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Interactive comment on "An ensemble probabilisitic approach to reconstruct the biogeochemical state of the North Atlantic Ocean using ocean colour images" *by* Florent Garnier et al.

Anonymous Referee #1

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The paper is extremely interesting, and clearly very beneficial for the future assimilation of surface chlorophyll in BGC models, including future stochastic operational systems. The paper heavily relies on Garnier et al, 2016, and builds on the ensemble experiment to use it for data assimilation. In this aspect, it clearly deserves publishing. However, a number of issues need to be resolved first. There issues do not require to re-do the scientific work, but rather to reformulate some parts of the paper more carefully. In my opinion, some of the re-writing is really necessary for the paper to be accepted.

General comments

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Discussion paper

The paper is confusing at the first lecture because it lacks a few capital (basic) pieces of information in the first sections. One needs to read it entirely to find that information, or re-read it to fully understand. Some of the missing information is very superficially mentioned, without being definitively clear about it,and one is left more confused than if nothing was said at all.

The most striking example is the fact that the authors mention on line 171 (page 6) that "In this part, the prior probability distributions are used to update the model bio-geochemical state. We present a sequence of analysis meaning that updated states are not propagated by the model." I noticed that, but did not understand exactly what it meant. Do you produce xˆa, maybe use it to compute statistics etc, but then actually discard it and continue the model run using xˆf ? Then page 15 (9 pages later), you introduce another run where you compare 2 ensembles during a 1-month run. One is the same as previously, and the other probably is one where you actually use xˆa to restart the model ? Even that is not clear. The text lines 75-80, 170-171 and 400-405 should be made much clearer in that respect.

I understand that ensemble simulations at this scale, including a Bio model, require a lot of computing power, and I don't have problems with you discarding xˆa for subsequent model runs (if that's what you do). For the 1-year run, it could be that you computed the analysis from the [Garnier et al 2016] outputs, and did not actually re-run the ensemble of models. That is OK, but it is a highly unusual way of working, and if it's the case, then it should really be explained clearly.

Another such confusion is what you actually put in the state vector x introduced around line 130. Is it the whole biogeochemical model (all 24 3D variables) as you seem to imply with "biogeochemical state"? Is it NCHL and DCHL ? Is it the sum of NCHL and DCH ? Suddenly at line 404, we see "multivariate". Also you don't explain anywhere (unless I missed it) how you link the chloro observation (y) with the state vector (x). Is it H that performs the sum of NCHL and DCHL ? Intuitively, if y>Hx, how do you split the increment onto the variables inside x ? In the same proportions of what they already

contained ? All this needs to be clarified.

Finally, the paper sometimes presents affirmations, of the kind "we see A, so this implies B", which, in my opinion, are not valid. They will be listed below among the specific comments.

Specific comments and typos

Line 75-80: this is already related to my first general comment, to lines 130, and should really be clarified much more explicitly, so that the reader unequivocally knows what you are doing, what you are using to restart the model every 5 days, and what you are analyzing in the statistics. For example, to me, the "update" (line 81) means the "increment", not the "analysis" itself (i.e.: xˆa = xˆf + update), so I'm confused when you say "some of the BGC updates are used as initial conditions". The word "some" is confusing me even more.

Line 113: PISCES is computed to be able to ...: please formulate elsehow.

Line 117: the coupling frequency is 40 minutes. "frequency" is unfortunate but common, and we understand what you mean, so that's ok. In Garnier et al 2016, you coupled at every time step. Do you now try to save cpu time ? Because you did resimulate the whole ensemble after all? What's the impact of this 40-minutes coupling compared with the coupling in Garnier et al 2016 ?

Line 130: please define more precisely what you put in x . Define also H, here or when you introduce it around line 184

Line 148: [my comment probably outside this article's scope] Would the conclusion about the ensemble dispersion in upwelling region be different, if you also perturbed physics?

Line 153: "the main difference with respect to a deterministic simulation" –> this is not a "difference", because in a deterministic simulation we are not speaking about whether observations are in an envelope, or not. In a deterministic run, we need to use different

(poorer) metrics. So, I understand your point, but this needs to be rephrased.

