We wish to thank Pr. Marcello Vichi for offering many insightful comments and helping us clarify our results. Here we offer detailed responses to all questions. Reviewer's comments are in black, our replies are in blue.

General Comments:

This manuscript is indeed a valid compendium of diagnostics for assessing global ocean ecosystem models, which has been prepared with the aim to demonstrate the use of the multi-disciplinary dataset made available by the BGC-Argo array. The authors should thus be praised for their intention to bring together the community and follow the steps taken by Russel et al. (2018). However, that paper had different entry points, since it was specifically dedicated to a poorly sampled oceanic region and offered a multi-model analysis. This manuscript is well written and constructed, but only conveys a demonstrative message. I am thus not fully convinced by the scope of this present version of the manuscript, as well as by its effective novelty, since it does not add further knowledge to the existing literature [...]
Hence, I have carefully thought about how to write this review, and realised that the most relevant point of clarity would be to illustrate some cases of how readers could approach it. From a point of view of someone approaching modelling validation as a student or early career researcher, this manuscript offers a limited perspective, and one would gain more theoretical and methodological background in the 2009 JMS special issue (Lynch et al., 2009, and all the other papers in the issue), if not from earlier papers in the ecological modelling literature (Oreskes et a', 1994; Rykiel, 1996). If a reader is interested in the validation of the global version of PISCES, this manuscript is insufficient, because it provides a series of figures with few comments and discussions. It is surely of interest to the PISCES developers who are knowledgeable of the model details and possible deficiencies, but then an internal report would suffice. Finally, for experienced global ocean modellers, this manuscript is an illustration of the minimum set of assessments (which I prefer to the term "validation") that serious modellers have been doing in the last ten years when evaluating their model results. In terms of "metrics", it gives indications to compare the model output against the state variables that can be measured by the array of floats and to add derived state variables from applications of artificial intelligence. Ultimately, the assessment is based on visual comparisons of coarsely gridded spatial maps and time series, or through the use of basic univariate scores (bias and RMSD) and cumulative diagrams that combine the same skill scores (e.g. the Taylor diagram, which also includes linear correlation).

REPLY: Thanks for the careful assessment of our work. The goal of this paper is to demonstrate the use of BGC-Argo floats for the evaluation of BGC models at the global scale, through a concise evaluation of the CMEMS global BGC forecasting system. Our hope is that the methodology employed in this study can be useful and informative for other research teams interested in model assessment with BGC-Argo floats. In particular, the main points we want to highlight are: 1) how do we handle BGC-Argo data (e.g., quality control and flags) for model assessment purposes, and 2) to propose BGC-Argo metrics, which we believe are useful to assess the accuracy of the model state. We have intentionally chosen simple metrics, a minimum set of assessments and basic quantitative techniques (visual inspection, bias and RMSE) to focus the message of the study on the 2 points listed above and not on the evaluation of the model simulation. Therefore, this study is not designed as a review of biogeochemical models validation and it does not represent a thorough assessment of PISCES either.

We agree with the reviewer that the main message conveyed by the manuscript is not clear enough and that it can be confusing for the reader. Based on the reviewer's comments, we will modify the manuscript so that the main message of the study appears more clearly to the reader.

First, we will change the title to "*Using BGC-Argo floats for the assessment of marine biogeochemical models : a case study with CMEMS global forecasting system.* "

In the abstract, P1, L-28, we will change to "*Here, we demonstrate the use of the global array of BGC-Argo floats for the assessment of biogeochemical models through a concise evaluation of the CMEMS global forecasting system. We first detail the handling of the BGC-Argo data set for model assessment purposes, then we present 18 assessment metrics to quantify the success of BGC model simulations. The metrics evaluate either the model state accuracy or the skill of the model in capturing emergent properties, such as the Deep Chlorophyll Maximums (DCMs) or Oxygen Minimum Zones (OMZs). These metrics are associated with the air-sea $CO_2$ flux, the biological carbon pump, oceanic pH and oxygen levels. We also suggest four diagnostic plots for displaying such metrics.*"

