bay of Biscay (areas previously marked by OSPAR as eutrophication-problem, Axe et al. (2017); Devlin et al. (2023)), but also additional coastal areas were identified in the south of Ireland, eastern Scottish coastline and the Irish Sea. We have found that nitrate trends in the last two decades were mostly minor, with exception of coastline in Bay of Biscay. This confirms recent observations by Axe et al. (2017, 2022); Devlin et al. (2023). We have also demonstrated that winter nitrate is only a limited predictor of the growth next season, which again supports recent findings of Van Leeuwen et al. (2023)."

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.

We acknowledge this is a major point by the reviewer (emphasized also later on in their review, in the point 40). We sincerely thank the reviewer for raising this issue, we mostly agree with the reviewer's points of view, and suggest to make changes and corrections accordingly. We would like to clarify our current point of view:

1. The large positive correlation between Summer nitrate and chlorophyll demonstrates that growth is in the Summer either strongly limited by nitrate, or a driver positively correlated with the nitrate. The only other driver fulfilling this condition that one can reasonably think of is another nutrient, e.g. phosphate. This means one can conclude that such correlation potentially highlights the areas which are more strongly nutrient (or even nitrate) limited than others.

2. In such areas we conclude that growth can be likely more "explosive" in the Summer compared to other areas, if there is sudden occurrence of the limiting nutrient.

3. In the coastal zone such occurrence can appear through riverine deposition e.g. from agriculture.

4. This means Fig.6 contains some important information potentially related to the risk of eutrophication, however such risk does not depend only on the nutrient-limitation, but also depends, in a completely essential way, on the likelihood that nutrient pollution happens in the given area (e.g. total river outflow affecting the area, socio-economic factors).

This means we fully agree with the reviewer that our statements have been pushed too far and we now suggest to carefully reformulate our points in the Abstract as follows:

"With such a data-set we are able to: (i) highlight coastal regions that show strong Summer nutrient limitation, covering eutrophication-problem areas identified by the monitoring bodies (i.e. OSPAR), but also other regions, such as southern Irish coastline and parts of Irish Sea. Our results could indicate greater potential for eutrophication events in those regions subject to high riverine nutrient discharge scenarios."

We believe that these statements are sufficiently precise and they will be further detailed in the Discussion section (see our response to the reviewer point 40).

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.

We thank the reviewer for those comments. We agree that there have been recently published papers and reports that reported similar, or related results. We will make sure these papers/reports are cited and mentioned. However, we do believe that this additional evidence provided through our methods is worth mentioning in the abstract. To make sure that the link to the existing literature is provided, we suggest to add the following sentence to the Abstract (also related to reviewer point 6):

"The last two results are consistent with recent findings in the literature (Axe et al., 2022; Devlin et al., 2023; Van Leeuwen et al., 2023)."

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.

This seems to be a major misunderstanding between us and the reviewer (also see reviewer point 39): the winter nitrate correlated with the spring chlorophyll has been predicted by our NN model. The link of the NN model to the reanalysis is only that it has used as inputs some variables from the reanalysis. The correlation has been therefore calculated between the NN-predicted winter nitrate and the spring chlorophyll from the Copernicus reanalysis. In both nitrate and chlorophyll cases the data are supposed to be the best representation of the biogeochemistry variables – the chlorophyll reanalysis acts as extrapolation of the OC chlorophyll satellite data, as explained in the Section 2.2. We take a full responsibility for this misunderstanding as we have indeed completely failed to mention where the nitrate values came from. We will suggest to emphasize this in the caption of Fig.6 by adding "NN-predicted" in front of "nitrate" wherever it occurs and also emphasize everywhere that the chlorophyll is from the Copernicus reanalysis. We will also make sure we will cite in the Abstract the Van Leeuwen et al, 2023 paper in connection with this result, as suggested in our response to reviewer point 5.

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.

We suggest to rephrase this sentence to

"Eutrophication is a fundamental problem in many shelf sea and coastal areas (Rabalais et al., 2009), where nitrate monitoring and predicting, along with other indicators (e.g. chlorophyll, dissolved oxygen, phytoplankton species), provides an essential tool informing marine management and policy."

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.

