Earth System Dynamics Discussions

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Comment on esd-2021-4

Anonymous Referee #2

Referee comment on "Trivial improvements in predictive skill due to direct reconstruction of the global carbon cycle" by Aaron Spring et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-4-RC2, 2021

Review of "Trivial improvements of predictive skill due to direct reconstruction" by Spring et al.

General comments

The authors have performed a set of perfect model experiments to investigate the impact of the nudging various physical (indirect reconstruction) and biogeochemical variables (direct reconstruction) on the reconstruction of the ocean, land and atmospheric carbon cycle. Further, they look into how this reconstruction impacts the predictive skill of the carbon cycle. They found that nudging of physical state variables reconstructs the carbon cycle well, and that an additional nudging of biogeochemical state variables only gives marginal improvement, and sometimes even deteriorates the reconstruction. Also for the predictive skill they do not find any substantial improvements of directly reconstructing the carbon cycle.

This manuscript is an important contribution to the research on carbon reconstruction and prediction. I do not know of any perfect model studies investigating biogeochemical reconstruction and the importance of direct versus indirect reconstruction. Further, their results on the predictability add confidence to the results of another perfect model study that showed that the biogeochemical initial conditions play a minor role for the predictability of t ocean biogeochemistry.

However, there are some improvements that can be done before a potential publication:

- I have one question mark regarding the presentation of your results. For the carbon reconstruction, why do you present the results from the atmospheric nudging and the atm+ocean+ice nudging for the indirect simulations? I do not know of any prediction systems that nudge atmospheric variables only (I may be wrong). It is rather the opposite. It is standard practice to nudge ocean variables, and then there might be nudging of atmospheric variables as an "add-on". I therefore do not see what the scientific community gains from your experiment with atmospheric nudging only. It is quite intuitive that you cannot reconstruct the ocean carbon cycle by assimilating only atmospheric data. I think that the manuscript would greatly improve if you presented the simulation where you nudge the ocean only, and then the atm+ocean+ice simulation. If you want to go into details you could even have one ocean simulation, one ocean+ice and one ocean+ice+atmosphere. In that way you could see what additional skill you could gain when nudging sea ice and atmospheric variables for carbon reconstructions.
- The discussion of the results needs some work. Specifically, the manuscript lacks a deeper discussion on advantages/disadvantages with you various nudging schemes, and the reasons behind (i.e. how does it affect the physics/biogeochemistry, regional differences). For example, why does the indirect reconstruction result in higher correlations in the subtropical ocean compared to the extratropical ocean? It is already done to some extent, but it can be improved. Moreover, you need to put your results more in context to what is currently done in reconstruction/prediction research. For example, at the moment it is not clear from the text what you want to show with the different indirect reconstruction schemes, and what knowledge we can gain from it. Overall you need to connect your results better to the literature. For example, you refer to the Servonnat et al., 2015 and Fransner et al., 2020 papers in the introduction, but you do not put your results into context with them in the discussion. How does your results compare to other studies with biogeochemical reconstructions/predictions?
- The structure of the paper can be improved, specifically, the number of sections and subsections can be drastically reduced. See suggestions under Specific comments.
- Work needs to be done on the language/formulations and flow on sentences. I have put some suggestions under "Technical Notes" below.

Specific comments

- I suggest you merge sections 2.2, 2.3 and 2.5 to one. You could use subsubsections for the different kinds of simulations.
- Similarly, I would suggest you to merge sections 2.4 and 2.6
- Furthermore, I would suggest you to make sections 3 and 4 to subsections in a section "Results" or "Results and DIscussion".

(It is difficult for me to understand whether sections 3 and 4 are supposed to be only Results or Results and Discussion)

- You can furthermore reduce the number of subsections in section 3. I would simply merge 3.2.1 and 3.2.2 under 3.2, 3.3.1 and 3.3.2 under 3.3 and 3.4.1 and 3.4.2 under 3.4.
- Why don't you do any indirect reconstruction of the land physics? You should explain this.
- Under section 3.4 you write that you will start by examining figure 4, but then you only mention it briefly in the end of the section.
- Why are you looking into seasonal timescales when it comes to atmospheric CO2 (figure 5), while you are looking into 10 year chunks for the land and the ocean? I would focus on one time-scale to be consistent, but maybe I'm missing something.
- The manuscript would gain a lot of you would discuss the regional patterns in our results in more detail. For example, for the ocean we see large differences in the reconstruction skill in the tropics compared to the extra-tropics, why is this?
- In your ACC plots, the ACC that is significantly different than internal variability, is found in areas of lower correlation. Why is this? Shouldn't the significant result be related to higher correlation?
- Why have you chosen RMSE as a measure of skill for your predictions, while you use RMSE and ACC for your reconstructions? Did you try also ACC for your predictions?
- Section 4.2: if the cVeg is not predictable, what is it then that yields the predictability in the air-land CO2 flux? If you cannot give an answer to this you should at least discuss it. (for the ocean you are looking into the oceanic pCO2 that is an important driver for the air-sea co2 flux.)

Technical Notes

Abstract: The abstract is long and heavy to read. Try to shorten it down and make the text more fluent. Some suggestions:

- The first sentences are not capturing the reader., and need reformulation. Maybe you could start by saying that state-of-the art climate prediction systems now include a carbon component. Then you shortly explain that while there is assimilation of physical state variables, this is not the case for the biogeochemistry.
- line 8 : Why have you chosen the word "target"? It is quite abstract, could you choose another word? If you want to keep it, maybe just a reformulation of the text would help.
- lines 10-15: This is very much into technical detail, and I suggest to remove most of this from the abstract.
- I would suggest not to go into ACC's and RMSE's in the abstract, and instead give your interpretations of your results in words. If you want to keep it, only mention it for the most important results.

