

We are grateful to the Reviewer for the careful consideration and evaluation of our manuscript. We believe that the manuscript will be significantly improved in clarity and effectiveness after incorporating the suggested revisions.

In this document, the Reviewer's comments are presented in black, while our responses are highlighted in blue.

Review of the manuscript How extreme river discharge events shape coastal biogeochemical dynamics: the 2024 northern Adriatic case , by Di Biagio et al, submitted to State of the Planet (SP-2025-09).

Manuscript overview

The manuscript details a study into the drivers of the observed 2024 eutrophication issues in the Northern Adriatic Sea. To this end the authors use 9 different sources of information, which are handily listed, and of which 3 are observational or semi-observational (satellite product) sources and 6 are modelling products. They use these sources to visualise the differences between the 2024 season and the long-term mean of 1990-2022 for Po river discharge, sea water temperature, marine phosphate levels and marine net primary production (all from model sources). Selecting periods of high river discharge the authors then proceed to quantify the elevated levels and suggest main driver mechanisms for the observed high Chl-a levels in the different periods.

Review overview

The manuscript is well written but the presented analysis is not very deep or detailed. The proposed driving mechanisms for the observed high Chl-a levels are mainly speculative and a real quantification is not presented.

A very similar analysis was done with purely observational data by Vona et al (2025), as is referenced in the manuscript, but it is unclear what the use of the model products is contributing. Indeed, Vona et al (2025) showed positive correlations between Po river discharge on one hand and marine Chl-a and TSM concentrations from satellite data. Yet in this manuscript only a few drivers are used, despite the fact that the selected model products are quite extensive. Therefore I suggest that the authors 1. Include more drivers in their analysis (at the very least a proxy for the underwater light climate, possibly also NOx concentrations, zooplankton biomass and strength of stratification) and 2. Present a better quantification of the anomalous events. The promised quantification currently consist of Tables 2 and 3 and textual indications of visible differences. As it is now it is unclear to me what the current manuscript adds to the understanding we get from the work by Vilibić et al (2025) and Vona et al (2025).

We thank the Reviewer for these suggestions. We believe they are extremely helpful for improving our manuscript.

Regarding the quantification of chlorophyll anomaly, we plan to clearly state the definition of our estimation in the text, as follows:

$$\text{anomaly\%} = 100 \times [(\text{cumulative_2024} - \text{cumulative_clim}) / \text{cumulative_clim}]$$

The formula compares the cumulative value observed in 2024 with the corresponding climatological cumulative value to determine how much higher or lower the total for that period is.

In doing so, it offers a clear quantitative measure of the overall anomaly, highlighting the extent to which 2024 departs from the climatological baseline over the selected period.

We also agree that explicitly showing the temporal evolution of other ecosystem variables in the revised manuscript would strengthen our interpretation of marine coastal processes. In particular, we propose to add: the light attenuation coefficient at 490 nm (as proxy for water transparency), mixed layer depth (for vertical stratification), nitrate concentration and zooplankton biomass, following the Reviewer's suggestions.

However, we can only include them in the Appendix, along with a brief discussion in the text, as the standard format for Ocean State Report (OSR) contributions allows a maximum number of figures equal to 4 and 3000 words in the main text (<https://marine.copernicus.eu/access-data/ocean-state-report>); in the submitted paper the limits of figures and words have already been exceeded.

In preparing the submitted manuscript (including the selection of figures), we chose to present primary production as a way to illustrate the effects of eutrophication on the marine ecosystem. Unlike the submitted manuscript, studies such as Vilibić et al. (2025) and Vona et al., (2025), for example, do not include primary production, and therefore do not allow assessment of the decoupling between chlorophyll and primary production. In this regard, our inclusion of primary production adds significant value to the present study.

Additionally, we plan to include an evaluation of oxygen levels recorded in the summer of 2024 as a concrete example of potential stress on marine organisms associated with organic matter consumption (please see our reply to the comment 4, pages 3-4 of this document).

