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Ocean Science Discussions

Interactive comment on "Assimilating GlobColour ocean colour data into a pre-operational physical-biogeochemical model" *by* D. A. Ford et al.

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Thank you very much for your comments. We will address the points you raise in turn, quoting your comments in italics.

"I think that the power of the skill assessment presented in the paper is affected by the low number of independent field data used by the authors. This is relevant, because of the objective #2 of the paper. More data should be exploited. For example, Nitrate data were considered at just one area and two months (SeaBASS data measured in North-Atlantic, in April and May), which is a rather small dataset to assess the skill of

a global model. For example, why AMT data of nitrate were not used (but the authors used AMT data of Chlorophyll only)? A larger comparison with nitrate in situ data is relevant because the comparison with the nitrate climatology (Fig. 9) does not show noticeable impact of DA on the model estimates of the annual mean of surface nitrate (and DIC and alkalinity, as acknowledged by the authors)"

We agree that the small number of in situ nitrate observations used brings limitations when drawing conclusions, especially given the results of the comparison to climatology. We will acknowledge this more clearly in the manuscript. Unfortunately AMT nitrate data were not available to us for 2008, so these could not be used.

"Moreover, the authors should discuss what DA taught them on the deficiencies of the biogeochemical model and tell the reader which are the possible future improvements of the model structure and/or parameterization (e.g. introducing plankton functional types, modelling variable carbon:nutrient ratios?). This discussion would help future progresses in biogeochemical DA."

We will add this to the discussion section. Potential model improvements could come from better parameterising the growth, mortality and grazing rates of phytoplankton and zooplankton, either through changes to the model or through better tuning of the current model parameters (a proper tuning exercise has not been performed for the current configuration). A more complex light model could be implemented, which would take better advantage of the high resolution fluxes and explicitly resolve the diurnal cycle. This would also allow the observation operator to perform a more accurate comparison between the model and observations for which time information is available. Furthermore, the same light model could potentially be used for both the biogeochemistry and the physics, ensuring consistency.

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The inclusion of iron would be desirable in order to better capture the biogeochemical variability in the iron-limited High Nutrient Low Chlorophyll (HNLC) areas, in particular the North and Equatorial Pacific and Southern Ocean. The inclusion of the iron cycle in the model, though in itself a major source of uncertainty, would be expected to impact on the strength of the biological pump and the air-sea fluxes of CO_2 (Archer and Johnson, 2000, Global Biogeochem. Cy.). The inclusion of oxygen would also be desirable due to its relevant role in the global biogeochemical cycles, and its impact on marine life and carbon production and ventilation. The ocean oxygen minimum zones are particularly relevant to ocean-atmosphere gas fluxes and the control of nitrogen availability. In terms of stoichiometry, some studies have documented deviations from the Redfield stoichiometry (e.g. Schneider et al., 2003, Global Biogeochem. Cy.; Koeve, 2004, Deep-Sea Res.), particularly at depth and with respect to remineralisation of organic matter changing with depth or with ambient oxygen levels. Implicit assumptions made about the hydrogen content of organic matter can lead to inconsistencies in the modelled remineralisation and denitrification stoichiometries (Paulmier et al., 2009, Biogeosciences). Paulmier et al. suggested that future marine biogeochemical models explicitly state the chemical composition assumed for the organic matter, including its oxygen and hydrogen content, and this is something which could be explored.

However, there are a number of points to consider in terms of increasing model complexity. For instance, there is a balance to be struck between complexity and computational cost, especially when performing data assimilation. There also remains much debate in the scientific community as to the optimal degree of complexity in biogeochemical models, with the solution clearly depending on the questions and applications the different models are being used to address. For example, in coastal waters, where biological systems are highly variable, the use of more complex models is advisable in order to capture ecosystem dynamics realistically. However, too much complexity can lead to too much uncertainty and problems with interpreting the model's dynamics and predictions (Fulton et al., 2003, Mar. Ecol.-Prog. Ser.), especially when few observational data are available for model validation. Moreover, Friedrichs et al. (2006, Deep-