In the introduction, the paragraph starting P. 4, L2 , will change to " *We aim to demonstrate the use of the BGC-Argo global array for the assessment of BGC models at the global scale. To that end, we performed a concise evaluation of CMEMS global BGC forecasting system using the global fleet of BGC-Argo floats. We expect that the methodology employed here (from the data handling to the use of assessment metrics) would be useful and informative for other research teams interested in model evaluation with BGC-Argo floats.* "

The BGC-Argo data are certainly invaluable, and this is the reason why the community has strived to develop the technology and the financial support to deploy them. The authors did not however succeed in showing their enhanced value for model assessment, beyond the obvious consideration that this increases the number of data, which would be much more evident if this same assessment was done by comparing datasets with and without the contribution of the BGC-Argo.

REPLY: The reviewer brings up an interesting point. It is true that BGC-Argo dramatically increases the availability of data collected by traditional oceanographic cruises. It would indeed be informative to repeat the same assessment by comparing datasets with and without the contribution of the BGC-Argo, such as for example the World Ocean Atlas. While we are very interested in this question, we do not think it belongs to this paper whose main focus is to show the use of BGC-Argo floats for model assessment rather than showing the impact of increasing the number of observations on skill scores.

In summary I have found two major issues with this manuscript that the authors have not considered to a satisfactory extent:
The loose definition of metrics and the absence of uncertainties' treatment. The authors use the term metrics in a rather ambiguous way. They also do not differentiate between measured data and artificially generated data. This implies that the evaluation process does not necessarily lead to an improvement of the model(s).

REPLY: We agree with the reviewer that our definition of metrics was somewhat ambiguous. In the introduction, we will change our definition of metrics based on the recent review of Hipsey et al. (2020):

"*In this study, the BGC-Argo dataset is used in conjunction with the model evaluation framework developed by Hipsey et al. (2020). In particular, they propose three levels of assessment metrics to evaluate the skill of a model simulation: state variables validation (e.g., Chla, nitrate, oxygen, etc…), mass fluxes and process rates validation (e.g.,primary production or division rates), and emergent properties validation (e.g., Deep Chlorophyll maximum, or Oxygen Minimum zones). In this study we present 18 metrics for the assessment of a model simulation with BGC-Argo data. Most of them evaluate the model state accuracy through the comparison of simulated state variables with BGC-Argo observations in the mixed layer or at fixed depth. In addition, some of the metrics assess the skill of the model in capturing emergent properties. These metrics are associated with the air-sea $CO_2$ flux, the biological carbon pump, oceanic pH, oxygen levels and Oxygen Minimum Zones (OMZs). Recent works demonstrated*

*the feasibility of calculation at basin scale, from BGC-Argo observations, of mass fluxes and process rates, such as primary production, phytoplankton division and accumulation rates (Yang et al., 2021; Mignot et al., 2018), net community production (Plant et al., 2016), or carbon export (Dall'Olmo et al., 2016). However, it would be arduous to achieve such estimations on the global BGC-Argo dataset as it requires ad hoc calibration that cannot be easily defined. As a consequence, the evaluation of simulated process rates with BGC-Argo data is not addressed in this study*."

In reply to the second comment, as we explain above, the object of the paper is not a thorough analysis of the model performance. Nevertheless, the proposed concise evaluation of the model (e.g., maps of rmsd) can be further exploited (e.g., by analysing the spatial and temporal distribution of the rmsd maps or multivariate relationships of the errors) to study the model uncertainty sources.

Last , we agree with the reviewer that we do not provide justification for mixing together measured data with artificially-generated data. We will add a paragraph in the Data section that justify our choice.