We would like to thank the reviewer for this comment, we suggest to change this sentence to

"An important region, subject to eutrophication, is the North-West European Shelf (NWES). NWES is impacted by significant river inputs, such as Elbe, Rhine, Loire, Seine, Scheldt, Meuse, Humber, Weser or Thames which introduce substantial freshwater and nutrients into the region, influencing salinity and water properties (Sonesten et al., 2022)."

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

Thank you we will correct this.

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

Thank you we will correct this.

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).

Thank you, we suggest to phrase the text as follows:

"Until the 1980s, the NWES, particularly near the German Bight and the Westerschelde estuary, experienced notable shifts in nutrient distribution, primarily driven by increased continental nutrient inputs. Riverine discharges, particularly from the Rhine and Elbe, have been identified as major contributors to nutrient dynamics in the region (Brockmann and Eberlein,1986; Radach, 1992), having adverse effects on the local ecosystem. However, EU regulations following OSPAR convention in 1992 substantially decreased the nitrate deposition into the NWES (Soetaert et al., 2006; Radach and Pätsch, 2007; Lenhart et al., 2010a; Burson et al., 2016a; Axe et al., 2022; Sonesten et al., 2022)."

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).

Thank you, this will be addressed by the answer to your point 11.

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.

It is one of the key future applications of this nitrate data-set. We agree with the reviewer that this application can be mentioned in the abstract and we suggest to add to the end of abstract the following sentence:

"We propose to use the nitrate data-set for data assimilation and hypothesise that it has the potential to substantially improve phytoplankton forecast in the operational run."

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.

We apologize for this omission. It is true that satellite nitrate algorithm do exist, but these are either global, or have been developed for different regions than NWES (Chen et al, 2023). Also please note that these algorithms are mostly based on simpler statistical approaches (e.g. polynomial fits) than our machine learning approach. We will make sure that we mention the satellite algorithms and cite all the most relevant papers. We suggest to add the following to the paper:

"There is existing work on developing statistical algorithms to derive nitrate from the satellite (Durairaj et al., 2015; Chen et al., 2023), but these have so far been developed either for the global open ocean, or for other regions than NWES (such as different regions in Asia, or California, Yu et al., 2021; Chen et al., 2023), and those algorithms are unlikely to work for NWES."

15. Line 55: "This is to our knowledge by far the most complete"

Thank you, we will address this.

16. Lines 57-59: again, these are not questions and grammatically the sentence is severely impaired. Please rephrase.

Thank you, we will address this.

17. Lines 85-90: again, why are satellite-based products of surface nitrate not mentioned at all?

Thank you, here we were talking only about NWES, but we did not emphasize this. We will make sure we will emphasize it, i.e. by adding "NWES-based" in front of the observations.

18. Lines 90-91: naturally, this is what assimilating data into operational models does.

Thank you for the comment. The reviewer points are obviously correct, what we however wanted to highlight is that not only DA moves the model forecast towards the observations (an essential requirement for DA to reduce uncertainty in the state estimate), but the reanalysis ends up extremely close to the assimilated observations. This has been discussed in many papers (Skakala et al, 2018, 2020, 2021, 2022, 2024) and is probably a result of the DA set-up we are using (lacking observational error cross-correlations, and flow-dependent background variances). In this sense using the reanalysis near the observational location effectively amounts to using the observations themselves.

19. Lines 93-94: please specify exactly which products were used. Now that information is only provided in the acknowledgement.

Thank you, we are happy to do that adding the project identifiers (NWSHELF_MULTIYEAR_BGC_004_011 and NWSHELF_MULTIYEAR-_PHY-105_004_009).

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.

Thank you, we are very happy to add the reference as well as all the other references mentioned by the reviewer.

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.

Thank you, yes we will make sure we will properly distinguish between the different models.

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)?