Apart from this I would like to see more discussion on the reference period used. For the long-term mean the authors use the period 1999–2022, likely due to availability of modelled products on CMEMS. But marine temperature trends around Europe tend to show an increase in annual mean temperature starting around 1980 (e.g. Raicich & Colucci, 2019 and examples on <https://ocean.ices.dk/core/iroc>). Here the reference period is completely within the current warming period, which makes me suspect that any current year will show anomalies with respect to the long-term mean in term of temperature (2024 was the warmest year on record in all marine T time series I have seen), and exhibit associated anomalies in Chl-a (faster regeneration of nutrients at the very least). During the whole reference period the discharges of the Po river have been declining (Marini & Grilli, 2023) as is pointed out by the authors. Only considering anomalies from 1 year with a high discharge level with an unstable reference period has its limitations. This should at least be discussed in the manuscript.

The OSR contribution format requires climatologies to be calculated starting from 1993 or later, depending on product availability and limitations. In our case, the BGC reanalysis starts in 1999 (due to the availability of satellite chlorophyll data, which is assimilated), and therefore we considered our climatology to begin in 1999. The analysis of 2024 is mandatory for all contributions; in our case, i.e., the proposed contribution for Chapter 4 “Specific events in the ocean in 2024,” this is the year for which events of interest are analysed by definition.

However, we agree with the Reviewer that it would be appropriate to include a comment on the choice of this temporal comparison (between 2024 and 1999–2022 years). We will add it in Section 3 of the revised manuscript.

More detailed comments are provided below.

Recommendation

Major revision

Detailed Comments

1. Table 1: I really like this overview of data sources, it works well with the text.

We thank the Reviewer for this comment. The presented format is the standard adopted in the Ocean State Report editions.

2. Line 44: has the decrease in P inputs led to the NAS system being P limited in the coastal zone?

Yes, as supported by previous publications (e.g., Mozetič et al., 2010 and references therein; Cozzi and Giani, 2011; Giani et al., 2012).

3. Line 54-57: a reference to figures 1 and A1 could be added here, as could a quick mention of the 2024 annual mean discharge and the annual mean discharge over the reference period. This would allow readers to compare the 2024 annual value with those reported by Vona et al (2025, their figure 4A) and better quantify just how out of the ordinary the year was.

We agree with the Reviewer's suggestion. We will add references to Fig. 1 and A1, as well as a mention of the annual mean discharges in 2024 and in the reference period.

4. Line 83-84: this is definitely worth adding as eutrophication is only a problem when undesirable consequences occur. Most definitions of eutrophication include the negative effects, as does the cited MFSD, and so stating that there are high chlorophyll levels is not enough. So I would put this earlier in the text and not add it as an afterthought.

We agree with the Reviewer: according to the MSFD, eutrophication is defined by both nutrient enrichment and its undesirable effects.

However, the appearance of mucilage is considered a multifactorial phenomenon, where eutrophication is one of the contributing factors, i.e., mucilage events are not necessarily a general and unequivocal sign of eutrophication (although eutrophic conditions can favor it), such that we can put this part earlier in the text when we introduce the eutrophication in general (at line 48-49). We added the mention to mucilage events here, at lines 83-84, since this paragraph includes the available observations of dynamics potentially connected to eutrophication (i.e., high Po river discharges, high chlorophyll levels measured in the coastal waters, mucilage events).

Our evaluation and analysis of eutrophication is instead based on the estimation of the chlorophyll anomaly (whose definition will be provided in a more detailed way, as written in our reply to the general comments in the "Review overview" part, pag. 1-2), along with the interpretation of the marine ecosystem functioning, during the 2024 year. In particular, we analysed the high levels of net primary production in summer and commented on the possible negative effects on the marine organisms.

Following the Reviewer's suggestion to specify that eutrophication is only a problem when undesirable consequences occur, we plan to: 1) explicitly address this aspect in general, when we introduce eutrophication (lines 48-49 of the submitted manuscript); 2) explore further this aspect in our analysis, by adding: (i) another figure (in the Appendix) showing the oxygen levels in the subsurface layers in August 2024 as a representative example of negative effect, along with an explicit discussion in the main text of the paper; (ii) other figures (in Appendix) for a more exhaustive description of the marine dynamics: the light attenuation coefficient at 490 nm, mixed layer depth, nitrate concentration and zooplankton biomass.

In particular, regarding dissolved oxygen, following Vaquer-Sunyer & Duarte (2008), we consider $5 \text{ mg O}_2 \text{ L}^{-1}$ ($\sim 156 \text{ mmol m}^{-3}$) as a precautionary threshold for hypoxia. Using this threshold, we note that episodes of potential hypoxia-induced stress may have occurred in the subsurface layers in 2024; for example, at 30 m depth between 12 July and 13 September. The Figure R1 below shows the monthly mean of dissolved oxygen at 30 m depth in August 2024, according to the Med-MFC Analysis and Forecast Biogeochemistry (Product 1, on which the paper is focused). Shaded areas correspond to values lower than 156 mmol m^{-3} , i.e., to areas potentially affected by hypoxia-induced stress for some species.

