We thank the reviewer for her/his *comments*. We report here our proposed replies to the two major weaknesses and the specific comments: if accepted, they will be properly included in the revised version of the manuscript.

(1) It doesn't give an overview of other published simulations on the Mediterranean Sea. The reader is not able to appreciate the quality of this simulation in relation to other BGC models in the literature;

REPLY 1 – We agree. The revised version will refer to other biogeochemical model applications in Mediterranean Sea. In particular, considered the focus of the manuscript (i.e., biogeochemical operational forecast and its uncertainty estimates), we will include the Poseidon operational system (HYBRID-POM-ERSEM model, Tsiaras et al., 2017; Petihakis et al., 2018). Besides that, there are other recent publications describing multi-annual simulations (Mattia et al., 2013; Macias et al., 2014; Guyennon et al., 2015; Richon et al., 2017) but we think it is out of the scope of this manuscript to list all the models and to provide an overview of their characteristics.
Nevertheless, one important aspect that we would like to highlight in the introduction is an overview of the data availability for validating biogeochemical simulations in the Mediterranean Sea. In particular, the most used variable for validation is satellite-derived surface chlorophyll (Tsiaras et al., 2017, for year 2000; Mattia et al., 2013; Macias et al., 2014; Guyennon et al., 2015; Richon et al., 2017, for a portion or the whole respective investigated multi-year period). In situ measurements from vessels and scientific cruises are also used in Richon et al. (2017) and Guyennon et al. (2015), but allow only to validate very limited time and space portions of the simulations (fixed stations in time or single transects in a very limited time range). Only a few basin-wide validation frameworks, especially for nutrients, are based on comparison with climatology, e.g. Tsiaras et al. (2017) use a seasonally aggregated reference for the whole Mediterranean Sea build on 1990-1999 data from SeaDataNet. Very rarely the vertical proprieties of biogeochemistry are assessed (e.g. Guyennon et al., 2015 and Teruzzi et al., 2014).
Our work introduces the use of BGC-Argo floats data as observational counterpart for the model validation, and presents a comprehensive validation framework from basin-wide and seasonal scale to mesoscale and weekly scale (following GODAE recommendations), with an accuracy level depending on the specific variable and the availability of reference data: for this reason, it may represent a novelty.

(2) It doesn't manage to rationalize the discrepancies diagnosed by the comparison with observations and to disentangle the origin of the observed biases (problems due to the physical model, the biogeochemical parameterizations or to the sensor quality control?). They are a lot of metrics that are not enough used to suggest some solutions and to improve the future systems

REPLY 2 – We thank the reviewer for this comment. We will enrich the comments related to the results gathered from the metrics analysis and coherently relate them with our guesses on possible causes. However, it must be pointed out than the discussion in the submitted manuscript already outlines the general weaknesses of the MedBFM model, which will require future development work (as planned within Med-MFC and requested by the CMEMS "continuous improvement" paradigm). Nevertheless, if accepted, in the revised version we will improve our analysis by reporting and rationalizing the cases when our validation framework can detect specific discrepancies.

For example, as reported in the reply to RC1 (point 2), we highlight that our definition of 16 sub-basins revealed to be a sensible choice since it allowed to demonstrate the model good performance in reproducing the mean spatial gradients within the domain (Fig. 5 of the submitted manuscript) and possible anomalies, such as the underestimation of nitrate in the NWM sub-basin in the subsurface layer that is related partly to possible underestimation of the terrestrial input and partly to the impact of the incoming Atlantic waters.
Another possible detected discrepancy that we would like to mention is the presence of some overestimation for the oxygen field between model and BGC-Argo profiles below 200 meter. However, since the quality check protocol of oxygen data from BGC-Argo floats is continuously progressing we envisage the need for a specific study regarding the oxygen validation and the analysis of oxygen variability simulated by the MedBFM model (see also point 12 of the specific comments).
Further (as also reported in the reply to RC1, point 4), the metrics based on BGC-Argo data (e.g., DCM and MWB metrics) are very innovative and informative. BGC-Argo floats provide simultaneous measurements of physical variables, such as profiles of PAR, salinity and temperature, that act as forcing of the biogeochemistry processes (or proxies for the forcing impacting the biogeochemistry). Thus, an integrate analysis of the model-observation uncertainty can be made comprehensively using multivariate analysis (e.g. PCA and neural network methods), with the aim to disentangle the error sources on vertical profiles. Moreover, we have to note that such analysis would need not just simple measures of distance between observations and model values (such as BIAS and RMSD), but indexes that can put in relation the shape and intensity of the biogeochemical profiles with the underlying processes (light limitation, temperature kinetic dependencies, nutrient availability through vertical fluxes, vertical dispersion through mixing). We think that our work (and the proposed metrics) provide a first step towards the identification and quantification of several functional indexes. Another critical point is the availability of a sufficient amount of float profiles for variables like nitrate and oxygen, which would be required for a statistically significant analysis capable to cover most of the Mediterranean regions and a longest period of the year. A comment on this issue will be added as well.