Line 155: this is an example of invalid "A–>B". The fact that the observations are all in the envelope (or 70% of them) does NOT imply that your ensemble has a consistent level of uncertainty. I can make an ensemble, instead of CHL, I use an envelope which is {0 Chl+100000}. Now ALL observations will be inside my envelope. If you really want to say that the level of uncertainty is consistent, you could check how many of the observations fall into the different percentiles. An example (for physics, not BGC) is given in Vandenbulcke and Barth, 2015 (10.1016/j.ocemod.2015.07.010), when we can suppose the variable is Gaussian, than we know how many observations SHOULD fall into the different percentiles. Obviously in your case, a priori you don't know the real PDF shape

Line 163-165: Since...content of the observations ==> the model error ...observations. Again, this is not a consequence. Furthermore, in line 165, please avoid to use "error" for the model but "variability" for the observations.

Line 167: what you say is logic, and I agree. But even if you choose another method (not a multiplicative one), I suspect that the effect of DA on the model state would be very small when CHL is close to zero, because I expect both x and y would be close to zero. This is not a critic of your method!

Section 3 title: add "s" to "update"

Line 171: not clear, see general comments

Equation 7: translate "avec"

Equation 9: if xˆa contains the sum of NCHL and DCHL, explain how you upgrade the model variables once you have xˆa. If xˆa contains NCHL and DCHL themselves, explain how the analysis equation takes this into account (I suppose that H is doing the summation over the 2). This is the same comment as the before-last general comment

Line 195: replace "in" with "and" Line 196: add "an" at the end of the line

Line 203, remove the line (except the last 2 words), it's obvious

Line 216: remove "MyOcean", the reader will not find that anymore, just Copernicus / CMEMS

Line 233 : I agree with the method to inflate the diagonal to account for missing non-diagonal elements in R. Maybe you can cite e.g. Brankart et al (2009) or Cosme et al (2013) who propose other methods (tri-diagonal etc), but it's not mandatory. You could also link what you do to the representativity error (observations represent smaller scales than the model). In any case, maybe replace "refers" with "is proportional to" ? Else, in equation 10, wouldn't you multiply by 3.14?

Line 248-249: Replace by: In general, the updates reduce deviations with observations. A more detailed comparison with observations will be performed in next sections.

Line 250: The ensemble average does not show abrupt variations, but maybe individual members do? Small scales are usually averaged-out of ensemble means. You could show some members to prove your point, or not (your choice).

Lines 250-255: You seem to imply that "Line 250-254" imply "254-255". Again, I do not agree. Again, I could give you a stupid counter-example: use the most stupid and defect method in the world, but choose R=diag(10000000). Small scales and large scales *will* be preserved in the analysis. It does not "ensure" and "confirm" what you say. But I agree that it is a good sign and gives confidence in the method. I don't criticize the method, but I suggest to rephrase more carefully

Line 256: Remove "To go further", it doesn't help with anything.

Line 260: after 40°N, it is reduced –> you probably mean increased, not reduced ?

Line 262. Where in the plot is the DCM "enhanced" ? I don't see that (maybe I'm not looking where I should. What do you mean by 'enhanced' anyway? Stronger absolute values of chlorophyll ? Deeper DCM ? In general, the enhancement (or lack of it) of your DCM is "just" what your correlations indicates (the P matrix, or more precisely

the columns of S, the members of your ensemble). Whether the enhancement is right or not, who knows (unless you showed us some observed vertical profiles, that you probably don't have). So we cannot know if the actual correlation (from the ensemble) between surface and deeper layers in the euphotic zone are actually right (by looking at the DCM enhancement). It sure is reassuring that you obtain zero correlation between surface and depths>euphotic.

Line 263. correlation between surface "concentration" and vertical "distribution", please rephrase better

Line 266: use capital 'W' on 'which'

Line 267. Here it would have really helped me if I knew what was inside the vector x, and how the experiment is working. If it's only Chl, or alternatively, the model does NOT restart from xˆa, but just from xˆf, then it is normal that the model does not generate vertical fluxes of organic matter in between assimilation cycles. If necessary, you can rephrase this

Line 270. Ok, I agree with this

Line 275: remove *the" before "figure 4"

Line 279-280: It seems to me that whether you preserve or not a "significant" (please define what is significant, or use another word) level of uncertainty in Pˆa, is a consequence of both Pˆf (your ensemble) and R. Furthermore, even if Pˆa was small (compared to Pˆf), it could very well be that the model increases it in the same amount until the next assimilation cycle, isn't it ?