" *Finally, we complemented the existing BGC-Argo dataset with pseudo-observations of $NO_3$, $PO_4$ , Si, and DIC concentrations as well pH and $pCO_2$ using the CANYON-B neural network (Bittig et al., 2018). CANYON-B estimates vertical profiles of nutrients as well as the carbonate system variables from concomitant measurements of floats pressure, temperature, salinity and $O_2$ qualified in "Delayed "mode together with the associated geolocalization and date of sampling. The CANYON-B estimates of $NO_3$ and pH were merged with measured values on the rationale that CANYON-B estimates have RMS errors (NO3 = 0.7 µmol/kg , pH= 0.013) (Bittig et al., 2018) which are of the same order of magnitude than the BGC-Argo observations errors (NO3 = 0.5 µmol/kg , pH= 0.07) (Mignot et al., 2019; Johnson et al., 2017). We also verified that RMS errors of CANYON-B estimates are at least 4 times lower than the RMS difference between the model and CANYON-B estimates, so that the comparison of simulated properties with the neural network estimates leads to an evaluation of the model performance. We believe it is reasonable to draw conclusions on the model uncertainty from CANYON-B estimates as long as the pseudo-observations errors are much lower than the model-pseudo observations RMS difference. However, caution should be considered when errors are comparable.*"

The unconvincing enhancement of the effective role of BGC-Argo data in model assessment. Basically, the question I have is: why BGC-Argo are good enough and should be used separately and not as part of a global compilation of data such as the World Ocean Atlas? (which incidentally includes or will include the BGC-ARgo data).

Since BGC-Argos are ultimately increasing the availability of data that are usually collected by means of traditional oceanographic cruises, what is indeed their value in model validation?

REPLY: We thank the reviewer for bringing this to our attention. When we wrote the first version of the manuscript, we did not know that the BGC-Argo data were available from the World Ocean Database (WOD). We have examined the documentation that deals with the data processing in the WOD (https://www.ncei.noaa.gov/sites/default/files/2020-04/wod_intro_0.pdf) but we haven't found sufficient information concerning the data mode used in the WOD. As we detail in the manuscript, the "Delayed-mode" represents the highest quality of data but for some variables, only a limited fraction of data is accessible in "Delayed-Mode". Consequently, for each variable, we selected the highest quality of data (i.e., "Adjusted" or "Delayed mode") that did not compromise too much the number of observations available. We are not sure whether such data selection is possible with the World Ocean Database, so we prefer to use the BGC-Argo data directly downloaded from Argo Coriolis Global Data Assembly Centre and not as part of a global compilation of data.

Furthermore, one of the issues of large databases such as WOD, is the accessibility and the interoperability of the data that compose it, which, ultimately, affects their overall accuracy. Using the BGC-Argo dataset separately is a way to ensure consistent accuracy. The GLODAP V2 data set (on which CANYON B is developed) is an illustration of an interoperable homogenous data set (with very strict data QC procedure) used for model assessment and not used as part of a global compilation of data.

Finally, in reply to the last question, the BGC-Argo floats provide observations at high vertical and temporal resolutions and for long periods of time allowing to compute time-series of vertical characteristics of the variables. This is not possible with discrete vertical samplings provided by cruise cast *in situ* measurements..

We will comment on these points in the revised version of the manuscript.

For clarity, I would like to elaborate more on the first concept above, while the second point is mostly derived from the specific comments detailed in the next section. Russel et al (2018) also use the concept of metrics in a wider sense, although they define metrics as "any quantity or quantifiable pattern that summarizes a particular process or the response in a model to known forcings". The strength of the ACC transport at Drake Passage or the latitude of the maximum zonal mean winds over the Southern Ocean

are “metrics” in this context. They are combinations of state variables, or values of state variables at specific locations.
In this context, all the surface state variables listed in Table 2, are indeed components of the biological carbon pump, but they are not metrics. They are simply state variables. Only when considered together to evidence emergent patterns they may give indications of proper process functionality (e.g. the ratio of particulate organic carbon to total chlorophyll, de Mora et al, 2016). I agree that the DCM and the “nutricline” (which would deserve a more appropriate definition, see specific points below) are “metrics”, as well as the depth of the hypoxic layer. Mixing together indicators of processes with state variables is confusing, unless a rigorous link between a single state variable and the process is established.

REPLY: As we explain above, we have changed our definition of metrics. We now use the framework proposed by Hipsey et al. (2020). They propose three levels of assessment metrics to evaluate the skill of a model simulation: state variables validation (e.g., Chla, nitrate, oxygen, etc…), mass fluxes and process rates validation (e.g.,primary production or division rates), and emergent properties validation (e.g., Deep Chlorophyll maximum, or Oxygen Minimum zones). We will indicate in Table 2, which level a proposed metric is referring to. We will also make a rigorous link between the state variable and the associate process.