<u>Specific comments:</u>

1. P3 L26-27: "surface chlorophyll . . . of CMEMS" Which CMEMS product? Is it the same as the one used for assessment on Fig 3 and Fig 4?

REPLY 1 –The product is the OCEANCOLOUR_MED_CHL_L3_NRT_OBSERVATIONS_009_040 as described in section 2.2 ("Set up of the pre-operational qualification simulation for Med-BIO") at (P5, L7-8) and section 3 ("Reference datasets for validation") at (P6, L17-20).
Then, it is important to remind (as explained at P7, L24-27) that this product is used in the data assimilation scheme (at the beginning of each weekly run at day T-7) and then to assess the skill of the forecasts (as in Fig. 3 and 4 for the pre-operational run, and in Fig. 12 for the forecast run) before it is used for assimilation at the next weekly run. From Cossarini et al. (2019), following Mattern et al. (2018), the root mean square (RMS) of the differences between observation and prior model solution provides a measurement of the data assimilation performance and represents a short-term forecast skill metric since it is based on observations that are going to be assimilated in the following cycles. We think that this concept is already clearly depicted in the text. However, we are open to add details if the reviewer suggests they are needed.

2. P4 L10: Which database do you use for river inputs? Reference?

REPLY 2 – Details on the boundary conditions for river nutrients inputs are reported in section 2.2, 5th and 6th items (P5, L17-26), and derive from the dataset built during the PERSEUS FP7 project. We will also include that the dataset is based on a total of 39 rivers with runoff larger than 50 m^3/s. To the best of our knowledge, this deliverable represents the most up-to-date information about terrestrial nutrient discharges for the Mediterranean basin.

3. P4 L15-16: "Additional 2D fields include the surface data for solar irradiance and wind stress": Where are these data from?

REPLY 3 – These data are provided via the offline coupling with MED-PHY component, which derives the atmospheric forcing from the 6-hours ECMWF operational analysis and forecast fields at 0.125° horizontal-resolution. For details please refer to CMEMS catalogue: http://marine.copernicus.eu/services-portfolio/access-to-products/?option=com_csw&view=details&product_id=MEDSEA_ANALYSIS_FORECAST_PHY_006_013.
The source of the 2D fields will be added to the revised text.

4. P5 L11: Is the spin-up forced by a climatological year?

REPLY 4 – The spin-up is based on year 2016 run without data assimilation. This procedure is coherent with that adopted for the spin-up of the pre-operational run of the physical component of the Med-MFC (see details in Clementi et al., 2018).

5. P6 L16-20: is it the same dataset as the one used in the data assimilation scheme? Can you precise the temporal frequency. Daily?

REPLY 5 – Yes, the dataset is the same, and it is used at daily frequency. We will add "daily" at line P6-L17 to better clarify this point. As discussed at previous Reply point n. 1, validation is based on satellite surface chlorophyll data that are not yet assimilated.

6. P6 L24-30: do you use the BGC-Argo dataset available on Coriolis website?

REPLY 6 – We download our data from LOV database due to specific quality control developed in the frame of the CMEMS Service Evolution project MASSIMILI (see details at https://www.mercator-ocean.fr/en/portfolio/massimili-2/). The use of BGC-Argo floats data from Coriolis and CMEMS INSITU-TAC is presently under consideration.

7. P11 L19: prefer 'averaged' instead of 'integrated'

REPLY 7 – We agree with the suggestion, we will substitute it.

8. P11 L20 and Fig9: why do the correlation values vary so abruptly and reach zero sometimes in winter?

REPLY 8 – We thank the reviewer for having raised this issue. The plots of Fig. 9 (right panels) were erroneously computed also for dates without float data (see also Reply point n. 10, and revised Fig.9 in Fig.R1). In other few cases, very low correlation values were related to some possible inconsistencies in the float measurements (see for example the isolated chlorophyll values up to 0.20 mg/m^3 below 200 m in April 2016 and in May 2017). Our operational check of the goodness of the input data is still progressing. Thus, in these cases, we safely decided to remove from the computation these profiles. The revised version of Fig. 9 (here reported) has been corrected.