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Sea Res. II) compared three fundamentally different ecosystem models, of varying complexity, in a 1D framework and assessed which model structure best captured the underlying biogeochemical processes of the region studied. Their results showed that, after optimising the models' parameters, all three models behaved similarly, implying that the additional complexity of a multiple size-class model may not be advantageous. Furthermore, as biogeochemical variability is strongly dependent on the physical environment, the use of a physical model that depicts physical variability realistically has been shown to have a far greater impact on biogeochemical model response than a change in ecosystem model complexity (Friedrichs et al., 2006). Increased horizontal and vertical resolution could therefore be explored, with a move to an eddy-permitting or eddy-resolving configuration (although this would bring added computational cost and require additional tuning of model parameters). For these reasons, we would argue that it may be preferable to keep model complexity as low as is feasible (whilst taking care to ensure that the model is able to realistically capture observed global biogeochemical patterns).

We would suggest that future progress in biogeochemical data assimilation should come from the use of data assimilation for simultaneous state and parameter estimation, in order to deterministically tune the biogeochemical models, as pointed out in Friedrichs et al. (2006). Our results show that assimilation increments are consistently applied in certain regions in order to counteract model biases which are likely caused by the use of constant non-optimal parameters, which in reality will vary. If the assimilation approach of Hemmings et al. (2008) were extended such that alterations were made to the model parameters describing (for instance) growth and loss rates, this could change the model trajectory in order to minimise the biases, as well as updating the state variables.

"Section 4.1 The problem of Gaussianity in biogeochemical DA is well known and the log-transformation is a common, pragmatic procedure. However, the log-transformation

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does not guarantee Gaussian distributions of the assimilated chlorophyll data (that is different from what stated by the authors at p 688, l16). To this regard "anamorphic functions" or similar approaches would be preferable (see e.g. Simon and Bertino, Ocean Science Discussions, 2009, Lenartz et al., Journal of Marine Systems, 2007; Brankart et al., Ocean Sci. Discuss, 2011). Please comment the possible implications of using non-Gaussian distributions in the assimilation scheme."

We will make it clear that log-transformation does not guarantee Gaussianity, and comment that other approaches exist, which could be investigated in the future.

"Moreover, log-normal assumption and transformation are typically used in chlorophyll DA (e.g. Bertino et al., International Statistical Review, 2003; Torres et al. , Journal of Marine Systems, 2006; Nerger and Gregg, Journal of Marine Systems, 2007; Gregg, Journal of Marine Systems, 2008; Ciavatta et al., J. Geophy. Res., 2011). Why do the authors use log10?"

The log-normal assumption is indeed used in many studies. However other studies have shown that phytoplankton biomass follows a $\log_{10}$ distribution (e.g. Barnes et al., 2011, J. Plankton Res.). In practice, it should make no difference to the assimilation whether log-normal or $\log_{10}$ is used. The shape of the distribution is the same (the ratio of log(x) to $\log_{10}$(x) is identical for all values of x), except that $\log_{10}$ gives a smaller variance. It is the shape of the distribution that matters for the assimilation, so as long as the same transformation is applied consistently to both model and observations, it should not matter whether log-normal or $\log_{10}$ is used.

"Section 4.2 I do recognize that the "nitrogen balancing scheme" is extensively described by Hemmings et al. (2008). However, some more lines on the method would

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help the reader, e.g. to understand the relevance of the growth rate and loss rate that the authors mention in Fig.8."

Agreed, we will expand the overview of the method accordingly.

"Section 5.1 If I am not wrong, the authors: 1) apply FOAM-HadOCC (with physical DA) in a 2-year run and they found that the Nitrogen field is relatively poor, 2) thus they replace it with a climatological Nitrogen field and 3) they run a one-year spin-up. What is the utility of the 2-year run if the authors replace the nitrogen field? Does this replacement introduce inconsistency with respect to the other model variables? Is it sufficient a 1-year long spin-up to solve this inconsistency?"