Line 283 : if you think it's helpfull, you could give a number here. We know that for the prior envelope, 70% of observations are included. You don't give the equivalent for the posterior envelope, you just say "nearly always".

Line 284: add "the" before "updated ensemble spread"

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Line 285: It means that ... . Same comment, it doesn't necessarily means that. You have a good and convincing situation, but A does NOT imply B.

Line 286 : obvious (but you should keep the line if you think it helps the reader)

Figure 4 (and other similar figures): many lines, difficult to see. The xf-members are hidden by the xa-members? But I don't have any suggestion to do better than that. At least the min,max,median are visible and convincing

Figure 4 legend: add a space after ")" (2 times)

Line 293-294: add "model" to "uncertainty" in line 293, at the replace "biogeochemical uncertainties" with something like "uncertainty on the observations".

Line 295: Maybe it is positive, maybe not. Maybe the model will greatly increase P ? Maybe not, in this case you're right. The reader cannot fix his ideas because [see general comments]

Line 297: obvious (but you should keep the line if you think it helps the reader)

Line 301, last word: replace "an" with "a" Lines 300-304: I would move this to the conclusions, or rephrase

figures 5. In May it seems the DCM is decreased, in October increased. Is this what you meant with 'enhanced' ?

Figure 6 (lower panels) and Lines 330-332 : this is not obvious from the figure. Can you explain ?

Line 354: Suddenly you say that the first months of the year are not reliable ? Isn't there a spin-up as in Garnier et al 2016 ? Isn't it possible this rather (or also) has to do with seasonality (reliability goes down again from the end of the summer) ? If the first months aren't reliable, what about all the previous things you said in the article, what about the plots you showed for 31/March, etc ? Maybe you are saying that the reliability in the first 4 months should actually look like the one in the last 3 months ?

That except the seasonality effect, the remainder of the (lack of) difference between prior and posterior reliability is due to (lack of) spin up ?

Section 3.4 Title : Use capital 'C'

Line 368: put a space before "nevertheless" Line 369: remove "s" from "cascades"

Section 3.4. Here it would have been good to know for sure, from the start of the article, if x contains chlorophyll or all model variables

Line 379: yes if there are correlations between variables, then observing one can "correct" them all. If you put all variables into x, then I bet you will have correlations even between not-so-closely-related variables, exactly as we have unphysical long-range correlations in space (which we remove with localization techniques). So yes, we can "correct", but until we have observations, I'm not convinced yet that we are improving anything. In the EnKF, the analysis step is still a linear process (although the propagation of P by the model is not).

Line 383 on the next page. Ok now I agree.

Line 386, "should be relevant", yes indeed they should. I'm really curious now if the EnKF is correcting the non-observed variables (through the multivariate covariance matrix and state vector) or if the model is (by propagating corrections of chlorophyll to other variables).

Line 388: add "some" before "first" and "the" before "forecast"

Line 400: if you're using a 5-day window, the analysis could be rapidly deteriorated, but what do you mean with the strong level of uncertainty for PISCES ? Do you mean that you added large stochastic terms in the equations, or that PISCES (even deterministic) has a tendency to rapidly generate errors (e.g. drifting always toward the same "wrong" solution as you write later in the article) ?

Section 4, in general: the term "forecast" is may be confusing, it usually means a

model run without assimilation (because it's in the future), as opposed to hindcasts and analysis

Figure 10 and discussion lines 425-430: do you assimilate only at time=0 in this experiment ? Obviously at some point, you restarted the model from xˆa, and then let it run for one month ?

Line 474: yes I agree with that ! also, "sustanaibly"

Line 476: ... should BE OF the order of a day, while it is OF the order of ...

Line 479: "over 24" –> "out of 24"

Line 479 : multivariate ...

Line 480: are not "be" sufficiently –> remove "be"

Line 498: "to correct": I disagree with the word choice. I would rather say it is necessary to take the uncertainty of the physics into account. Whether you can correct it or not depends on observations, etc. But if you represented the uncertainty in your ensemble, at least you would account for it.

I remember a talk from K. Fennel where she explained that perturbing BGC without perturbing also the physics, did not work for her (maybe she says something similar in the paper that you cite, Yu et al 2018)

Title : I personally find the title long and maybe a little redundant. For example, "ensemble" and "probabilistic" are redundant. "to reconstruct" is also sort of useless. However I recommend to the author to keep this title if he so wishes.

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