9. P11 L22-23 and Fig9: This sentence is not convincing. Why these differences between modelled and observed depth of MWB? What is the difference between the mixed layer depth (MLD) and the depth of MWB? Why did you choose this index? Did you compare the mixed layer depths along the float trajectory?

REPLY 9 – We introduced the MWB in order to identify an index quantifying the thickness of the layer affected by the surface chlorophyll winter bloom. As commented in the Reply to major point n. 2, this is a first step towards a specific metric to keep track of biogeochemical processes and their relationship with physical drivers.

The definition of MWB is based on the paper by Lavigne et al. (2015), who identified some standard shapes for chlorophyll profiles from the analysis of a large number of chlorophyll (fluorescence data) profiles in the Mediterranean Sea (see their Fig. 2). Three of those shape profiles (i.e., "homogeneous", "HSC" and "complex") are characterized by decreasing-with-depth values and are typically observed during the winter months in different Mediterranean regions (Fig. 5 by Lavigne et al., 2015). They are not necessarily limited by the MLD since other factors (e.g., light and nutrient conditions) play a role in this surface bloom dynamics.

Thus, the MWB index aims at detecting the thickness of the surface productive layer during winter. The specific choice of the limit of 10% was made after a sensitivity analysis varying the limit between 1 to 10% (not shown). The value of 10% gave results qualitatively consistent with those reported by Lavigne et al. (2015), whilst lower percentage values gave more unclear patterns because the depth increases substantially and the thickness of model layers has also an impact (i.e. thickness of model layers is 5 m at 70 m depth, 7 m at 100 m and 10 m at 140 m). Further, the MWB metric fails to capture the "modified DCM" shape profile (as defined in Lavigne et al., 2015) which occurred for float 6901653 during winter 2016 (Fig. 9). Thus, given the constraint in the definition and the vertical discretization of the model, the application of this index to the floats data may originate some inconsistency (as shown in Fig. 9) and under- or overestimations and uncertainty of a few decameters (see Tab. 3). Despite these limitations, we think that the MWB represents a feasible and informative metric to be coupled with the DCM metrics.

In the revised manuscript, we will explain better the scientific rationale of the MWB metrics and we will modify the original sentence ("The depth of MWB shows some inconsistency between model and float data, since it is not always computable from BGC-Argo floats data.") according to this comment.

10. Fig9: in summer 2017, there is no diagnosed DCM for model. Why?

REPLY 10 – We thank the reviewer: there was an error in the plot (see also Reply point n. 8). The revised version (Fig. R1) reports a wider y-axis and we observe that the DCM in 2017 appears shallower than in 2016 (please also consider the float trajectory – in the left panes of Fig. 9 –, which covers the south-western Mediterranean from Balearic Islands to Alboran Sea).

