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Interactive comment on "Assimilating synthetic Biogeochemical-Argo and ocean colour observations into a global ocean model to inform observing system design" *by* David Ford

Anonymous Referee #2

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The manuscript by Ford presents an OSSE experiment to investigate a number of assimilation strategies for ocean colour (OC) and biogeochemical Argo (BGC-Argo) observations using an already published DA method. The simulations are performed using a global model and sets of synthetic observations that resemble the current L3 chlorophyll OC and two potential arrays of BGC-Argo based on the current Argo network.

The manuscript is well written and the performed modelling experiments allow novel and useful insights on the integration of BGC-Argo and OC data into global model assimilation. However, as presented, results seem rather superficial. The work would be

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the basis for a very valuable paper, but the results need to be explored further before I can recommend publication.Âă My main concern is that results present a single month of simulation (i.e., the last month of a 2-year simulation: 1-year spinup and 1-year assimilation) and a single global statistics (e.g., Figs. 3, 4 and 10). Conclusion/discussion on assimilation strategies and impact of the observing systems are possibly misled by the limited results. Results of the whole year of the assimilation runs should be presented and the MEAred maps and profiles enriched with spatial and temporal statistics to provide quantitative insights on the impact of BGC-Argo in different seasons and regions of the global ocean. In particular, which are the areas and seasons that could benefit most from BGC-Argo assimilation and the integration of the two observing systems? I feel that the manuscript misses the objective to provide useful indications to design future observing system strategy, as it is proposed in the title.

A second issue concerns the comparison between the PHY (phytoplankton update) and NIT (nitrogen balancing scheme) assimilation schemes. While the novelty of the OSSE experiment is related to the integration of OC and BGC Argo, the lack of the NIT implementation for BGC-Argo assimilation is a significant limitation of the manuscript that should be discussed. L425-427 are misleading. In fact, the choice of the PHY update scheme is explained at L229-230 (i.e., apparently, the NIT method has not been implemented for the joint BGC-Argo and OC assimilation). The joint OC and BGC-Argo assimilation strategy should be clearly described at L347-348. Then, it is not clear the objective of the first set of experiments (OC assimilation, which conclusions are mainly already published) if its results are not used for the second set of experiments. Even if the NIT method has not been implemented, the manuscript can be completed by a more unbiased discussion on pro and cons of the two methods and by presenting the work to be done and the the benefits to have the NIT method working with the BGC-Argo assimilation. In fact, some conclusions seem misleading. While the nitrogen balancing scheme is reported as the method with more potentiality (L406-407 in results and L510-L515 in discussion), results on BGC-Argo assimilation runs do not support this conclusion. I suggest to clarify better the proposed assimilation strategy and to detail better what would be needed to have the NIT method working with BGC-Argo assimilation. The conclusion that “only minimally altered for use with MEDUSA, so more specific tuning may help (L516-517)” seems misleading.

Minor points:

Line 173: why should the two methods provide similar increments? One is uniform with depth from surface to MLD depth, while the second method is not limited to the MLD and vertical increments are mediated by covariance.

Line 335: can the author provide some more details on how the observation and background errors have been matched? and which is the value of inflation?

Line 336: this sentence is not clear: the observation error is set from the real global BGC-float array, so what will change when the system is functioning with real BGC-floats? More generally, the discussion missed to tackle this topic: how much are the results affected by the selected observation and background errors? would it be a different impact of the two observing systems using different observation errors?

Table 3 can be enriched to improve the identification of the runs at a glance. I would suggest to add 2 new columns for the assimilated and updated variables, and to split the note column in two new ones: background error (i.e., vertical propagations: 2D and 3D) and type of increment.

L354-355: explain how MAEosse and MAEcontrol are computed for pixels in the maps of Figures 5-9 and for points of the profiles in Figures 3, 4 and 10. Which are the distribution compared?

L377: the absolute differences in Atlantic and Indian Oceans appear significant (i.e., 1 order magnitude). Can the author provide more details about which modifications of the FREE run (w.r.t. NATURE run) should have served to increase nutrient concentration? And which modifications compensate it (L382)? I agree that achieving a global appropriate level of error with a complex model (with uniform parameterization) can not

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be managed, however the author should provide some details on which modifications didn't work as expected. This can be helpful in understanding the effectiveness of the data assimilation in those areas.

L385: the absolute difference of NO3 and pCO2 between FREE and NATURE seems much lower than that between FREE and real observation. Can the author discuss which are the implications of the low difference for the OSSE assimilation results? I would argue that the effectiveness of the assimilation might be limited in some areas by the low differences between the synthetic observations generated from NATURE and the FREE run. Further, I would argue that MAEred might not be a good metric because of the MAEcontrol at the denominator in the areas where NATURE and FREE are so close.

L395: Since large areas of the global ocean are characterized by a DCM below 60m depth, the degradation of MAE below 60m would deserve a more detailed comment.

L405: why should the similar behaviours of OC_2D and OC_3D demonstrate that the use of NEMOVAR to create 3D increments for the combined OC and BGC-Argo float assimilation is fit-for-purpose?

Figure 4: I would suggest to reduce y-axis to 0-250m depth for the chlorophyll, phytoplankton and zooplankton plots to increase their readability. Are the high positive values below 250 in chla and phytoplankton plots due to the very low values of those variables below the euphotic zone?

L450-451 and L477-478: Can the degradation of Alkalinity be due to an improper working of the smallest combined change of DIC and Alk with pH assimilation? A more detailed analysis is expected to show the pro and cons of the method when pH is used instead of pCO2.

L454-L455: why are the negative values of MAEred in the Atlantic and Indian Oceans (Fig. 5) related to the compensating errors introduced in FREE? Since the FREE and

NATURE differences are very low in those areas (Fig. 2), the impact of the assimilating synthetic observations (generated from NATURE) should be negligible. I wonder whether the MAEred is not a good metric because of the MAEcontrol at the denominator for those areas.

Figures 8 and 10 seem redundant and not necessary. For example, the MAEred over OC_3D_PHY of ARGO_FULL_OCÂă(Fig. 8a and Fig 10a) provides the same information (except for the normalization of denominator) of the difference between MAEred over FREE of ARGO_FULL_OC and MAEred over FREE of OC_3D_PHY (Fig. 4a and Fig 5a and c). I suggest that the relative impact of adding BGC-Argo can be shown by a new table of the MAEred over FREE numeric values. The table can report values for selected regions and different seasons/months providing indications of which areas of the global ocean and periods of the year can benefit most by the BGC-Argo assimilation.

L505-508: which parameter settings between NATURE and FREE caused the degradation of the other variables? Can additional details be added?

L510-511 please explain: this seems not supported by results or references.

L522: BGC-Argo assimilation has a small and positive impact on O_2 as shown in Figure 10f (BGC-Argo assimilation). The degradation of O_2 at surface seems due to OC assimilation (see figure 4f).

L531-L535: DA method improvements are important, but the paper does not really tackle this aspect; thus, those lines may fit better in the introduction and not as the last conclusion.

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