

General comment by the authors

We have carefully revised the manuscript in light of the two reviewers' comments and suggestions, which have helped improve the clarity and robustness of the study.

In particular, in response to Reviewer 2's comments, we have:

1. added additional analyses on ecosystem functioning, highlighting oxygen depletion as a consequence of organic matter accumulation (see our response to Review Overview and Comment 4; Figures A3 and A4 in the Appendix, respectively);
2. expanded the discussion on the comparison with observational data, i.e., satellite-derived chlorophyll and Po river discharge measurements at the Pontelagoscuro station (see our response to comments 3, 8, and 17).

A detailed point-by-point response to all comments from both reviewers is provided below.

Reviewers' comments are presented in black, while our responses are highlighted in blue and our changes in the manuscript text in red.

Reply to Reviewer 1

We sincerely thank the Reviewer for the positive feedback and thoughtful perspective on the manuscript. We address the minor comments in detail below.

The paper is interesting and well presented. It effectively communicates the research findings. I have only minor suggestions:

Line 145: The authors could consider providing a definition of 'climatology' in the context of the study, such as 'climatology (long-term average)', to help clarify the terminology for non-specialists in the field.

We thank the reviewer for the suggestion. We incorporated the recommended expression "(long-term average)" in the first definition of "climatology" (lines 140-141 of the revised manuscript) to improve clarity:

We also maintained the weekly basis in the reconstruction of their climatological reference (i.e., long-term average) 1999-2022...

Lines: 180-185 "increasing temperature (i.e., 11°C - 16°C, i.e., 2 °C - 3°C above the climatology)" is unclear. Do you mean "increasing temperature (temperature in the range 11°C - 16°C, i.e. 2 °C - 3°C above the climatology)"? Please clarify in the text.

Yes, we meant exactly that. We clarified the text at lines 196-197 of the revised manuscript as suggested:

... increasing temperatures (temperature in the range 11°C - 16°C, i.e., 2 °C - 3°C above the climatology; Fig. 4b)...

Line 230 “HR” missing in the legend.

Thank you for pointing out this typo. We added “HR” in the caption of the Table 3 (line 243 of the revised manuscript).

Reply to Reviewer 2

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

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, in the revised manuscript we clearly stated the definition of our estimation, at the end of 1.1.2 Section (lines 151-154 of the revised manuscript):

In particular, we expressed the anomaly as a percentage, computed as the difference between the cumulative 2024 values and the climatological values, normalized by the corresponding climatological values:

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

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 added: 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 could only include them in the Appendix (as Fig. A3, reported in the next page of this document), along with brief comments in the main 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>); please note that the limits of figures and words have already been exceeded in the submitted paper.

In the main text of the revised manuscript, we included these properties of the marine ecosystem at lines 187-189:

Additional information on the ecosystem status (i.e., vertical stratification of the water column, nitrate concentration, zooplankton grazing and water turbidity) is reported in Fig. A3, in the Appendix.

lines 193-196:

A January-February positive anomaly in surface temperature (Fig. 4b), connected with low values of mixed layer depth, occurred earlier than usual (Fig. A3a in the Appendix), represents a preconditioning factor of stratification in the area. During the same period, an enrichment in phosphate (the limiting nutrient in the area) was observed (Fig. 4c), whereas nitrate remained closer to climatological values (Fig. A3b).

lines 277-279 (in Sect. 3):

Moreover, the high levels of phosphate with respect to climatology (Fig. 4c), as well as the high temperatures (Fig. 4b) and an earlier-than-usual and strong vertical stratification (Fig. A3a in the Appendix), already present in January and February 2024, further enhanced the spring peaks of chlorophyll.

line 200:

We attribute it to enhanced grazing pressure (as reported in Fig. A3c, in the Appendix)

and lines 196-198:

In early spring, the additional supply of phosphate (Fig. 4c) and the increasing temperatures (temperature in the range 11°C - 16°C, i.e., 2 °C - 3°C above the climatology; Fig. 4b), likely further enhanced by concurrent surface turbidity (Fig. A3d in the Appendix), promoted the large increase in chlorophyll shown in Figs. 3c,d.