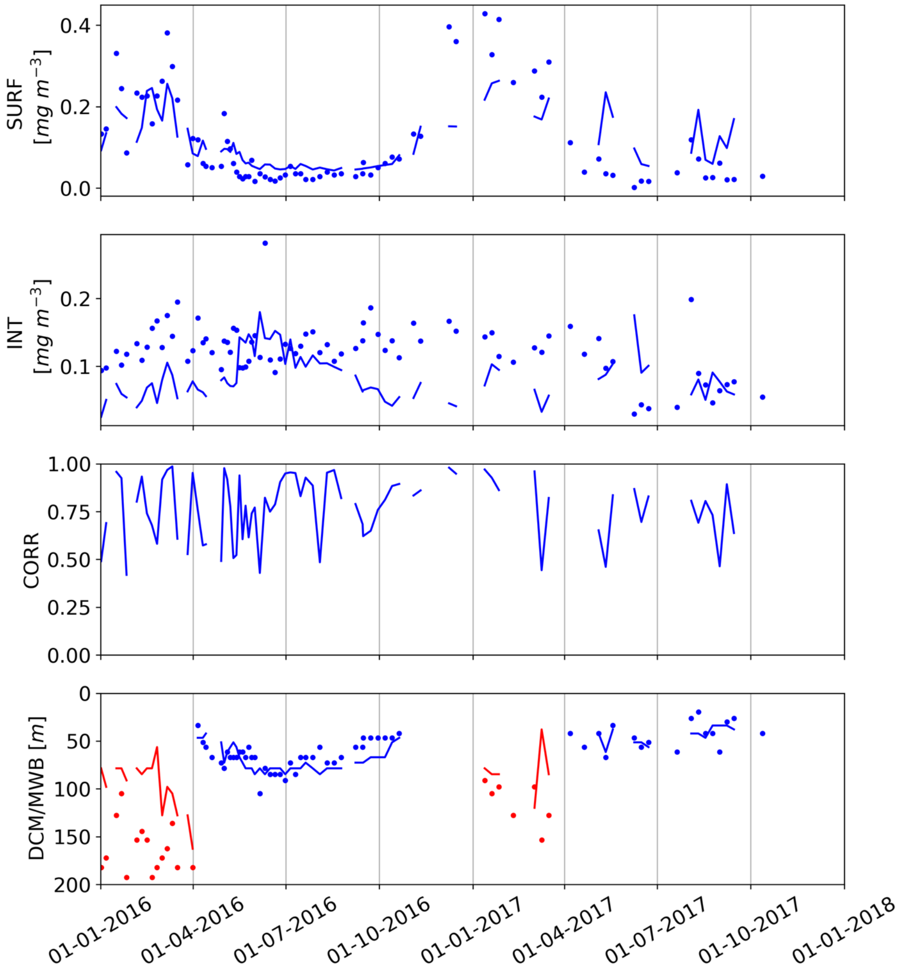


Fig. R1 – Revised right panels of Fig. 9: computation of selected skill indexes (1st to 4th row) for model (solid line) and float data (dots). The skill indexes are: surface (SURF) and 0-200 m vertically averaged (INTG) chlorophyll, correlation (CORR), depth of the deep chlorophyll maximum (DCM, blue) and depth of the mixed layer bloom in winter (MWB, red).

11. Fig 10: skill index 4th row: the legend is missing for NITRCL1/NITRCL2. Why is the blue index missing for model in the beginning of 2017?

REPLY 11 - We thank the reviewer: we will correct the caption (NITRCL1 is in blue and NITRCL2 is in red). We have also extended the maximum depth to 300 m (new version of Fig. 10, right panel is here reported as Fig.R2 for clarity). It can be observed that in the period April-July 2017 one to the two nitracline indexes (i.e., NITRCL2) computed on model output is much shallower than the one estimated from the BGC-Argo float data. Thus, the use of two indexes may help in disentangling between errors due to model behaviour or to sensitivity of the index calculation. If accepted, a comment will be added in the revised version.