The original two-year hindcast referred to was not performed by the authors. It did not start from climatology, and used a similar but earlier configuration of FOAM-HadOCC. Fields from the end of this run were used because they were the most convenient set of (semi-)spun-up initial conditions available for the required time period. However the nutrient field was poor, largely due to the poor initial conditions of the original hindcast, and it was judged that this would be a greater source of error than the inconsistency introduced by replacing it with climatology. We accept that the one-year spin-up is shorter than ideal, however the biological fields adjust quickly enough for "normal" model behaviour to be reached in this time, even if some adjustment is still occurring. Given that the aim of the paper is to investigate whether the data assimilation can improve an imperfect model, one of those imperfections being the initial conditions, we believe that the length of spin-up and choice of initial conditions are sufficient for the purposes of this study. We will comment on and clarify this in the paper, and for future studies we plan to perform a longer spin-up starting from climatology.

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"Section 5.2 I think that Section 5.2 is rather unclear. The observational and background error are highly relevant in an assimilation scheme. Thus, the authors should clarify several points in this section."

We agree that this section is briefer than it should be, and will expand and clarify it accordingly, making sure in particular to address the points raised below.

"Firstly. In section 3.2, the authors had provided a detailed description of the accuracy of the GlobColour products. Are those ones the errors applied in the assimilation run, or they use observational errors computed in Section 5.2?"

The observational errors computed in Sect. 5.2 are the ones applied in the assimilation runs. The errors provided with the GlobColour products are just used for the quality control procedure described in Sect. 4.1.

"Secondly. The authors compute initial errors from the GlobColour data on a 1° grid for the period 1998-2007. This error is then assigned "in equal proportions" (what does it mean precisely?) to the assimilated variables as well as to the background. I do not see the point of defining the background (model) error in relation to the error of the GlobColour data. Could the authors please explain this point?"

The variance of the GlobColour data was calculated at every point on a 1° grid. At each grid point, the mesoscale background variance was set to 25 % of the variance of the GlobColour data, the synoptic scale background variance was set to 25 % of the variance of the GlobColour data, and the observation error variance was set to 50 % of the variance of the GlobColour data. This is indeed a slightly arbitrary method for defining these error variances, hence why new estimates were calculated

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for use with the main hindcasts. However defining the error variances in relation to the variance of the GlobColour data ensures that the highest error variances are in the regions with the highest chlorophyll variability, as would be expected. Performing an assimilative run with these error estimates produced more realistic chlorophyll fields than a run without assimilation, therefore allowing more accurate calculation of the error estimates used in the main hindcasts.

"Finally. The authors use the same correlation length for chlorophyll and SST (100 Km for the mesoscale and 400 Km for the synoptic scales). However, phytoplankton patchiness leads often to relevant changes in chlorophyll on a much shorter scale. Please comment the choice of the chlorophyll correlation length."

This point was also raised in the short comment of Anna Teruzzi, and we give the same response:

Estimates for the length scales are given as part of the NMC error covariance calculations. These were found to be broadly similar to those for SST for the synoptic scale, and a bit shorter for the mesoscale. However given the relatively coarse (1°) resolution of the model, it is undesirable to set the mesoscale length scale to be much shorter than 100 km (noting that "mesoscale" refers here to small-scale processes resolved by the model, rather than to the actual ocean mesoscale). Therefore the length scales were set to be equal to those for SST, in part due to these results, and in part for consistency and convenience. No other length scales have been tested, but future improvements could potentially be achieved through tuning of these. We will note this in the manuscript.

"P703, l17-23. The authors should consider to re-rewrite this part more clearly (e.g. l18: "verifying" what?). The National Meteorological Centre (please specify the

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acronym "NMC" in the text) and the Hollingsworth-Lonnberg method provide error estimates of the background error, of the observational error, of both of them? At the end of the day, which is the range of the background and chlorophyll errors applied in the assimilation run?"

We agree that this section is too brief, and will expand and clarify it. Please see our response to Anna Teruzzi's short comment for details of how the NMC and Hollingsworth-Lönnberg methods are combined to give both the background and observation error estimates. In this context "verifying" means "valid on". Whilst this is accepted forecasting terminology, we will change it to make the meaning clear.

"Figure 4. Please provide a quantitative comparison between reference vs satellite and assimilated versus satellite data, to help the reader in appreciating the improvements. For example, maps of mean percentage differences between outputs and satellite could be helpful."