We have used the established river dataset used for the UKMO NWES biogeochemistry reanalysis. These data originate from the updated version of Lenhart et al. (2010b) dataset, combined (as we say in the paper) with climatology of daily discharge data from the Global River Discharge Data Base (Vorosmarty et al., 2000) and from data prepared by the Centre for Ecology and Hydrology as used by Young and Holt (2007). As suggested by the reviewer, there are indeed more recent updates to the river data (e.g from https://dataverse.nioz.nl/file.xhtml?fileId=2507&version=1.0), which could be hypothetically considered as well, but we suspect the overall impact of this update on the NN model performance would not be substantial. This is because the riverine values impact only very small areas (true, the most important ones from eutrophication point of view) and substantial part of the potential information carried by rivers is also carried by other input variables, such as the sea surface chlorophyll. We will be however happy to recognize that newer riverine products could be used and mention this among the potential future improvements.

Finally, yes, the direct atmospheric deposition of nutrients was not used and we will mention it explicitly in the revision of the paper, as proposed by the reviewer.

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.

Thank you, we will add text to better emphasize this point including the references mentioned by the reviewer. We would like to add the following sentence:

“However, such scheme is clearly a major simplification for the real river impact (e.g. Painting et al. (2013); Lenhart and Große (2018)) and should be ideally improved upon in the future work.”

24. Line 124: atmospheric deposition is listed here as an important source, yet is not included, Correct?

Yes, the atmospheric deposition of nitrogen is not directly included, it is only indirectly represented through some of the other inputs (e.g. atmospheric variables such as wind speed). Again, this will be emphasized in the new version of the manuscript.

25. Line 130: “were considered at the same times than the predicted nitrate”, you mean the same times as the predicted nitrate?

Yes, thank you, we will correct this.

26. Line 131: Please explain what SHAP stands for

Yes, we will: it is “Shappley Additive exPlanations” .

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.

Yes, we fully agree with the reviewer.

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.

The river data-set used in the reanalysis has been based on several careful considerations about the quality of the available data and their uncertainties. Some smaller rivers in this area have been indeed left out due to insufficient data availability.

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?

It is hard to identify and collect relevant time-series data from observing stations outside of large data repositories. We were aware of our L4 (long) time-series nitrate data and were subsequently made aware of the 5 locations near Scotland providing another reliable nitrate time-series. Given that we have used large number of ICES data spread across the domain to independently test the model, we have found additional time-series from 6 different coastal locations sufficient, even though all of them are near the British coastline.

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?

We have used as inputs into the NN model: 1. the structural inputs (coordinates, bathymetry..), 2. the ERA-5 atmospheric inputs forcing the ocean reanalysis, 3. the riverine inputs forcing the ocean reanalysis and 4. specific variables from the ocean reanalysis, such that they have been very directly constrained by the observations (such as SST, or surface chlorophyll). So some specific variables from the ocean reanalysis are used as an input into the NN model. As we argue in the paper, due to the proximity of the assimilated observations to the reanalysis outputs near the observed locations, the reanalysis is seen here as a way how to coherently extrapolate the observations across the domain and provide consistent gridded inputs for the NN model. This approach was used also in Skakala et al (2023) and effectively provides consistent matching of different observational data-sets (e.g. atmospheric and different ocean observations) across space and time, whilst maintaining that every available observation is used in the training and test data. We believe that any other form of interpolation of satellite observations would be less reliable than using the gridded reanalysis and/or would substantially reduce the number of available training/validation/test data.

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?

We apologize for the confusion, the issue is that in the Eq.1 we explicitly compare ``NN'' and ``Rean'', whilst in Eq.2 we compare both of those with ``Obs''. It would be impractical to write the same equation twice, once for ``NN'' and second time for ``Rean'', so we wrote it once introducing ``Mod'', where ``Mod'' stood for both ``Rean'', or ``NN''. To clarify this we suggest adding a sentence:

"The bias between any model "Mod" (``Mod'' could be either NN, or reanalysis) and observations is defined as: …"

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.

Thank you, we will do this.

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

Thank you, we will do this.

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.

Despite of the reanalysis representing advection, it has huge known biases in nitrate. On the other hand the NN model is trained on the in situ observations, so it has the capability to predict much more reliable nitrate values than the reanalysis. An issue occurs on finer temporal and spatial scales where dynamical advection processes play an important role, and those scales are not captured well by the NN model. On those scales combining the NN prediction (working well on the coarser spatio-temporal resolution) with additional model dynamics could further improve the nitrate values. This approach would combine the advantage of both, the NN and the dynamical model, and could be done by e.g. unbiasing the dynamical model with the NN predicted nitrate. One way of doing this would be through data assimilation. We suggest to add some text on this into the conclusions section:

"Curiously the finer spatio-temporal representation of nitrate could be automatically improved if we assimilated the NN-predicted nitrate into the dynamical model, as such scheme would benefit from both, the dynamical model advection scheme and the improved representation of the (coarser resolution) nitrate by the NN model."