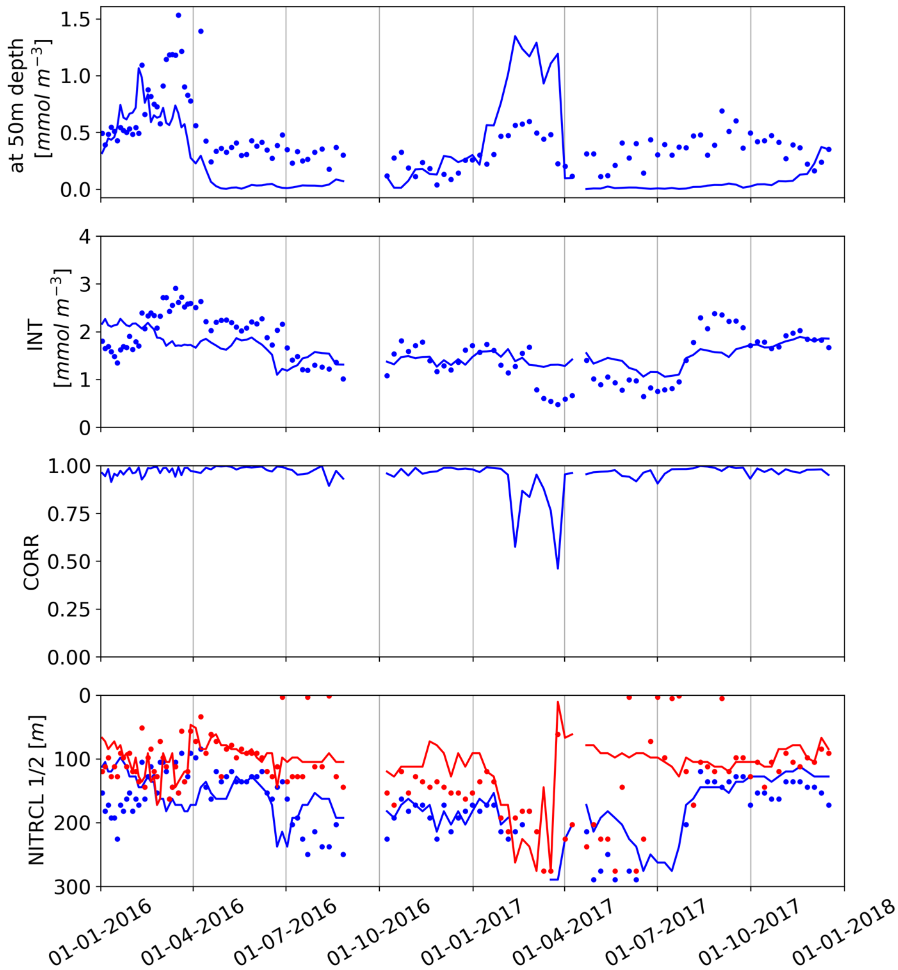


Fig. R2 - Revised right panels of Fig. 10: computation of selected skill indexes (1st to 4th row) for model (solid line) and float data (dots). The skill indexes are: nitrate concentration at surface (SURF) and 0-200 m vertically averaged concentration (INTG), correlation between profiles (CORR), depth of the nitracline computed as NITRCL1 (blue) and NITRCL2 (red).

12. Fig10-11: Authors don't show depths below 300m. For oxygen, the colorbar is saturated below 200 umol/L. It doesn't allow to study the nitrate gradient and the OMZs.

REPLY 12 – The model comparison for depths below 300 m is shown in Fig. 5 for nutrients and oxygen on 6 selected sub-basins. Given the slow dynamics of the deep layers, the comparison based on climatology demonstrates the capability of the model to reproduce the nutrient values along the Mediterranean Sea gradient and the relative minimum of the oxygen concentration below 400 m.

Regarding oxygen, the depth of the relative minimum of oxygen displayed in Fig. 5 is consistent with published information: according to Tanhua et al. (2013; see their Fig. 6), the oxygen minimum layer (OML) core in Mediterranean is located at 500 - 700 m in the eastern basin, "well below the layer of maximum S occupied by the LIW"(see sub-basins ion2, ion3 and lev4 in Fig. 5), whilst in the western basin it is shallower at around 400 m depth (see sub-basin nwm in Fig. 5, and other sub-basins are in Cossarini et al., 2018).

On the other hand, BGC-Argo floats allow to observe relatively fast dynamics (i.e., at weekly time scale according to their current 5-day sampling frequency) which are relevant for the biogeochemical dynamics in the upper layer. Thus, we used BGC-Argo floats data to verify the model capability to reproduce the mesoscale and vertical dynamics in the upper layer at the weekly time scale. This represents a very rigorous comparison, which allowed to demonstrate the timing and intensity of the evolution of signals that track biogeochemical processes in the upper layer. Therefore, we think that it would be not enough illustrative to compare and show deep profiles for model and float, also considering the ongoing and progressing advancements of the product quality practice for BGC-Argo floats data. In fact, only very recently a new product quality procedure (following Bittig et al., 2018 and Thierry et al., 2018) has been started to be implemented for oxygen to correct biases on sensor, but up to now it is not yet available for all floats in the Mediterranean.

Thus, we prefer to keep the maximum depth of Figs. 10 and 11 to 300 m to better exploit the model-float visual comparison in the upper layer (roughly identified with the euphotic layer, generally not deeper than 200 m), where most of the biogeochemical processes occur. We propose to revise the colorbar of Fig. 11 starting from oxygen value at 180 mmol/m^3: the new Figure 11 (see Fig.R3) displays some discrepancies (i.e., a bias of about 10-20 mmol/m^3) that were already commented along with Table 5. However, given the aforementioned ongoing quality improvements in the BGC-Argo float oxygen data, a formal quantification of the discrepancy between model and floats (as done for the chlorophyll and nitrate metrics) has not been defined yet, and we proposed only qualitative considerations, as reported at (P12, L10-12). In the revised manuscript, we will better clarify the limitations of the BGC-Argo data use for oxygen.

Finally, we would like to point out that with improving oxygen data quality, the use of BGC-Argo floats to investigate quantitatively the simulated oxygen will become very promising for evaluating surface and deeper layer dynamics.

A comment on the agreement with historical data (Tanhua et al., 2013) and on the improvement of the oxygen validation framework will be added in the discussion regarding future developments.

