Ocean Science Discussions

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Comment on os-2021-35

Anonymous Referee #1

Referee comment on "Can assimilation of satellite observations improve subsurface biological properties in a numerical model? A case study for the Gulf of Mexico" by Bin Wang et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-35-RC1, 2021

This is a very interesting and valuable contribution to the present scientific literature addressing the impact of (DEnKF) assimilation of physical and biogeochemical data, including Argo floats, on the simulated state of the ocean. More specifically, the paper addresses an important question of the impact of (mostly) surface data assimilation on sub-surface physics and biogeochemistry, focusing on the Gulf of Mexico. The simulated physical and biogeochemical tracers are validated with an independent BGC-Argo data-set. The paper is well written and I have mostly only minor comments to address, which can be found below. One remark: please note that in the downloaded .pdf version of the manuscript the first digits of the three-digit line numbers are running off the left side of the page, so beneath the section 1 I avoid referring to the line numbers and refer to the paragraph and section number instead.

Section 1:

The introductory section is nicely written and the only comments that I have are regarding the use of references:

- in particular on line 52 when the paper talks about assimilation of optical properties, the included references are very far from exhaustive. On top of my head I could add to the list the following papers: 1. Jones et al (2016), Biogeosciences, https://doi.org/10.5194/bg-13-6441-2016, 2. Gregg and Rousseaux (2017), Frontiers in Marine Science, https://doi.org/10.3389/fmars.2017.00060, 3. Skakala et al (2020), Journal of Geophysical Research: Oceans, https://doi.org/10.1029/2020JC016122**.** I completely understand that sometimes there simply are too many references and you cite

only the first ``pioneering'' papers, but in such case it would be good to put "e.g." in front of the citations, or add after the citations "with few more recent follow-ups", or something similar. Otherwise this might look like the cited papers are all that has been published on the topic and it might lead to the other papers being omitted when someone new-to-the-area uses your references as part of literature review on the topic. Similarly to this, I would make sure you somehow emphasize that the citations aren't exhaustive also in the other cases, e.g. the phytoplankton size-class chlorophyll has been assimilated also in Ciavatta et al 2019, Journal of Geophysical Research: Oceans, https://doi.org/10.1029/2019JC015128 and for total surface chlorophyll there's many references left out.

- more importantly the text on the lines 65-70 suggests that the subsurface validation was done only in the 2 cited studies and only on climatological, or basin-wide scale. Firstly, I believe it is appropriate to include here the Cossarini et al 2019 reference that is cited elsewhere in the paper (which looks at vertical profiles along BGC-Argo locations), but secondly there is a new study of Skakala et al (2021), Journal of Geophysical Research: Oceans, https://doi.org/10.1029/2020JC016649 that validates both free runs and surface data assimilative runs along glider trajectories (in the North-West European Shelf) to determine the impact of surface data assimilation (and some other DA as well) on the simulated sub-surface tracers. This study, although it uses a very different, 3DVar, system, is of direct relevance here, since it addresses similar questions to this submitted manuscript, and, as mentioned before, it does model validation along a specific glider 3D transect (which is of course spatially limiting), rather than on basin-wide scales. Btw. please note that there is another study that assimilated BGC-Argo oxygen data that perhaps deserves to be cited in this paper: Verdy and Mazloff, Journal of Geophysical Research: Oceans, https://doi.org/10.1002/2016JC012650.

Section 2:

I find it would be perhaps beneficial for the reader to provide slightly more information on the system set-up and maybe a bit rearrange the text. In particular:

- could you please mark the exact model domain? The Fig.1 shows the Gulf of Mexico region with its bathymetry, but I guess what's in Fig.1 is not precisely the spatial model domain (?) Or is it the red rectangle in Fig.1, which however isn't explained (?) Maybe you can mask out the ``irrelevant'' regions?

- a very minor (bottom of first paragraph in the section 2.1): could you please provide a rough km scale for the 1/8 degree resolution. Of course this is slightly variable depending on the latitude, but is it something like ~12km?

- again extremely minor: just beneath Eq.2 “measurement operator” should be “observation operator”?

- I would perhaps suggest to put the first two paragraphs in section 2.4 behind the section 2.1, or maybe behind the section 2.2, so you say clearly how the ensemble is generated, how many members are run (and so on), around the same time when you talk about ensemble DA. I got firstly confused how little information is provided about the ensemble generation until I realized it’s been put behind the “Observations” section.

- regarding the ensemble generation: how was the parameter sensitivity determined, in order to perturb the ``right'' parameters to account for the model uncertainty? Physical model parameters weren’t perturbed (e.g. vertical diffusion parameter values)? If so, why? I know 20 member ensemble is itself limiting, but perhaps some more reasoning on how the ensemble was generated would be desirable..

- it seems that the observation uncertainties don’t come with the observation product (?), but based on what you estimate the 35% observational uncertainty for total chlorophyll, or the physical observation errors? This might be of importance, since you are aiming at estimating model variances quite accurately with the ensemble, so the true state estimate can be quite sensitive to the observation error estimates.

- btw do you assimilate chlorophyll, or log-chlorophyll? If the former, how do you treat the problem of non-Gaussianity?

– what exactly is the 7 day window assimilation at the end of third paragraph of section 2.3, and how exactly does it help with data sparsity, should be explained more in detail.. E.g. is this that you assimilate more data that have been otherwise merged in the OC CCI products?

- I am not particularly great expert on EnKF, but maybe you could say for a general readership a bit more about how exactly are the correlation length-scales (especially the vertical) calculated from the ensemble? There is something on the spurious correlations and localisation, but perhaps you can say a bit more? Also I understand correctly that SSH, T & S are updated without updating also the horizontal velocities? Could that be also discussed slightly more?

- the skill metrics at the end of section 2: why don’t you also include the overall bias? I find that calculating bias always carries worthy information (at least worthy enough to be mentioned somewhere in the paper).

Section 3

Nice, just some relatively minor clarifications would help:

- section 3.2: I find it surprising that addition of Argo (physical) data to the assimilation (Daargo) has such impact on surface chl (Fig.6), especially if it didn't change much T & S (Fig.4)? Could you comment a bit more?

Section 3.2 paragraph 2: where are the nitrate data coming from? Maybe I'm getting confused, but nitrate data weren't mentioned between the BGC Argo data in section 2.

specific Figure-related comments:

Fig.3: although I understand what the Figure is trying to demonstrate, it might be better to show the model – observation differences for the different runs, or at least show them for the free run next to the existing panels?

Fig.4: its interesting how little difference there is in subsurface T between sat DA and argo DA! Can you discuss how much of this is due to the assimilation length-scales and how much due to the model dynamical adjustment? Btw what was the relationship between the spatial positions of Argo floats and BOEM floats? This hasn't been shown..

Fig.7: perhaps in this case there is no need to reproduce exactly Fig.4, since the nitrate concentrations are very similar between the three simulations? Maybe it's better to have the first row and then differences between the DA and the free run, since this would show more clearly the changes introduced by DA? Also why there aren't the observed nitrate concentrations similarly to T in Fig.4? It's interesting that nitrate and surface chlorophyll aren't correlated as much as nitrate with temperature (Fig.10), I understand that's because of non-linearity, but in my region and model of experience this non-linearity still produces overall strong correlations, just highly variable in time (including changing signature)..