This manuscript increases the risk of misinterpretation by mixing together “metrics” and skill scores. Neither Russel et al (2018) and this manuscript expand on the concept of metrics performance and objective assessment (performance indicators, skill scores, cost functions, are all synonyms that depend on the specific discipline), which was instead done by Allen et al. (2007), Friedrichs et al. (2009), Vichi and Masina (2009) and others in the JMS special issue. For ease of simplicity, I will use the term skill score, which is the one used in the more mature field of weather forecasting. State variables can be assessed using univariate skill scores, and this is a necessary exercise for any modeller to ensure the model has some grip with reality. Figure 3 and the other density plots in the Appendix give a visual indication of the skill score, but they do not quantify it (e.g. Smith and Rose, 1995; Rose and Smith, 1998). I also have another question linked to my Point 2 (and further detailed in the specific comments): why should this exercise be done only with the BGC-Argo and not also including the other existing data? Since BGC-Argo are evaluated against cruise cast benchmarks, then those data are usually considered always superior, and should be used. Again, the real value of the BGC-Argo would have been shown if the score had been substantially modified with the inclusion of the Argo data.

REPLY: We will add a Table that quantifies the skill scores for each metrics as done in Vichi and Masina (2009) or Doney et al. (2009). As we explain above, we believe it is

more reasonable to use the BGC-Argo data as a separate dataset rather than as part of a global compilation of data.

Specific comments:

P2L1 - Earlier work has specifically addressed the impact of assimilation on the carbonate system (Visinelli et al., 2017)

REPLY: We will add the reference in the revised version of the manuscript.

P2L26-29 - This sentence is mixing together sensor accuracy, which has been assessed by Johnson et al and Mignot et al, in two specific regions of the world ocean) and temporal/vertical resolutions, which have not been assessed as far as I am aware. This is misleading. 10 days may not be sufficient for all variables, as well as the vertical binning that is done. The comparisons have assessed the equivalence between rosette casts and the floats, but they say nothing about the temporal and vertical resolution. For certain processes, such as carbon exchange and phytoplankton biomass through chlorophyll and backscattering proxies, a resolution of 10 days would lead to sampling aliases either of the mean or of the variability (Monteiro et al., 2015, Little et al., 2018). These are examples from the Southern Ocean, where there is the highest density of buoys.

REPLY: We will revise the sentence and we will remove the part about the temporal and vertical resolutions.

P2L32-34 - The authors should be more specific. Other datasets, such as for instance remote sensing, are less limited in terms of temporal and spatial resolutions. This is connected to the concerns expressed in Point 1 above.

REPLY: We will revise the sentence, and we will be more specific about the temporal and spatial resolutions.

P4L3-5 This sentence seems to imply that one can only perform point-by-point comparisons when there are few floats, which is odd. Again linked to my main Point 1 above. The authors should explain why given the current computing capability, they only suggest to perform diagnostics for few selected tracks and not for the overall dataset (Section 5.d).

REPLY: We have changed this paragraph based on point 1 and point 2 (see above). This sentence will be removed in the revised version of the manuscript.

P4L12-16 The connection between the variables and the ocean health/ecosystem functioning is not made explicit in the text. Taking as an example the ocean health index (http://www.oceanhealthindex.org/), establishing ocean health is obtained as a multivariate analysis of several data layers, forming a selected set of drivers and their associated thresholds. The authors should be more explicit about their intent here.

REPLY: We have changed our definition of metrics. We will no longer refer to ocean health and ecosystem functioning in the revised version of the manuscript.

P5L12-13 This is not an objective criterion. What is an acceptable level of compromise?