We propose to add a similar figure in the Appendix, focused on the specific area under study (i.e., NAS ROFI area under the Po influence), along with a comment in the main text in the Sect. 2 and revised lines 268-273 about dissolved oxygen in Sect. 3.

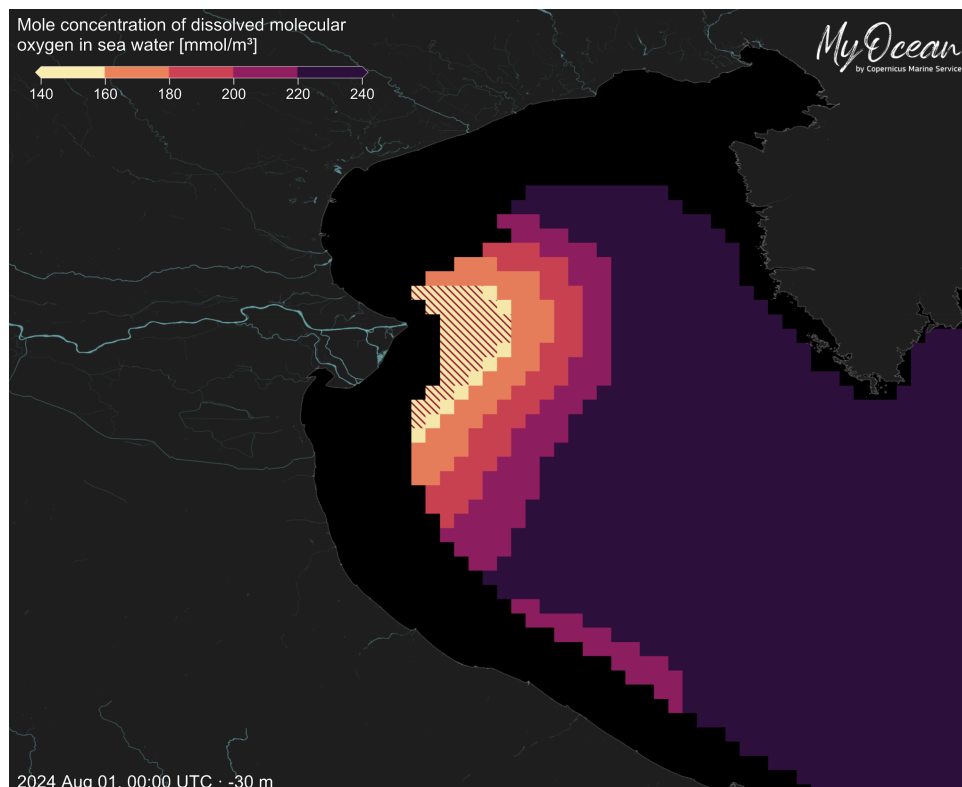


Fig. R1: Monthly mean of dissolved oxygen concentration at 30 m depth in August 2024, according to the Med-MFC Analysis and Forecast Biogeochemistry (Product 1 in the Table 1 of the submitted manuscript); shaded areas refer to values lower than 156 mmol m^{-3} . Data from MyOceanPro (Copernicus Marine Service).

5. Line 84-87: I would rephrase this as now the point of this sentence is unclear. Any in any case, the authors do not try to establish a direct connection between Po discharge and mucilage events. Even the connection to elevated Chl-a levels is not direct but simply by inference.

We thank the Reviewer for this comment. We propose to rewrite the sentences at lines 82-87 as follows:

“The summer of 2024 was also marked by other anomalous phenomena: extensive mucilage events were reported along both the western and eastern coasts of the NAS (e.g., Vilibić et al., 2025). Such events have become rare since the eutrophication period of the 1980s, and a direct, unequivocal link between mucilage formation and exceptional spring discharges of the Po River cannot be straightforwardly established. This is because mucilage development results from multiple concurring processes, including the massive release of polysaccharide excretions by phytoplankton under prolonged calm and sunny conditions in strongly stratified waters (Precali et al., 2005), sometimes following intense phytoplankton blooms (e.g., Cozzi et al., 2004). Nonetheless, its occurrence in 2024 may represent another indication of the NAS system’s anomalous behaviour during that year.”