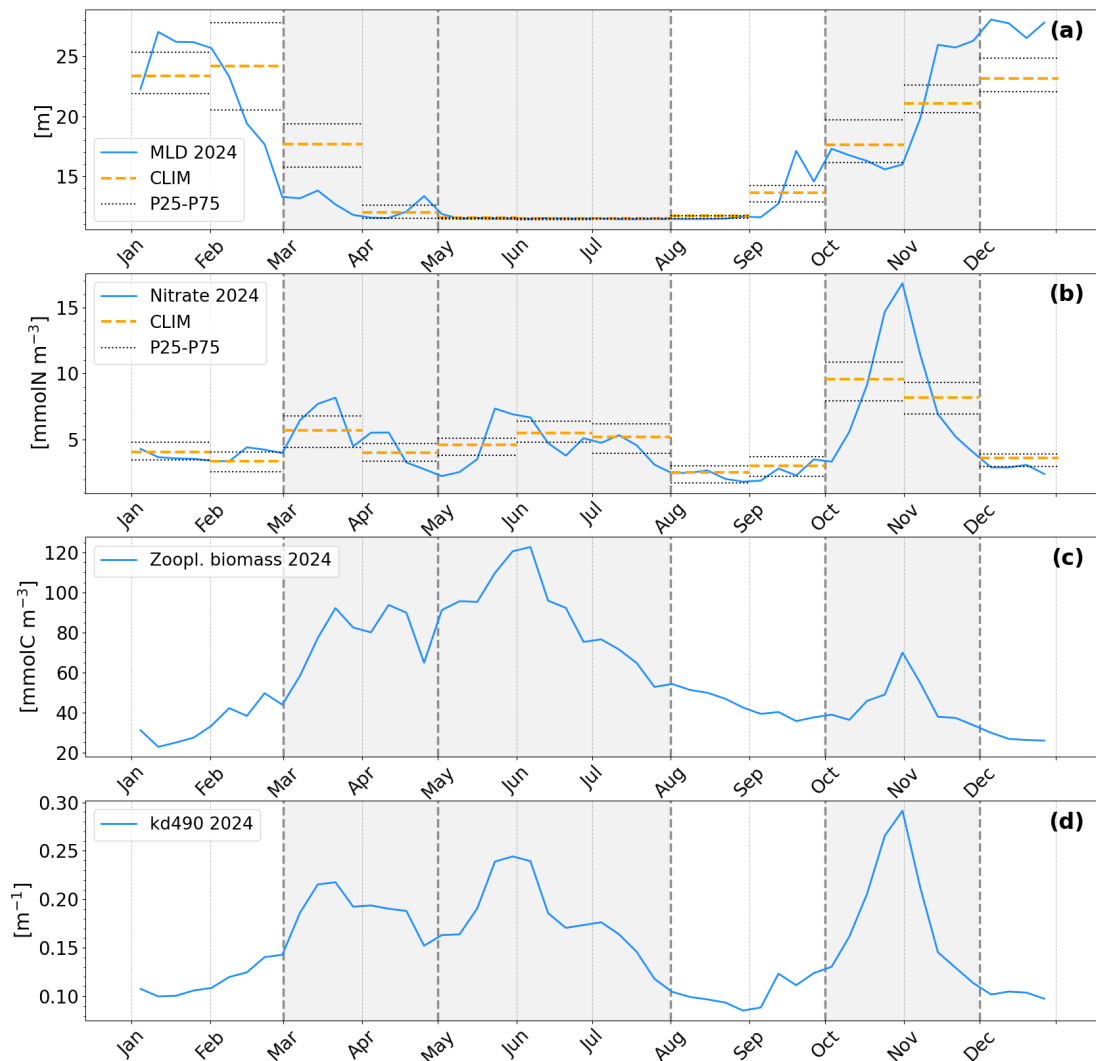


Figure A3: 2024 weekly time series (blue lines) and 1999-2022 climatology (dotted lines, orange for monthly mean, black for 25th and 75th percentile) of modelled variables: (a) mixed layer depth (Products 5 and 6, respectively), (b) nitrate concentration at surface (Products 1 and 2, respectively), (c) zooplankton carbon biomass at surface (Product 1), and diffuse attenuation coefficient of the downwelling irradiance at 490 nm (Product 1), spatially averaged in NAS box (Fig. 2). Nitrate climatology included the bias-correction illustrated in Sect. 1.1.2. Time periods of interest (Mar-Apr, May-Jul, Oct-Nov) are indicated as shaded areas between vertical gray dotted lines.

In preparing the first 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, showing how model prediction systems can complement observing systems in terms of added variables and ecosystem indicators.

In fact, in the revised manuscript we included 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).

In light of the figures added to the Appendix, we have also included the following text at lines 317-320:

The appendix includes selected additional figures supporting the discussion of the model results. In particular: the comparison between EFAS and observations for Po river discharges (Fig. A1), 2024 anomalies with respect to the long-term mean for other key environmental variables (Figs. A2 and A3) and the time evolution of oxygen depletion at 30m depth in the area during the summer period (Fig. A4).

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. Thus, the used time periods are a constraint and a requirement rather than a choice.