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

Thank you, we suggest to add a third column to the Figure showing the difference between the WOA and NN-predicted nitrate.

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?

It is true that the main Figure showing this is Fig.S3 in the Supporting Information (SI). We put the Figure to SI to avoid overwhelming the reader with too many Figures in the main text, and we would like to maintain this also in the paper's revision. However, if the reviewer insists that the Figure needs to be put in the main body of the paper, we can do that.

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

Thank you, we will correct this.

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.

Thank you, we will correct this.

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.

We think, this is a misunderstanding. As stated in our response to reviewer's point 6, the winter nitrate is taken from the NN prediction using range of inputs into the NN model including the ocean reanalysis inputs. However the nitrate is predicted by the NN model, it is not the reanalysis nitrate. As we already said we take the full blame for this misunderstanding.

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.

We thank again the reviewer for this point and, as we already wrote in our response to their analogous comment 4, in light of the reviewer's arguments we are very happy to revise these statements. We are also very happy to cite all the recommended papers. We would suggest to replace the text in this section with the following text:

"In Fig.6:C we look at correlations between inter-annual time-series of summer nitrate and chlorophyll concentrations. High positive correlation in Fig.6:C indicates regions where growth is either strongly nitrate-limited, or limited by other driver positively correlated with nitrate. Realistically the only such drivers can be other nutrients, i.e. phosphate (there is substantial phosphate-limitation on NWES, Skogen et al.(2004); Philippart et al. (2007); Loebl et al. (2009); Lenhart et al. (2010a); Burson et al. (2016b); Grosse et al. (2017)), so we can conclude that those regions are strongly nutrient-limited in the Summer (with the nutrient being likely nitrate). The coastal part of the regions with strong positive correlation delimits areas which could be more sensitive than others to high river nutrient loads. This does not automatically imply high risk of eutrophication, as this would also depend on other factors, such as the overall river outflow in each area and the socio-economic activity, but it indicates certain increased vulnerability. The Fig.6:C shows high Summer nitrate-chlorophyll positive correlations mostly in the southern North Sea region, in the western English Channel, Bay of Biscay and the south-west of the domain. These are again the regions where the inter-annual fluctuations of summer nitrate are relatively large (Fig.6:D). Interestingly, the eutrophication-problem areas, as identified by the OSPAR NWES eutrophication status reports (such as south-eastern North Sea, coastal areas around Brittany, Axe et al. (2017); Devlin et al. (2023)), fall under these vulnerable zones delimited in Fig.6:C. However, Fig.6:C includes also other regions, such as eastern coastline of Scotland, southern coast of Ireland and zones in the Irish Sea."

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.

Thank you we will make that clear. But we would like to remind the reviewer that as we responded to their comments 6 and 39, the Fig.6 is correlating NN-predicted nitrate with the reanalysis chlorophyll (acting as extrapolation of satellite chlorophyll observations).

42. Line 250: “over a 22 year period”

Thank you very much, we will correct this.

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.

As we said, we are happy to modify our text in the light of this reviewer objection. We would suggest to state a milder version of what we originally wrote:

“Using the newly developed product, we have identified nutrient-limited coastal areas with potentially strong ecosystem responses to an event of river nutrient pollution …”

Also the misunderstanding regarding the NN predicted nitrate has been now clarified in our responses to reviewer comments 6, 39 and 41.

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.

We agree that this would be very interesting. It is however another major piece of work and will take some time to complete in the future. We are however working on this goal and would like to write a follow-up paper on this application hopefully in the near future.

45. Fig. 7: please add clearly that the time series is from the NN model.

Thank you very much, we will do this.

We would like to thank once again the reviewer for their hard work and their great contribution to improve the manuscript quality,

best wishes,
Deep Banerjee and Jozef Skakala