REPLY: We have added an objective criterion to the paragraph: *" However, for some variables, only a limited fraction of data is accessible in "Delayed-Mode". Consequently, for each variable, we selected the data modes, where at least 80 % of the data are available (see Table 1). Note that this criterion does not apply to $O_2$, where only delayed mode data were selected in order to generate the pseudo-observations from CANYON-B neural network (see after). "*

P5L22 There are many other relationships, and they have been shown to give different results (e.g. Thomalla et a., 2017l). The authors should explain why they are recommending this one.

REPLY: In the revised version of the manuscript, we will use a POC vs $b_{bp}$ relationship developed for the global ocean (https://catalogue.marine.copernicus.eu/documents/QUID/CMEMS-MOB-QUID-015-010.pdf) based on a global database of in situ POC and satellite $b_{bp}$ (Evers-King et al., 2017). This relationship, developed for global application, has been shown to outperform regional relationships, such as Cetinic et al. (2012), at global scales.

P6L12-15 It appears that this method of linear resampling would artificially increase the number of data, and hence bias the statistical results, especially in conditions where there are not enough data.

REPLY: This is a good comment. We will add that the method of linear resampling can possibly bias our statistical results.

P7L10-12 The authors do not discuss what would happen if the MLD is different between the observations and the model.

REPLY: In this study, the dynamical component has been extensively validated (Lellouche et al., 2013, 2018), and correctly represented variables that are constrained by observations (e. g., temperature and salinity), including Argo profiles. We verified that the MLD, which is calculated on a density criterion basis, is indeed correctly represented in the model. The global bias between the model and the BGC-Argo observations is 0.3 m. We will add a sentence that specifies that we verified that the MLD is well simulated by the model.

P7L29-30 Related to my point 1 above. The relationship between the state variables and the ecosystem functions is not made explicit. The term “useful” should be motivated.

REPLY: We will revise this section, and we will make the relationship between the state variables and ecosystem function more explicit. Note that, we will add new metrics in the mesopelagic layer as explained below.

“*The biological carbon pump is the transformation of nutrients and dissolved inorganic carbon into organic carbon in the upper part of the ocean through phytoplankton photosynthesis and the subsequent transfer of this organic material into the deep ocean. The functioning of this pump relies on key pools of nutrients and carbon as well as a number of processes that control mass fluxes between the pools.*

*The first level of assessment of a biological carbon pump simulated by a model consists in evaluating the different pools (or state variables) of the pump (Hipsey et al. 2020). In particular, the comparison of simulated surface nutrients ($NO_3$, $PO_4$, and Si), DIC, Chla and POC with BGC-Argo observations gives an indirect evaluation to demonstrate the model is capturing key processes of the biological carbon pump in the upper layer of the ocean, such as primary production, respiration, grazing. A second-level , and more indicative, assessment would be to directly compare these key processes with measured mass fluxes, but this is not addressed in this study. The surface nutrients , DIC, Chla and POC (hereinafter denoted $sNO_3$, $sPO_4$, sSi, sDIC, sChl and sPOC) correspond to the average concentrations in the mixed layer.*

*Similarly, the evaluation of the mesopelagic nutrients, DIC and POC concentration (hereinafter indicated with the subscript $_{meso}$) provides an indirect evaluation of the key processes in the mesopelagic layer, such as export production, respiration,etc. The mesopelagic concentrations correspond to the depth-averaged concentrations between the base of the mixed layer down to 1000 m.* ”

P8L7-8 Same as above, the value of DCM as an indicator should be contextualized. Why are BGC-Argo data providing a better estimate of this metric than other data?

REPLY: We will revise the paragraph and we will contextualize the use of the DCM as an indicator. "*At the base of the euphotic layer of stratified systems, a Chla maximum (hereinafter denoted Deep Chlorophyll Maximum, DCM) develops that generally escapes detection by remote sensing (Barbieux et al., 2019; Cullen, 2015; Letelier et al., 2004; Mignot et al., 2011, 2014). It has been suggested that the DCM plays an important role in the synthesis of organic carbon by phytoplankton (Macías et al., 2014). DCMs are therefore important features to be assessed in BGC models with respect to biological carbon pump processes such as the primary production. Furthermore, DCMs are also an emergent feature that develops in response to complex physical and biogeochemical interactions (Cullen, 2015). Thus, their evaluation provides critical information regarding the accuracy of the model in capturing complex patterns of key ecosystem processes.* "

As we explain above, the BGC-Argo data provide consistent profiles at high vertical and temporal resolution allowing to derive time-series of DCM depths. In comparison, discrete vertical samplings provided by cruise cast *in situ* measurements have a vertical resolution much lower (10 samples taken over a 100 m layer ), with no repetitive sampling.