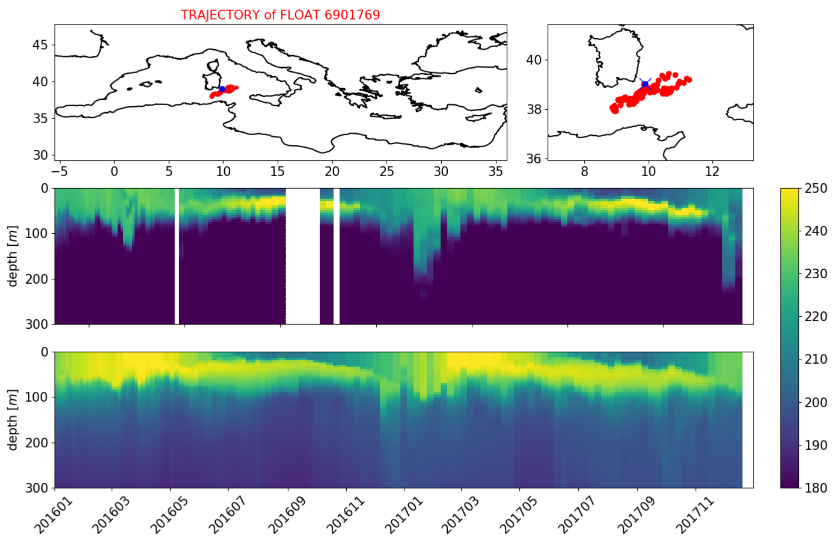


Fig. R3 - Revised Fig. 10: Hovmoller diagrams of oxygen concentration (mmol/m^3) of one selected BGC-Argo float 6901769 (top panels) and model outputs (bottom panels) matched-up with float position for the period 2016-2017.

13. P12 L21: I would say 'solubility' instead of 'saturation'

REPLY 13 –Thanks, the term "solubility" will be used in the revised version.

14. P12 L22: What are the consumption terms?

REPLY 14 – Comsumption terms are defined as respiration terms by bacteria and plankton community (4 phytoplankton and 4 zooplankton groups).

15. Fig 12: needs to be improved (quality). Do you compare to the daily NRT L3 chlorophyll product? The figure seems to be at a coarser temporal resolution. Moreover, the yaxis labels don't appear correctly (they are truncated). I don't understand why the red curve is constant during the summer.

REPLY 15 – We agree about the poor quality of Fig.12: we will provide an improved version of it in the revision (see a proposition of a revised panel of Fig. 12 in Fig.R4). The comparison is made, for each week, between the first 3 days of forecast (black T1, blue for T2 and green for T3) and the corresponding daily NRT L3 satellite product (as written at P13-L10). The red line represents the seasonal benchmark defined as the mean RMSD shown in Fig.8. Thus, the 2 seasonal benchmarks should inform on the average quality of forecast for the 2 periods.