Quantitative comparisons are provided in Fig. 5 and 6, and in the statistics in the text. Maps of mean and median (a robust measure) percentage differences are shown in Fig. AC4 and AC5 below. These can be included if required, but given the clear visual differences in Fig. 4, and the subsequent validation, we do not believe that including these would add to the conclusions of the paper, and so would prefer not to include them.

"Figure 5. The mean global bias (MGB) of the outputs and the mean absolute error (MAE) of the observations are not directly comparable (as acknowledged by the authors). MGB could be lower than the MAE because positive and negative errors compensate when averaging: is this the case? I think that the authors should plot

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MAE for the model outputs as well, to facilitate the skill assessment."

By definition, MAE is the maximum possible value of MGB. MGB is lower than MAE for the model, and almost certainly is also for the observations (although this cannot be shown from the available information). This point was also raised in Anna Teruzzi's short comment, please see our response to that for further discussion of this point, and for versions of Fig. 5 with the model MAE included, and with the observation MAE excluded. We believe it would be clearest (and would not impact on the skill assessment) to simply remove the observation MAE from Fig. 5.

"The error statistics are computed using the data at latitudes > 60° ? (Probably these data could be excluded from the computation, since they were not assimilated. This should lead to a further improvement of the DA error statistics)."

These data have already been excluded, we will make this clear in the paper.

"Validation of the DA results vs. SeaBASS chlorophyll data. Are the SeaBASS chlorophyll data (or part of them) used to calibrate the assimilated GlobColour product (p707, l29)? In this case, the DA output cannot be considered fully independent from the SeaBASS data used in the DA skill assessment. The dependency could be low, but the authors should address this point."

The SeaBASS chlorophyll data were indeed used to calibrate the GlobColour products, as part of the error characterisation. You are right that they therefore cannot be considered fully independent, and we will state this in the paper (although the dependency is likely to be low).

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"Table 1. The authors should clearly state that the GlobColour product does a better job than assimilation in estimating the SeaBASS observations of log10 (chlorophyll) at the surface. It is true that the normalised standard deviation of assim (0.868) is closer to 1 than climatology (0.597) in Table 1 (but note that control does a better job: 1.065). However, all the other statistics of climatology are noticeably better than assim."

Strictly speaking, a comparison between the GlobColour products and the SeaBASS observations has not been performed here (for such a comparison see Maritorena et al., 2010). The comparison performed here uses a climatology which has been derived by the authors from previous years' GlobColour data. However we will clearly state that this climatology provides a more accurate estimate of the SeaBASS observations than the assimilative model run does.

"Please mention that assim leads to just a "slight" improvement in bias of the chlorophyll beneath the surface, with respect to the control (i.e. -1.3%). I think that the above statement on the better skill of climatology and mentioning "slight" improvements do not diminish the value of the work. Improvements are not obvious at all in biogeochemical DA."

We will mention this.

"However, the authors should mention which are the further improvements in the structure of the biogeochemical model that could lead to better result."

We will do this, see our comments above.

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"Tab 2-6. Why the authors do not show the value of MPE, as they did in Tab 1."

MPE was shown in Table 1 to allow a comparison with studies which validate the satellite products, and typically use this statistic. It was not shown in the other tables as it adds little to the information provided by the bias. However we can certainly show these results, and will add MPE to all tables in the paper.

"Figure 8. I recommend the authors to replace the figure with vertical profiles where field data are available for skill assessment. It is not relevant if the data does not cover the whole set of model variables. The AMT transects of chlorophyll and nitrate, or at least punctual profiles (HOT?, BATS?) could be helpful."

Unfortunately not many in situ data are available to us. We did not have access to AMT nitrate data or BATS chlorophyll data for 2008. Profiles at the HOT site are only taken approximately once per month, and only show a single point in space and time. Example profiles could be shown at HOT and from the other data sets, but unless a great number were shown, these would be less representative of the effect of the data assimilation than the current Fig. 8. Whilst it does not make comparison to observations, we feel that Fig. 8 gives a good overview of the impact the assimilation has on the other model variables. Given the multivariate nature of the nitrogen balancing scheme we believe it is relevant to cover the whole set of model variables. More could be done to assess this, including comparison to observations at certain locations, but we feel that this should form part of a future publication, with a general overview such as Fig. 8 presented here.