P8L13 Please explain what H is.

REPLY: It is an omission on our part. H is the mixed layer depth. We will replace H by MLD.

P8L14-16 This may be confusing for some readers, since it's not technically a gradient. The cited paper uses and justifies this definition. I'd suggest the authors to be more precise and give their definition and how this is an effective metric of the carbon pump. Also, there is a difference in sampling between argo and the layers of discrete models. How is this taken into account?

REPLY: We will be more precise about the definition of the nitracline depth and describe how this is an effective metric of the carbon pump.

"*The vertical supply of $NO_3$ to the surface layers is a critical process of the biological carbon pump as $NO_3$ is often depleted in the surface layers and is a limiting factor for phytoplankton photosynthesis in most oceanic regions. This flux depends, among other things, on the vertical gradient of $NO_3$ (the nitracline), and, in particular, its depth (the*

*nitracline depth)* (Cermeno et al., 2008; Omand and Mahadevan, 2015)*. Therefore, the comparison of the simulated nitracline depth with BGC-Argo observations allows for an indirect assessment of the model quality in reproducing vertical fluxes of $NO_3$. Following previous studies* (Cermeno et al., 2008; Lavigne et al., 2013; Richardson and Bendtsen, 2019)*, the depth of the nitracline corresponds to the first depth where $NO_3$* is detected. *The threshold value is set to 1 µmol/kg, which corresponds to an upper estimate of BGC-Argo $NO_3$ data accuracy* (Johnson et al., 2017; Mignot et al., 2019). "

Finally, there is indeed a difference in sampling between the BGC-Argo and the layers of discrete models. This is clearly visible in the scatterplot for the nitracline , the DCM and the OMZ depths. We will comment on this point in the revised version of the manuscript.

P8l28-30 At P4L11 it is reported "depth of the OMZ". This the depth of the oxygen minimum. It should be explained how and why this is a good indicator, and why the BGCArgo data are superior in its identification.

REPLY: We will explain in the revised version of the manuscript, why the depth of the oxygen minimum is a good indicator. "*Oxygens levels in the global and coastal waters have declined over the whole water column over the past decades (Schmidtko et al., 2017)and OMZs are expanding (Stramma et al., 2008). Assessing how models correctly represent ocean oxygen levels as well as the OMZs is therefore critical to monitor their changes over time. Similarly to DCMs, the assessment of OMZs is also informative on how the model simulates emergent dynamics as OMZs originate from intricate physical and biogeochemical interactions (Paulmier and Ruiz-Pino, 2009). "*

We detail in a previous reply, why the BGC-Argo are particularly fit in the identification of vertical characteristics of BGC variables.

P9L26 This statement about non-linearity is odd in the context of model goodness-of-fit (Smith and Rose, 1995; Pineiro et al, 2008; Vichi and Masina, 2009). If it's non-linear, then the assessment is failed.

REPLY: We will remove this sentence.

P10-8-12 The choice of the binning interval should be discussed. What is the advantage of losing the variability measured by the floats? Why not using the standard deviation as an indicator of the model skill to reproduce the proper scales? These are enhanced features that only the BGC-Argo data would allow to compute.

REPLY: We will discuss the choice of the binning interval in the revised version of the manuscript. "...*To do so, the metrics from 2009 to 2017 are averaged in 4°x4° bins, bins with less than 4 points being not included. The 4° distance in an upper estimate of the autocorrelation length scales for $O_2$, nutrients, and $pCO_2$ (comprised between 300 and 400 km) between 20° and 40° of latitude in both hemispheres* (Biogeochemical-Argo Planning Group, 2016)*.*"

We will also add in section 4.c that standard deviation can also be displayed on spatial maps as an indicator of the model skill to reproduce the proper scales. However, we won't show it in the manuscript as we prefer to not overload Figure 4 and the associated supplementary figures with additional panels.