We agree with the Reviewer that it would be appropriate to clarify the choice of this temporal comparison (between 2024 and 1999–2022 years). We added it explicitly in Section 1 in the objective 1, lines 92-93 of the revised manuscript:

(i) quantify the anomaly of the 2024 coastal chlorophyll in the NAS ROFI area with respect a reference period (1999-2022) encompassing the Ocean Colour era;

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 added references to Fig. 1 and A1 at lines 54-55, as well as a mention of the annual mean discharges in 2024 and in the reference period at lines 64-65:

With respect to the reference period considered in this paper, i.e., 1999-2022, the annual Po river discharge was approximately 50% higher.

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 47-49).

We added the mention to mucilage events here, at lines 83-85, 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 has been provided in a more detailed way, as written in our reply to the general comments in the “Review overview” part, pag. 3), along with the interpretation of the marine ecosystem functioning, during the 2024 year. In particular, in the submitted manuscript 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, in the revised manuscript:

1) we explicitly addressed this aspect in general, when we introduce eutrophication (lines 47-50 of the revised manuscript):

In this context, phytoplankton chlorophyll can be used as a valuable proxy for marine eutrophication (Descriptor 5 of the Marine Strategy Framework Directive, Ferreira et al., 2011; European Commission, 2020), reflecting variations in key drivers (e.g., nutrient availability, water column stability, and anthropogenic input) and undesirable changes in environmental conditions (e.g., oxygen depletion and excessive biomass accumulation).

2) we explored further this aspect in our analysis, by adding: (i) an additional figure (i.e., Fig. A4, in the Appendix) showing the oxygen levels in the subsurface layers in summer 2024 as a representative example of negative effect, along with an explicit discussion in the main text of the paper (reported in the next paragraph); (ii) another figure (i.e., Fig. A3, in the 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 (as reported at pages 3-4 of this document).

In particular, regarding dissolved oxygen, following Vaquer-Sunyer & Duarte (2008), we considered $5 \text{ mg O}_2 \text{ l}^{-1}$ ($\sim 156 \text{ mmol m}^{-3}$) as a precautionary threshold for hypoxia. Using this threshold, we noted that episodes of potential hypoxia-induced stress may have occurred in the subsurface layers in 2024. In fact, the Figure A4 below (added in the revised manuscript) shows daily dissolved oxygen at 30 m depth in the period July-September 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.

In the main text of the revised manuscript we also added a mention to this figure at lines 284-289 in Sect. 3:

Even if a detailed analysis is out of the scope of the present publication due to length constraints, the particulate organic matter produced during intense positive anomalies of net primary production with respect to the climatology, especially in early summer (Fig. 5), is either transported along shore or settles on the sea bed and undergoes remineralization. This process leads to oxygen depletion in the subsequent period. The model reproduces a northward evolution of the spatial extent of the impacted area (Fig. A4, in the Appendix), although simulated oxygen concentrations remain above the hypoxia levels observed during the 1980s and 1990s (e.g., Djacovac et al., 2015).

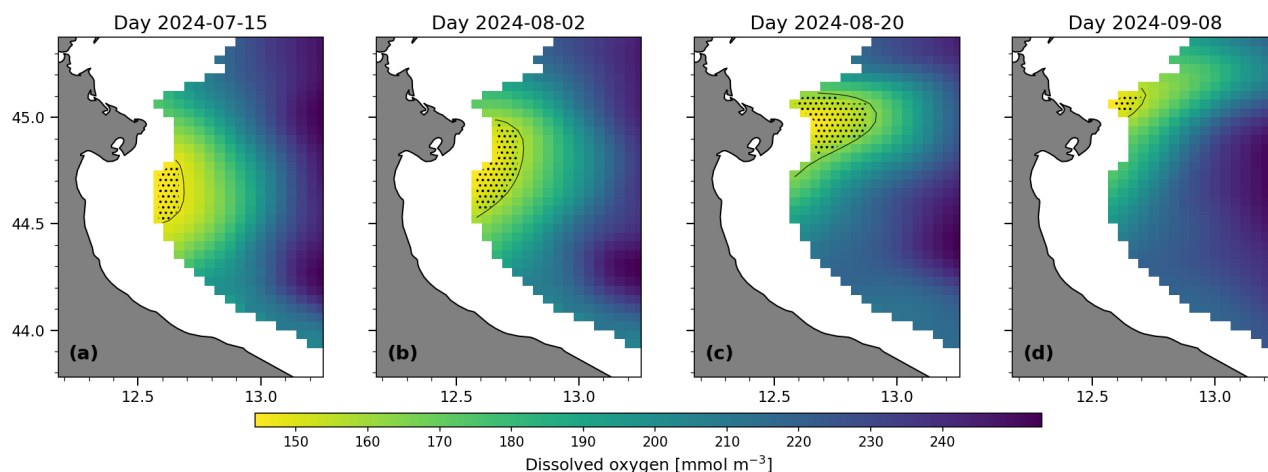


Figure A4: Selected daily maps of dissolved oxygen concentration at 30 m depth in the period July-September 2024; shaded areas refer to values lower than the precautionary threshold of 5 mg l⁻¹ (i.e. 156 mmol m⁻³) for oxygen depletion identification (Vaquer-Sunyer & Duarte, 2008). Data from Med-MFC Analysis and Forecast Biogeochemistry (Product 1).

as well as the cited study in the References:

Vaquer-Sunyer R. & C.M. Duarte C.M: Thresholds of hypoxia for marine biodiversity, *Proc. Natl. Acad. Sci. U.S.A.* 105 (40) 15452-15457, <https://doi.org/10.1073/pnas.0803833105>, 2008.