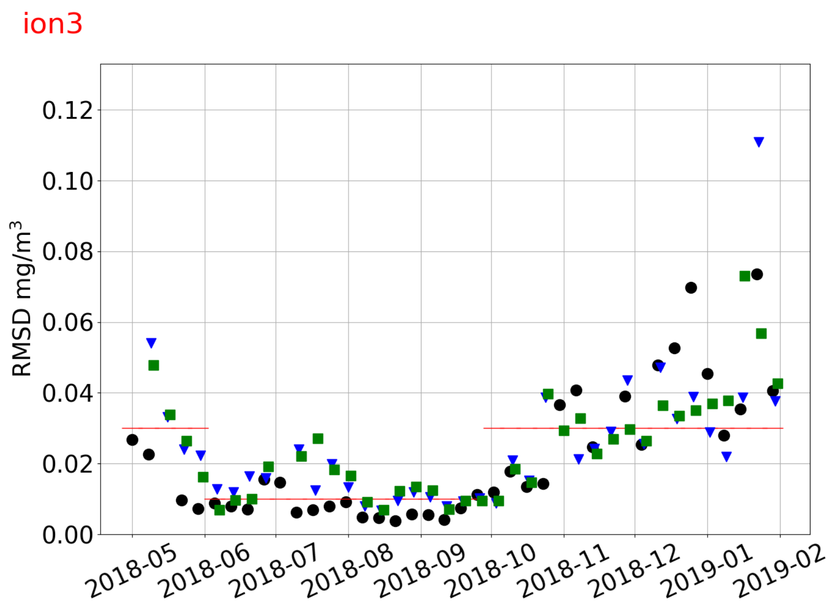


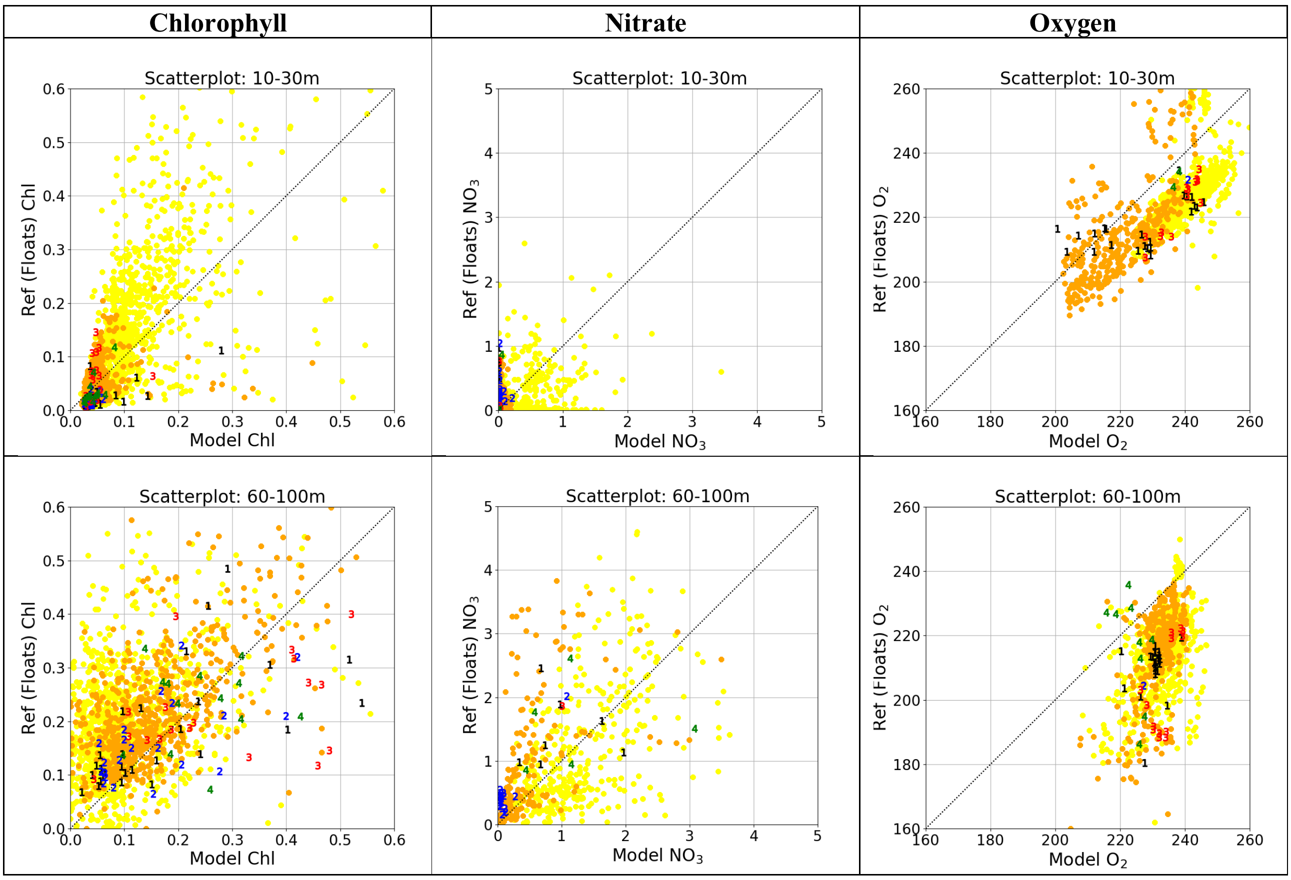
Fig. R4 - Example of new panel of Figure 12 for sub-basin ion3. The new caption is "Sub-basin RMSD between surface chlorophyll model forecast at lead time 24 (T1, black dots), 48 (T2, blue triangles) and 72 hours (T3, green squares) and daily satellite maps. As benchmark reference, the two seasonal mean RMSD values computed from 2016-2017 pre-operational run are shown (red line)".

16. P13 L15: 0.041 instead of 0.41 mg/m3?

REPLY 16 – Thanks: yes, it is 0.041 mg/m3.

17. P13 L18-20 and Fig 13: Authors don't comment much the Fig13. For example, the nitrate dots are very scattered (between 0 and 100m depth), do you think that it is due to nitrate sensor anomalous values or is it a problem in the physical or biological models? Oxygen panels display a bias (model overestimates float data): do you think it is a bias in the model or in the data?