Introduction:

- Move lines 47-56 to the end of the introduction
- line 55: the word perfect- model target reconstruction framework is very long. Can you make this shorter? Can't you just write "perfect model framework", and then you describe it in more detail in your methodology?
- Lines 74-77: This part, which is on ocean physics, comes in the middle of your discussion on initialization of the carbon cycle (in between the references to Li et al., 2016, 2019 , Seferian et al., 2018, Lovenduski, et al., 2019 and Fransner et al., 2020). I suggest moving it to the end of the introduction where you discuss your approach. If I understood it right you got inspired from this study?
- lines 74-75: You should make it clear already in this first sentence that Servonnat et al is a perfect model study.
- lines 75-77: This sentence is difficult to understand. There are several reasons behind; i) "target reconstruction approach" is quite abstract, and needs clarification, i.e. why is the method of Servonnat et al., 2015 called like this? Why does it allow to directly assess the quality of reconstructed initial conditions? The last part "which is useful and practical to know for forecaster issuing a forecast" also need a reformulation, or can be removed completely .
- lines 84-85: in which way is this more theoretical?

Methods:

- line 103: merge the two parentheses to one.
- lines 103-107: you repeat the reference Mauritsen et al., 2019 three times. Maybe this is not needed if you talk about the same model all the time
- lines 115-117: here comes the explanation for the "perfect-model target reconstruction framework" that I was looking for in the introduction. I would suggest to move these first lines there. Alternatively, you only use the phrasing "perfect model framework" before this section. Then the exact methodology and details behind are described here. In that case you can keep these lines here. Please consider making two sentences out of this one. it is very long.
- lines 117-118: how can the restart file for year 2005 come from the per-industrial control simulation?
- Line 119: you have already described the model setup in the previous section, and do not have to write about is again here.
- lines 126-128 + equation: here comes the explanation for the use of the word "target" that I was looking for earlier. I would suggest moving this after the first sentence in section 2.2.
- lines 132-134: The observational data is not needed at each model time step, but at the time scale of relaxation, right?
- line 136: I would suggest to write "a shorter relaxation time scale" instead of a "a

stronger nudging strength"

- line 138-139: you need to clarify why you nudge the logarithm of the surface pressure. Is the reference to Pohlmann et al related to this, or to the nudging of all variables?
- line 139: I would suggest you to briefly mention the 63 spherical harmonics in the model description.
- line143: I don't understand the use of the word "only" here. Should it be nudging the atmosphere only?
- line 144: consider merging the parentheses that are coincident.
- line 156: consider removing this first sentence, the information in it is basically repeated in the next sentence.
- Lines 157-160:To avoid repetition, I would remove the "over 10 year windows" in the first sentence. In the second sentence you can then explain that you do the calculations over ten year windows, and why you do it. Moreover, I would move the second sentence to be after the explanation of your skill-metrics.

Section 3:

- figure 1: the letters in subplots g-l are barely visible, consider changing the color to white. Check this also for the other figures.
- section 3.1: why are you looking at these physical variables, specifically? A short explanation would be good.
- lines 211-212: The ACC is not significantly better in most grid cells from what it looks like in Figure 1.
- line 238: "the state variable of the ocean carbon sink surface ocean pCO2" needs reformulation
- Lines 246-248: Here it would be interesting if you discussed the results more in detail and put it into context with the results in 3.1. For example, how come that the atmospheric nudging only improves the reconstruction in the tropics?
- Line 260: the biases in the direct reconstruction does not look larger than the ones in the indirect reconstruction for pCO2 in Figure 2 ?
- section 3.4.1: here you are again describing the effect of the reconstruction on the land and ocean carbon cycle as you did in sections 3.2 and 3.3 . I would remove it from here and only discuss the effect on the atmospheric CO2. The description of the land and the ocean carbon cycles should be done in the previous sections.

Section 4:

- figure 6: Use the same y-label for the two rows in the left hand side, i.e. either RMSE or root mean squared error
- line 389: add flux after "global air-sea CO2"
- 373: you have to describe how you construct this perfectly initialized ensemble in the methods. You write that it is started from perfect initial conditions. I assumed that you applied some perturbation to these restart files?

Section 5

- Line 435: shouldn't it be "reconstruction of the physical ocean fields"?
- Line 435-436: It is quite expected that you cannot reconstruct the ocean carbon cycle by nuding the atmosphere only no? You need to discuss this in more detail. Also please put your results into context of what is currently done in state-of-the-art climate prediction models, and other studies showing that the ocean carbon cycle/biogeochemistry can be reconstructed by nudging physics only (i.e Li et al., 2016, Seferian et al., 2014, Park et al., 2018 ...).
- Lines 451-460: you need to put these results into context with the Fransner et al., 2020 study.

References:

Séférian et al 2014, Predictability tropical marine productivity, PNAS, DOI: 10.1073/pnas.1315855111

Park et al, 2018, Modeling Global Ocean Biogeochemistry With Physical Data Assimilation: A Pragmatic Solution to the Equatorial Instability, Journal of Advances in Modeling Earth Systems, https://doi.org/10.1002/2017MS001223,