6. Line 110: Not quite clear to me, do products 1 and 2 include satellite derived Chl-a data assimilation? It would be good to add this information to Table 1, which products used Chl-a assimilation and which EFAS river data, as you look for similarities in response to these drivers and model results, and interdependencies should be clear.

We confirm that Products 1 and 2 include satellite derived Chl-a data assimilation, as stated in 111-112 lines of the submitted manuscript regarding modelled chlorophyll.

To clarify this in the text, in the revised manuscript we will specify “in Products 1 and 2”, modifying the text at lines 111-112 as: “Modelled chlorophyll concentrations in Products 1 and 2 account for weekly data assimilation of CMS OCTAC L3 satellite chlorophyll, both in near-real time and as consistent reprocessed time series (Products 3 and 4, respectively),...”.

On the other hand, since Table 1 conforms to the standard format adopted in OSR, we cannot add this information directly in the table. This is the reason for which we made explicit the interdependencies among the products in the paragraph named “Products used and dependencies”. In particular, CMS Med-MFC Analysis and Forecast Products 5 (for physics) and 1 (for biogeochemistry) implement EFAS v5 river discharges (as stated in lines 115–116 of the submitted manuscript); the HR National operational system assimilates chlorophyll from the same CMS OCTAC satellite and implements river discharges from EFAS v5 (as stated in lines 126-127).

7. Line 145: please explain exactly what the climatology is. As far as I understand it, it is the 1999-2022 mean of the same product, correct?

We confirm that the climatology is defined as the 1999-2022 mean; it was computed using the long-term products (i.e., “Multi-Year” products, as named in CMS), that are: 1) Reanalyses, for numerical products; 2) Reprocessed products, for the OCTAC satellite chlorophyll. In fact, the Analysis and Forecast products are “Near-Real Time” products implemented and delivered only for the most recent years (i.e., 2 years) and based on the same models implemented in the corresponding Multi-Year products.

In the specific case of chlorophyll (line 145), the climatology was computed using Products 2 (for the Med-MFC biogeochemical model) and 4 (Med OCTAC satellite), which are detailed in the

caption of Figure 3 as well, and which are consistent with the corresponding Near-Real Time products during the overlapping time period.

8. Section 2.1: Why I there no mention of the quite large differences in y-axis scale in Figure 3? Or the fact that the model is not able to reproduce the peaks present in the satellite data? Because the model is used to investigate Chl-a levels compared to the drivers, but does not seem to capture the extreme highs which could indicate eutrophication occurring.

We recognise that this is an important aspect in our work, and in fact we discussed it later in the submitted manuscript, in lines 252-259. However, we agree with the Reviewer that it is also appropriate to include at least a mention of this aspect in Section 2.1. Therefore, we suggest adding the following sentence at line 156: “Despite evident biases in the absolute values of the chlorophyll time series from the model compared to satellite observations, which will be discussed in Sect. 3, the periods of the highest river discharges (Fig. 1b) correspond to the highest chlorophyll anomalies, in terms of percentage values, shown by both satellite and model (Figs. 3c,d, respectively). In fact, ...”

9. Line 178: do you mean Figure 4a? Because Figure 1a shows observational data.

We confirm that line 178 refers to Figure 4a. We thank the Reviewer for pointing out this typo. We will correct the text accordingly.

10. Line 184-185: “promoted the huge increase”, it certainly would have facilitated the Chl-a increase, but how can you be so sure? You only examine 3 drivers and then present increases in those as the de facto evidence that this is what caused the Chl-a increase. I would word this more carefully.

We thank the Reviewer for this comment. As stated in our reply to the “Review overview” general comments, we will include additional drivers in the revised manuscript, to better highlight the interconnected dynamics within the marine ecosystem and strengthen our interpretation.

11. Line 186-187: “drove”, I think you mean is associated with? This is pure speculation after all, and neglects other drivers such as availability of other nutrients, light availability, carbon availability and grazing. And it should be “both in model and satellite data”

Please see our reply to the previous comment.

Moreover, we will include the suggested expression “both in model and satellite data” in the revised manuscript.