P10L22-24 Allen et al (2007) warned against the visual comparison of time series. This sentence is generic and should be explained in the context of the augmented data provided by the BGC-Argo.

REPLY: We agree with the reviewer that visual inspection relies on the subjective appreciation of the evaluator. Consequently, we will add time-series of normalized skill scores to Figures 5 and 6. We will add this sentence at the end of the section 4c. " *In addition to the time series of metrics, we also displayed time series of normalized skill scores such as percent BIAS and RMSD to avoid relying only on subjective visual inspection*. "

P11L11-14 The results are not presented according to the concept of the biological carbon pump "metric". It is evident that the nutrients are correlated while all carbon flux variables are not performing. Which ultimately questions the use of surface nutrients as indicators of carbon cycling.

REPLY: The fact that nutrients are well represented in the model suggests that the model captures the combination of process rates that drive nutrients dynamics. Some of these process rates drive both the nutrients and carbon dynamics, but there are also rates that are specific to each state variable. This probably explains why the carbon variables are not performing while the nutrients are well simulated. However, it must be recognised that without a direct assessment of the individual rates, we cannot verify this hypothesis. We will clarify this point in the revised version of the manuscript.

P11L31 I cannot see the data "around" the line. I rather see an overestimation. (it is either Cape Verde or Cap Vert)

REPLY: We will improve the clarity of the figure in the revised version of the manuscript.

P12-L2-17 Linked to Point 2 above. The authors seem to imply that BGC-Argo data are more suitable than ocean colour for model assessment. I acknowledge that this is not explicitly written, but there is no clear rationale. This kind of map would certainly be superior in terms of spatial and temporal resolution when using that product as Benchmark.

REPLY: We do not imply that BGC-Argo data are more suitable than ocean colour for model assessment.

P12-section-d This is the section that mostly led to the inclusion of Point 2 above. The shown time series is 2 years long, which is an invaluable source of data from a region that has been influential in shaping our understanding of the spring bloom. I am missing the point why the authors are writing the term spring bloom in quotes. The advantage of time series from floats that remained in a given province of the global ocean is of huge potential in model validation. The offered description is quite generic, which could have been done even using monthly climatological time series obtained from the WOA, or from the existing long-term observational ocean sites (BATS, PAPA, HOT). The BGC-Argo floats are an unprecedented source of multiple opportunities to do validation in several regions of the world ocean (with some limitations), but this present form of the manuscript does not offer any specific recommendation of what numerical modellers should do to unleash this potential. I would be very interested in seeing an exploitation of the multivariate nature of BGC-Argo, while I only see multi-panel plots.

REPLY: Based on this comment, we will revise this section. We will remove the unnecessary description of the spring bloom. We will also highlight the invaluable opportunities of such time series for the assessment of models by showing other time series in regions where in situ data are scarce. Concerning the evaluation of the multivariate nature of BGC-Argo, we agree that it is an interesting point to pursue. We are very interested in applying the multivariate approach proposed by Allen et al. (2007) to the BGC-Argo data set. However, we prefer to focus this manuscript on the presentation of the metrics and to exploit the multivariate approach in another study.

P13L4-5 The authors should do more than simply say “correctly represented”. This is a subjective statement, which is based on a visual comparison, exactly what the community challenged in the last 10-15 years. The advantage is that now we can use a frequency of 10 days, when initially phenology analysis was based on monthly data. Again, the authors are missing an opportunity to demonstrate the intrinsic value of this new data set.

REPLY: As explained above, we will include time series of skill scores to avoid relying only on subjective visual inspection. We agree that the frequency of 10 days is a significant progress over previous data sets. However, as explained in the conclusion, we do not address phenology metrics in this study because the number of observations per month and per bins is still too low to perform a global analysis.