"As it is, Fig. 8 is a general discussion on the features of the "nitrogen balancing scheme" by Hemmings et al. (2008). However, the short description of the method

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given in this paper makes rather difficult the understanding of the discussion. This discussion seems not necessary with respect to the objectives of the paper."

We will expand the description of the method to make it clearer. We feel that the inclusion of Fig. 8 and the surrounding discussion gives a brief but good overview of the multivariate workings of the scheme, which is relevant given that it has not previously been demonstrated in a 3D model.

"Moreover, the Figure has some problems with the headings, the variable units are not specified, and I do not see the point of showing the increments. These are computed as a difference between the background and the analysis (eq. 1). Thus they are not very helpful when comparing analysis and control."

We agree that Fig. 8 needs tidying up, and will do this. However we feel that the inclusion of the increments is very important. The surface $\log_{10}$(chlorophyll) increments are indeed computed as the difference between the background and the analysis (as calculated by the assimilation) on each particular day. But these are then applied incrementally, with the model allowed to adjust accordingly, so that Assim is not simply given by Control plus the increments. Showing the increments therefore illustrates which differences between Control and Assim are directly due to the increments, and which are caused by the model adjusting to the increments. With the 3D increments to the model state variables, which are calculated by the nitrogen balancing scheme rather than the optimal interpolation, this is even more pertinent. For instance it can be seen in Fig. 8d-f that zooplankton concentration is decreased in Assim compared to Control, as a result of the decrease in phytoplankton concentration (Fig. 8a-b), despite the positive zooplankton increments being applied as a result of the nitrogen balancing.

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"Finally, in Fig. 8 is quite evident that control and assim are quite similar in nutrients, DIC and alkalinity (if I do interpret correctly the shift in the headings). This low impact of chlorophyll DA on the model variables is reflected in the negligible DA changes in the global annual mean fields shown in Figure 9. What is the authors' comment on this low impact of chlorophyll DA on the model variables? How does "the low impact" reconcile with the DA improvement of the nitrate estimates shown in Figure 10?"

The impact of the assimilation is high on some model variables (phytoplankton, zooplankton, detritus), but we agree that the impact is generally low on nutrients, DIC and alkalinity. This impact is lower than might be hoped for, and is a source for potential future improvements to the scheme, but for now it is encouraging that there is no evidence of degradation. As mentioned in the paper (and which we will clarify), the low impact on nutrients is likely to be largely due to the large biases evident in Control. There are too many nutrients near the surface, which the assimilation would aim to reduce. However because a large reduction in phytoplankton is necessary, and nitrogen must be conserved, this reduction in nutrients cannot generally happen. Improving model biases would hopefully improve the performance of the assimilation in this regard. One region where nutrient concentrations are decreased, because phytoplankton concentration is being increased, is above 40°N in the North Atlantic (Fig. 8j-l). This happens to be where the SeaBASS observations are, hence the results seen in Fig. 10 and Table 4. As discussed above, the results are influenced by the low number of in situ observations, and we will make this clearer in the paper. Regarding DIC and alkalinity, the impact is low because the background concentrations of these are much larger than the changes that can be made due to changes in plankton and nitrate, and so proportionally the impact is always likely to be much lower.

"Tab 4. The authors should clearly state that climatology does a better job (not just "slightly") than assimilation with respect to all the skill statistics (e.g. correlation +0.3)."

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"Tab 6. The authors should point out that the normalized standard deviation of assim is worse than the control one."

"p 688, l 7 : delete "significantly": statistical tests to assess the statistical significance were not presented"

We will change the manuscript accordingly to address these points.

"p 688, l 10: not in every ocean basin (e.g. not in the Arctic Ocean)"

An improvement was indeed seen in every ocean basin, the Arctic Ocean included, as shown in Fig. 6.