12. Line 187: please show the active radiation in the appendix or provide a reference. And as the Chl-a levels in Figure 3c show an autumn peak that is higher than the one in May the autumn Chl-a levels do not look all that low to me.

We thank the Reviewer for this comment.

We will add a reference paper (i.e., Lazzari et al., 2021) showing the peak of the Photosynthetically Active Radiation (PAR) in the revised manuscript.

Regarding the autumn chlorophyll peak, we did not state that it is low in absolute terms, but rather that it is relatively low in comparison to the nutrient supply by Po river discharge (Figs. 4c,a).

Specifically, we stated: “Since the marine ecosystem becomes light-limited in autumn, we identify this feature as the main responsible for the relatively low values of chlorophyll in its autumn peak with respect to the very high values of nutrient concentration in the same period.” (lines 191-193 of the submitted manuscript). In fact, while the highest Po river discharges of the year occur in autumn (as shown in Figs. 1 and 4a, from both Pontelagoscuro and EFAS data), the highest peak of chlorophyll, as well as the highest percentage anomaly, occur in spring (according to both satellite and model data, in Figs. 3 c,d).

13. Line 238-241: please rephrase as grammatically incorrect. And I don’t see why you use the word “despite”. Vona et al reported positive correlation between observation time series, thus providing a good indication of eutrophication drivers. The current study does not contradict that work as far as I can see, neither does it expand much upon it. Vona et al do report a strong correlation between total suspended sediment concentrations and Chl-a levels in the NAS (both from satellite data), a driver that is not considered here at all (high rainfall events can lead to strong soil erosion, causing high levels of TSM in river discharge).

We thank the Reviewer for pointing out the grammatical inaccuracy.

In that sentence we used the word “Despite” to highlight the difference in the scale of the processes considered in the cited literature: long-term (i.e, for trend evaluations) in Vona et al., 2025, with respect to the short-term (that we defined “high-frequency scale”, i.e., weekly, for process studies) adopted in this paper.

We will rephrase the sentence in the revised manuscript in order to clarify this aspect.

14. Line 268: intense positive anomalies of what?

We will rephrase the sentence. Here we referred to the period of high anomaly of net primary production.

15. Line 274-279: please rephrase as grammatically incorrect and overly long. And as said before I would like to see an earlier discussion of any undesirable disturbance caused by the high Chla levels.

We thank the Reviewer for pointing out the grammatical inaccuracy.

Regarding the discussion about undesirable consequences of the eutrophication on the marine ecosystem, please see our reply to comment 4 (pages 3-4 of this document).

16. Line 295: I don’t feel that you have presented any real evidence for this claim, even though I would expect the operational model to do better as it has a higher spatial resolution. Figure 2c shows exactly that, as the satellite signal is different to both model products.

We thank the Reviewer for this comment.

The length limitation of the OSR contributions (i.e., max. 4 figures and max. 3000 words in the main text) prevented us from providing a more extensive comparison between the performances of the Med-MFC model (with a coarser grid resolution, i.e., approximately equal to 4 km on the horizontal direction) and the Italian national operational system (at higher resolution, i.e., 500 m) in simulating the horizontal variability of the chlorophyll patterns. Therefore, we provided a synthetic analysis in 220-232 lines, including Table 3 (in which a minor typo had caused the rows to

be swapped; this will be fixed in the revised manuscript), with reference to Fig. 2 as a representative example.

Anyway, we recognise that, in the expression “The novel Italian operational model ... proved to reproduce more accurately the spatial structures...”, the reference to the “accuracy” was not really appropriate.

We will rephrase the sentence as follows:

“The novel Italian operational model, which can be considered a natural extension of the operational CMS Med-MFC system, featuring EFAS v5 river discharges as boundary conditions, has demonstrated the ability to reproduce finer spatial structures associated with the Po river plume and has the potential to provide a more accurate representation of coastal dynamics related to eutrophication.”

17. Appendix: I miss a discussion on Figure 1A, which shows that the EFAS product underestimates Po river flow, especially during peak discharges. As these peak discharges are the focus of the study a quantification of this error in EFAS forcing should be included.

We thank the Reviewer for this comment. We agree with the Reviewer and we will add a comment on this in Sect. 3 (Discussion and conclusions).

In particular, we would like to highlight that even when river discharges were underestimated (i.e., in early spring), the model was still able to capture the maximum chlorophyll anomaly of the year, also benefitting from the assimilation of satellite-derived chlorophyll data.

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