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Interactive comment on "Impacts of physical data assimilation on the Global Ocean Carbonate System" *by* L. Visinelli et al.

Anonymous Referee #1

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The authors describe results from the assimilation of temperature (T) and salinity (S) data into the NEMO-PELAGOS ocean-carbon model using a variational approach and a corresponding control run without assimilation. The assimilation of T and S improves the representation of CO2 partial pressure (pCO2), dissolved inorganic carbon (DIC) and alkalinity (ALK) compared to the control. The work is considered as a first step towards constraining the space-time evolution of surface ocean pCO2 and towards the ultimate goal to constrain ocean-atmosphere carbon fluxes.

I appreciate that data assimilation is a difficult task and a considerable technical challenge. Nevertheless, I am somewhat disappointed by this manuscripts. It appears to be a description of the current, intermediate state of work by the group.

My suggestion is to update the treatment of ALK in the model, perform simulations

where indeed pCO2 data are assimilated and then resubmit a completely revised paper.

General comments

1) A) The treatment or better non-treatment of the CaCO3 cycle and thus ALK is highly cumbersome. Changes in ALK exert a strong control on pCO2. The authors employ a complex representation to simulate organic matter production and export considering iron, silicate, phosphorus, nitrogen as nutrients and different functional groups from bacteria to zooplankton. They also discuss how variations in S affect ALK. On the other side, and in sharp contrast to the complexity of the ecosystem model, they neglect the first order feature of CaCO3 formation in the euphotic zone and dissolution in the thermocline and deep ocean.

In my opinion, it does not make sense to apply a complex ocean circulation model and a complex model for the organic matter cycle in a variational approach, while at the same time neglecting first order drivers of ALK and thus pCO2 and air-sea flux (see e.g. (Sarmiento and Gruber, 2006).

b) The adjustment of the GLODAP initial ALK fields by 50 micromol in the entire Pacific is huge. This corresponds to an adjustment in pCO2 of about 30-50 ppm. How can this be justified?

c) It is unclear whether the impact of organic matter formation and dissolution on ALK is taken into account. Please clarify

A proper treatment of the CaCO3 cycle and of ALK is needed before publication.

2) A) The model is not spun-up towards equilibrium, but run from rest starting in 1988. I am surprised that the model is not properly initialized with a spin-up close to equilibrium as the resolution of the model is with 30 vertical layers and ∼2x(0.5 – 1) degree not as high as to prevent a spin-up. This would allow the authors to evaluate the model's physics and biogeochemistry in a comprehensive way by comparing simulated tracer

distributions and water mass formation rates with observations (e.g. GLODAP, World Ocean Atlas, Talley et al. etc), before applying the model in the data assimilation.

It would be nice to see a discussion how well the model is performing in terms of simulating nutrient distributions and thermocline ventilation, e.g. as indicated by the distribution of CFCs, radiocarbon or anthropogenic carbon.

How does model drift affect results in the control?

B) I assume that the assimilation of T and S implies adding/removing heat and salt. How do the sources and sinks of heat and salt compare to data-based reconstructions of air-sea heat and freshwater fluxes?

3) Expand information on wider context A) Why do the authors think that data assimilation is preferred or equally valid to other approaches applied to reconstruct pCO2 and air-sea fluxes such as atmospheric inversions, neural networks or similar interpolation approaches? A discussion of this point may be useful also in the context of the recent special volume in BG on air-sea fluxes (e.g.; (Schuster et al., 2013;Sarma et al., 2013;Ishii et al., 2014;Lenton et al., 2013;Rödenbeck et al., 2013).

b) Would it be useful to include also the most recent SOCAT version 2 pCO2 data (Bakker et al., 2013)

c) How does this work compare to previous ocean-carbon and air-sea flux data assimilation studies? (e.g.,(Mikaloff Fletcher et al., 2006;Mikaloff Fletcher et al., 2007) (Gerber and Joos, 2010, 2013;Gerber et al., 2009) (Schmittner et al., 2009) (Rödenbeck et al., 2013) or (Schlitzer, 1988, 2004)

4) Text structure: Metrics should be defined in a subsection of the method section to avoid unnecessary repetitions.

Specific comments

1) abstract, line 1: The first word in the abstract is “prognostic simulations”. I find this a

bit misleading as this manuscript deals not at all with prognostic simulations.

2) Methods, section 2.2: a) How are Fe, Si, P, N .. initialized? b) what is the atmospheric pCO2 boundary?

3) p5406, line 15: How do you define the error covariance matrix? Could you describe this a bit in more detail.

4) p5406 Line 23: Is there also a vertical correlation length scale involved? Could you please also specify over which horizontal and vertical domain you assimilate T and S?

5) 5407, line3/4 Are there physical reason to reject observations? How does this procedure affect the RMSE or similar metrics? Could it be that the practice of throwing away observations yields ‘artificially’ low RMSE?

6) 5407, l7: is there no convection?

7) 5407 line 10-15: you may see Gerber and Joos, OM, who also assimilated T, S fields

8) 5407, l23, eq. 2: I miss here nitrate alkalinity which should not be neglected. Could you please indicate whether you neglected nitrate alkalinity in the definition of ALK?

9) 15408 line 20: It would be illustrative to provide also the relative change in pCO2 per change in ALK and DIC

10) 5413, line 15: are there problems with model drift at depth?

11) section 5: I would prefer here a discussion of results instead a description of metrics. The latter should go to the method section.

12) 5416, eq. 9: Could you define G.

13) p5417 line 10 to 21. suggest to delete text as it provides hardly any information

14) 5418 line 5-7: unjustified claim, please delete. It seems not a sufficient requirements that model performance is just slightly better than when completely neglecting a first order process.

15) 5418, line 19: Mentioning that you neglected the first order process of CaCO3 formation comes way to late here.

16) 5418, line 25: The ad-hoc correction for alkalinity is not justifiable

17) 5427: table 1 is not needed

18) figure 2: It seems you are comparing to station data. The labels 'GLOBAL', 'Atlantic' etc are then very misleading. Please use other labels (e.g. TOGA-TAO) etc. What means 'GLOBAL'

19) figure 2: What about pCO2? Please show also RMSE for pCO2,e.g. as compared to SOCAT version 2 data.

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