"P693, l11: specify that the FOAM system you're using is non-operational but it still assimilates physics."

We will specify this.

"P696, l8: can the authors use the data of the "day after" in an operational system?"

This depends on what time of day the system is run at, and is mentioned in the discussion on p716, l12-16. "Day after" information in the GlobColour products does not tend to be later than about 02:00 UTC, so a full day's delay is not implied. As stated (p715, l16), the GlobColour products are typically available by 14:00 UTC, and the pre-operational near-real-time system runs shortly after this time, so this is not a problem. It would be more of an issue for the operational FOAM system, which runs at 05:00 UTC, and so could not currently assimilate the previous day's product. However

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this is also an issue for some other data types, such as sea ice concentration and some Argo data, and therefore operationally FOAM assimilates data from the previous two days, meaning the GlobColour products could still be used.

"p 697, 10: Just a curiosity: Why Seawifs error = 35.77%, i.e. it is higher than 25.96 % (p 697, l 1)? Operational errors are higher than non-operational errors?"

This is simply due to differences between the data periods and in situ observations used in each comparison. Bailey and Werdell chlorophyll statistics are based on 271 matchups, and GlobColour on 578 matchups.

"p 697 l. 28 - p 698 l 7: the "Discussion" is a better place for these comments"

These comments were placed here because it was felt that they followed on appropriately. However we can move them to the discussion section.

"p 698 , l 20 : "error" greater than 50?"

We will make it clearer what is meant by this line. Each grid square (bin) in the merged gridded product has an accompanying set of flags. One of these marks if more than 50 % of the area covered by the bin is land rather than sea. If this flag is set then the observation given by that bin is not used. No error is implied.

"p 698 , l 23 (and subsequent): could the authors use a word different than "background", to avoid any ambiguity with the assimilation background?"

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We would rather keep with the word “background”, as this is the correct technical term. Whilst there is ambiguity with the assimilation background, this is appropriate, because the assimilation background could potentially be used for this purpose (for instance this is done in the operational FOAM system). We will clarify this in the text.

“p 700, eq. 1 : replace x with y to indicate the observation vector”

Thank you for pointing this out.

“P701, l10: briefly summarize what “incremental analysis update” does.”

Incremental analysis update applies an equal proportion of the increments at each time step, rather than applying the entire increments at the first time step. We will clarify this in the text.

“p 701, l 18-19 versus l 24-25 : is sea ice concentration assimilated in the two-year hindcast?”

It was not. As discussed above, we will re-write this paragraph to make it clearer.

“p 703, l 16 define the acronym NMC (National Meteorological Center ?)”

National Meteorological Center is correct, we will define this in the text.

“p 704, l 15: “dotted” or “dashed”?”

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This should read “dashed”, we will change this.

“p 706, l 23 : is the “single day” the first analysis (1st January)?”

Yes, it is.

“p 707, l 3: it is not “curios”, but relevant. It indicates that the skill statistics are influenced by irregularities in the globcolour data.”

This is true, we will phrase this more appropriately.

“p 707, l 18 : “in general” (or similar) is better than “universally””

We can use a different word than “universally”. However since the statistics are shown to be improved in all ocean basins, “in general” is not necessarily a better phrase.

“p 710, l 5-6: “all aspects of the model” would include the model structure and parameterization. Replace with “the other variables”. Anyway, not all the variables are improved: see fig 9.”

Your comment regarding “all aspects of the model” is true, and we will rephrase it accordingly. The fact that not all variables were improved is not relevant, we are merely stating that we aim for them to be.

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"Figure 8 (please consider replacing the figure: see the above "specific comments"). It is not useful to comment the discussion of this figure since I am not sure about the figure headings."

Please see our comments above. We agree that the figure headings need to be improved, but still believe that the figure is worth including in the paper.

"p 714 , l 24: mention that climatology does a better job than assim in hindcasting the in situ chlorophyll."

We will mention this.

"p 714, l 26: "consistent" is slightly ambiguous here. It is true that the assimilation scheme changed the other variables consistently with the changes in chlorophyll. But it is also true that these changes were rather small (in magnitude): compare control and assim in fig 8 and 9."