REPLY 17 – We acknowledge the reviewer for this point: in the submitted version, the comment to Fig. 13 is at (P13, L18-L28) but we can enrich it further. As we pointed out at (P16, L27-28), and also thoroughly commented at Reply point n. 12, the systematic bias for oxygen at depth can be either due to model uncertainty and data. Only very recently, new product quality procedure (following Bittig et al., 2018 and Thierry et al., 2018) has been starting to be implemented but up to now not available for all floats in the Mediterranean.
On the other hand, concentrations of oxygen lower than 180 mmol/m^3 in subsurface layer (1-150 m) appear quite anomalous for the Mediterranean Sea (see for example Tanhua et al., 2013).
An in-depth analysis of the oxygen vertical profile dynamics is therefore preliminary. The main message we would like to convey here is the methodological approach that can be used to keep monitored the NRT forecast w.r.t. to a benchmark provided by a past simulation.
Regarding nitrate, the scatter plot at 60-100 m erroneously repeated the one for chlorophyll: we report here the corrected version (Fig. R5), where we observe that the operational forecasts are generally in line with the seasonal benchmark (i.e. most of the numbers are within the orange points cloud).



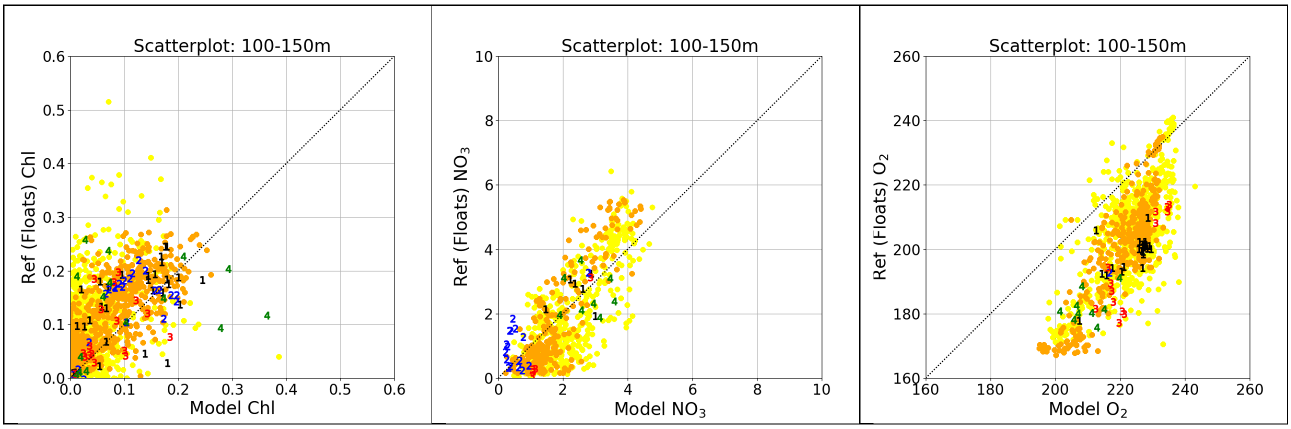


Fig. R5 – Corrected version of Fig. 13: scatter plots of reference (y-axis) versus model forecast (x-axis) for chlorophyll (left column), nitrate (middle column) and oxygen (right column) at different vertical layers: 10-30 m, 60-100 m and 100-150 m. Model forecast are labelled with numbers from 1 to 4 corresponding to lead time from T1 to T4. As benchmark reference, the 2016-2017 pre-operational results are shown for the period of investigation (May to August, orange dots) and for the other periods (yellow dots).

18. Table 6: I am wondering why the chlorophyll RMSD for the pre-operational is twice as large as the one for the T0 forecast. Do you have an explanation for the nitrate RMSD decrease from T0 to T3?

REPLY 18 – The limited observations available during the operational period (less than 5 per week) may hinder the statistical significance. In particular for chlorophyll, the qualification period spans 2 years, while the operational one here considered ranges from April to October, when the model errors are much lower (see Fig. 4).
For what concern nitrate, the decrease of its RMSD is related to the limited amount of data available (BGC-Argo floats data may exhibit wide oscillations over subsequent profiles, as shown in Fig. 10).
We will add a comment on this issue, stressing also that robust statistics require much longer time series of data and a larger number of BGC-Argo floats, which is becoming an urgent request for the observing systems to be used in operational biogeochemical oceanography (for both validation and assimilation purposes).
However, the important aspect here is that the quality of biogeochemical variables in the first 4 forecast days stays within a range of 25%, which allows to conclude that the quality of biogeochemical forecast does not degrade and remains satisfactory (i.e., in line with the benchmark) during the first week. This will be better commented at P13-L25 by detailing that "No significant differences can be recognized from the distribution of the four forecast days, showing that the quality of biogeochemical forecast does not degrade during the first week".

Minor/technical comments will be also thoroughly addressed in the review.
Best Regards

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