P13-L13-20 This is a more detailed analysis of this specific model, which indeed brings in some of the advantages of a multivariate data set. However, there is a combination of measured and derived variables, which are treated as if they were equivalent. Quite a few questions come to mind: Is there a possibility that there is artificial correlation in the derivation of the phosphate and silicate concentration? What is the error associated with the CANYON-B method? Which is the effective (measured) variable mostly responsible for the response of the other estimated nutrients? The reduced consumption occurs during the spring period, and is continued during summertime. Hence, there is a factor at play during the late spring period, which is less likely to be reduced uptake from smaller phytoplankton during summer as suggested. It may thus be a delayed onset of the phytoplankton succession, or maybe a faster remineralization occurring in the upper layers, which retain more inorganic nutrients closer to the surface. This may indeed be beyond the scope of the manuscript, but it has been the authors' decision to propose some mechanistic explanations of this discrepancy. Showing a complete example of how the use of multivariate data allows modellers to investigate model deficiencies would offer guidelines to other modellers.

REPLY: As explained above, we will include a paragraph in the Data section that discusses the error associated with the CANYON-B method. In reply to the second comment, we will also discuss the hypothesis proposed by the reviewer in the revised version of the manuscript.

P13-L22-23 This sentence bears lots of assumptions. This is really where BGC-Argo can make a difference. The related uncertainties should however be highlighted, together with recommendations to other modellers on how to best approach the assessment of the carbon cycle metrics.

REPLY: Based on the reviewer's comment, we will revise this paragraph. We will also provide recommendations on how to best approach the assessment of the carbon cycle metrics.

P13L26-29 This argument is flawed. If the occurrence of the peak is matched in the

mesopelagic layer rather than at the surface, it is a clear indication of vertical mismatches in the export. I would thus argue that POC concentration is a proper metric for the export component of the carbon cycle. I would again encourage the authors to replace the use of subjective terms such as “consistent” with objective indicators (see Allen et al., 2007). For instance the comparison of the skill score computed in two consecutive years would give indication if there is some variability or if the model tends to repeat the same pattern.

REPLY: We will revise this paragraph in the revised version of the manuscript and we will compute time-series of skill scores.

P14L16-19 I would recommend more clarity on this statement. Are these sensors not available on the global ocean floats? It is not clear why this example is presented for Mediterranean floats, and not introduced earlier as one major advantage of the BGC-Argo floats.

REPLY: We will clarify this statement. We will also add that the sensors are available on the global ocean. However, the global model used in the study does not resolve the spectral and directional properties of the underwater light field. That’s why we didn’t use the global model but a model of the Mediterranean Sea equipped with a multispectral light module. We will also clarify this point.

P14L26-28 This sentence is similar to the statements done in the earlier sections. This is not technically a perspective statement.

REPLY: We will add a perspective statement in the revised version of the manuscript.

P15L1-6 The question is whether these data should be used “on their own” or in conjunction with the other existing datasets. The authors should clearly explain in the conclusion why this dataset should be exploited as a separate unit.

REPLY: Based on our previous replies to this comment, we will explain in the conclusion why this dataset should be exploited as a separate unit.

P15L32-P16L3 I would thus recommend the authors to thoroughly address the issue of how the uncertainties should be treated. This is particularly important in the case of mixing measured and derived variables. If BGC-Argo are capable, within their limits, to reduce uncertainties in model assessment exercise, this should be adequately

argumented. The fact that there are more data available is undoubtedly of relevance, but I wonder if it does help to reduce uncertainties in model states.

REPLY: We verified that the RMS errors of BGC-Argo data are always lower than the RMS difference between the model and BGC-Argo observations, so that the comparison of simulated properties with the observations leads to an evaluation of the model performance. We will detail this point in the conclusion.

P16L15-18 Please highlight in which part of the results this is shown.

REPLY: We will highlight in which part of the results this is shown.

P17L2 Please add in the caption the meaning of the codes (or a link to where they are explained more in detail). Also, in the heading of the 3rd column, correct Date with Data. Figure 2 Taylor diagrams are based on geometric properties of the circle. Hence they should be presented using equal axes.

REPLY: We will add the meaning of the codes, change Date with Data and present the Taylor diagram using equal axes.

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