We will explain more clearly what is meant, perhaps using a different word than "consistent".

"p 714, l 27 please mention that few data were used for the DA skill assessment (e.g. nitrate at one area, at 2 months)."

We will mention this.

"p716, l10 : "considerably" holds strictly just for chlorophyll, not for the other model

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variables"

"p716, l17 : "considerably" holds strictly just for chlorophyll, not for the other model variables"

True, we will change this accordingly.

"p 716, l 17-19: which are the possible improvements in the biogeochemical model?"

We will add these, see our comments above.

"p716, l28 : what does it mean that the "consistency" was improved?"

We will explain this more clearly, perhaps using a more appropriate word. This refers mostly to the changes seen in phytoplankton, zooplankton and detritus, as shown in Fig. 8. Please also see our response to Anna Teruzzi's short comment for further discussion about this.

"p 717, l1 : "physical assimilation" was not integrated with the chlorophyll assimilation in this work?"

What we meant is that in this study the physical assimilation did not directly influence the chlorophyll increments; the two schemes were run concurrently but independently. We will make this clear in the text.

"Tables 2 & 3 : merge the tables in a single one"

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“Tables 4-6: merge the tables in a single one”

We will do this.

“Fig 3: it is log10 (Chl) (not “log(Chl)”)”

Thank you for pointing this out.

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Interactive Comment

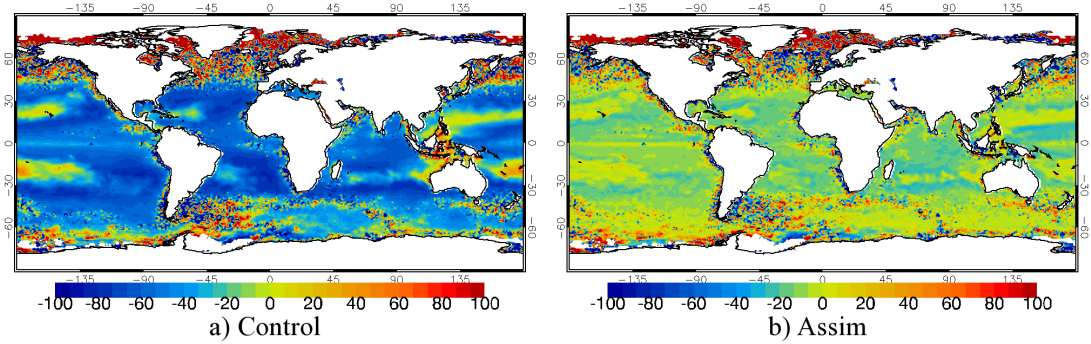

Fig. AC. 4. Mean surface log10(chlorophyll) model minus observation percentage difference

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Discussion Paper

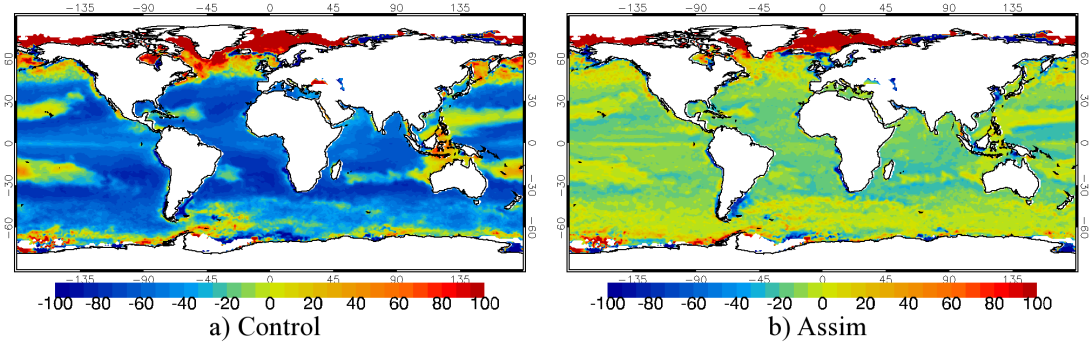

Fig. AC. 5. Median surface log10(chlorophyll) model minus observation percentage difference

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