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 rewrote the sentences at lines 83-90 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 specified “in Products 1 and 2”, modifying the text at lines 116-117 as:

Modelled chlorophyll concentrations in Products 1 and 2 account for weekly data assimilation of CMS OCTAC L3 satellite chlorophyll ...

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 120–122 of the revised 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 130-132).

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 (to which this comment refers), 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 added the following sentence at lines 163-165 of the revised manuscript:

Despite biases in the absolute values of the chlorophyll time series from the model compared to satellite observations, which is discussed in Sect. 3, the model is consistent with observations in predicting the periods of the highest river discharges (Fig. 1b) in correspondence with the highest chlorophyll anomalies (Figs. 3c,d, respectively). In fact, ...

Additionally, we added a few more details in the discussion at lines 267-272 about the reasons for the model-observation bias in the very coastal areas near river mouths.

... the biases in terms of absolute values are due to limitation in the assimilation procedure of very high value of chlorophyll (i.e., values higher than 3 standard deviations from climatological values

are not assimilated, Teruzzi et al., 2021) and by potential limitation of the BFM model, whose 4 phytoplankton groups have been optimized to simulate trophic open sea conditions across the Mediterranean Sea (Salon et al., 2019), to reproduce very high values of specific phytoplankton groups which can occur only in coastal areas near the major rivers.

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

We confirm that line 178 in the submitted manuscript refers to Figure 4a. We thank the Reviewer for pointing out this typo. We corrected the text accordingly, at line 190 of the revised manuscript.

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 included 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 included the suggested expression at line 200 in the revised manuscript:

both in model and satellite data

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 added a reference paper about the peak of the Photosynthetically Active Radiation (PAR) in the revised manuscript, in the main text at line 202:

... e.g., Lazzari et al., 2021

and in the References:

Lazzari, P., Salon, S., Terzić, E., Gregg, W. W., D'Ortenzio, F., Vellucci, V., Organelli, E., and Antoine, D.: Assessment of the spectral downward irradiance at the surface of the Mediterranean Sea using the radiative Ocean-Atmosphere Spectral Irradiance Model (OASIM), *Ocean Sci.*, 17, 675–697, <https://doi.org/10.5194/os-17-675-2021>, 2021.

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, now at lines 205-206). 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, included in the submitted manuscript, 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.

In the revised manuscript we rephrased the sentence in order to clarify this aspect (lines 251-254)

On the long-term timescale, positive correlations between a negative trend in Po River outflow and a negative chlorophyll trend have been reported in the literature (e.g., Vona et al., 2025). On the other hand, our study aims to quantify the connection between anomalous chlorophyll events and anomalies in Po River discharge, as well as other environmental drivers, on a high-frequency timescale (e.g., weekly) relevant to phytoplankton processes.

14. Line 268: intense positive anomalies of what?

We rephrased the sentence (line 285-286):

during intense positive anomalies of net primary production with respect to the climatology,...

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 error and the excessive length of the sentence. In the revised manuscript we rephrased the sentence (lines 292-297) as:

Although the CMS model has not been designed to simulate all the processes and substances associated with the formation and accumulation of mucilage aggregates observed in the NAS during summer 2024, our findings on marine ecosystem functioning are in agreement with Vilibić et al., 2025. They highlighted, in particular, the importance of the intense vertical stratification that started in February 2024 (also shown in Fig. A3, in the Appendix) as a potential preconditioning factor for mucilage events, further enhanced by high and repeated Po River discharges and elevated temperatures (2–3 °C above climatology, according to our estimates).

Regarding the discussion about undesirable consequences of the eutrophication on the marine ecosystem, please see our reply to comment 4 (pages 6-8 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 233-245 lines, including Table 3 (in which a minor typo had caused the rows to be swapped; this has been 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 rephrased the sentence as follows (lines 312-315):

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 added a comment on this in Sect. 3 (Discussion and conclusions), lines 259-262:

In fact, the EFAS discharge time series at the Pontelagoscuro station displays good overall agreement with the observed series in terms of both magnitude and timing of discharge peaks (Fig. A1), despite an underestimation of the mean annual flow (mean discharge is approximately 10% lower than observed) and of early spring peaks (EFAS peak values are about 25% lower than observed).

References

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