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Interactive comment on "Predicting near-term changes in ocean carbon uptake" *by* Nicole S. Lovenduski et al.

Anonymous Referee #2

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The authors have investigated the predictability and predictive skill of the ocean carbon uptake by using a large ensemble of 40-member decadal prediction and historical simulations based on NCAR CESM. They found a prominent improved predictability of the ocean carbon uptake in the initialized simulations in comparing with the uninitialized historical simulations and the persistence forecast. Furthermore, they attribute the predictability of ocean carbon uptake to the dissolved inorganic carbon and alkalinity. The outcome of this study is an important contribution for understanding and predicting variations of the ocean carbon uptake and the global carbon cycle, which are crucial for estimating climate change. Moreover, reconstruction and near-term predictions of global carbon cycle show large potential for supporting the future carbon stocktaking. Therefore the study on this topic merits publication on the Earth System Dynamics.

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The manuscript is well written and the results are clearly stated, however, the conclusions are not quantitatively precise and not statistically robust from the results. This together with some other issues listed below prevents me accepting this manuscript at its present format.

1. The authors claimed a potential predictive skill of up to 7 years in the abstract and conclusions. Is this conclusion from Fig. 3d by comparing the initialized forecast to the uninitialized forecast? The authors did not do a statistical test if the difference between initialized and the uninitialized forecast is significant. The red circles only show if the correlation of the initialized forecast itself is significant. It seems to me that the initialized forecast (red dots) at lead time of 6 and 7 years are very close to the uninitialized forecast, these are probably not significantly distinguishable. As the improved skill due to initialization is a main quantitative conclusion in this manuscript, it requires a sound significant test, such as the commonly used bootstrap method (Goddard et al., 2013), which is also suggested by the Decadal Climate Prediction Project (DCPP) (Boer et al., 2016). In addition, the authors only show time-series and maps of predictability at lead time of 1 year. Given the high predictability of ocean carbon uptake as stated in this study, time-series and maps of predictability at longer lead time at least of 2 years are more representative.

2. The authors estimated both potential predictability against reconstruction and predictive skill against observation-based data product. The two results were separately discussed in the main text, however, the conclusions are mixed especially in the abstract. It's quite difficult for the readers to distinguish the origin of the conclusions, they are from potential skill or skill against observation. For this reason, the abstract needs to be reorganized and make it clearer. Furthermore, the connections between potential predictabiltiy and predictive skill are weak in the manuscript. How consistent/inconsistent are the predictability and the predictive skill? What would be the implication of potential predictability to the predictive skill versus observation?

3. The initialized simulations were started from a forced ocean-sea ice simulation for

the ocean-sea ice component, but were started from the CESM Large Ensemble for the atmosphere and the land components (details were described in page 3 lines 8-13). This means that the ocean and the atmosphere and land are most probably in different climate state/phase, they need to adjust to each other and approach a new equalibrium. The mismatch of initial conditions in the ocean and in the atmosphere and land would affect the variations and predictions of the system, especially for the carbon flux across the boundaries. Discussions of the effects of mismatch in the ocean and the atmophere and land are necessary. Can the model drift due to the mismatch be largely eliminated by the drift correction?

4. As stated in McKinley et al. (2016), some ensemble members of the CESM-LE have problem in the ocean biogeochemical outputs. McKinley et al. (2016) used only 32 ensemble members of the CESM-LE, because some ensemble members were discarded due to a setup error which leads to corrupts of ocean biogeochemical output. In this study, the authors use 40 ensemble members as written in Page 4 lines 5-9. How do the authors treat the ensemble members with setup error in this study?

5. The numbers in Fig. 10 are not significant and deducible from Fig. 9. For instance, the maximum forecast lead time in biome 3 (NP STSS) is 8 years in Fig. 10, but if we look at Fig. 9a, the correlations at lead time beyond 4 years are not significant and end up with less than 0.2 at lead time of 8 years. As for biome 4 (NP STPS), the maximum forecast lead time is 7 years in Fig. 10, but the initialized forecast skill is not significantly higher than the uninitialized forecast skill at lead time of 5 years in Fig. 9b. Therefore, I think the numbers in Fig. 10 need to be carefully checked by taking into account the significant test and the relative magnitude of the correlations.

6. Table 1: the table caption and the title of the columns are unclear. I guess the “Initialized forecast” and the “uninitialized forecast” refer to forecast skill versus reconstruction, and the “Forecast skill” refer to forecast skill versus observation-based products. The time period used to calculate the correlations needs to be specified, especially for the “Forecast skill” which use much shorter period. In addition, statistical significant

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test information by highlighting of the numbers will be also helpful. Moreover, a table of predictability for the maximum forecast lead time will be necessary as supplementary information to Fig. 10.

7. Fig. 2: how different is the reconstruction comparing to the uninitialized simulations? Is the reconstruction closer to observations than the uninitialized simulaitons? It would be more informative to also include the climatology of the uninitialized simulations.

8. It is not introduced but I guess the authors use different time period for the drift correction and correlation calculation along different lead time. As shown in Fig. 3d, the red dashed line has a slightly positive trend, which indicates that the authors use different time period for the correlation calculation for different lead time. To make a consistent estimate of predictive skill along all the forecast range, it is better to use the same time period for all the lead years as suggested by DCPP (Boer et al., 2016, Appendix E) and previous studies focusing on the physical predictions (Hawkins et al., 2014; Smith et al., 2013).

9. Page 3 line 7: are the historical external forcings from CMIP5 or CMIP6 (i.e., the 5th of 6th Coupled Model Intercomparison Project)?

10. Page 5 line 16: "...for those forecasting year-to-year changes..." should be "...for those reproducing year-to-year changes..."

11. Page 6 line 5: "...the anomaly correlation coefficients are scaled to CO2 flux units..." The correlation coefficient itself is uniformed and has no unit, there is no need to further scale it. What are the results based on the correlation coefficients without the scaling? I think the results without scaling are similar to those based on the scaled correlations. It worths to check. One more question on the scaling formular: how do the authors calculate the $\partial\Phi/\partial x$, how long is the time step?

12. Page 6 line 22-23: "The similar predictability of DIC and Alk across many regions hints at an important role for ocean circulation, rather than biological productivity...,

in CO2 flux predictability." From this I understand that the biological productivity is a secondary regulation of CO2 flux, therefore the biome division is probably not a proper way to divide the global ocean for CO2 flux predictions. The last sentence is the same as line 8-9 on Page 7.

13. Page 8 line 26: "Li and Ilyina (2018)" should be "Li et al. (2016)", right?

14. Figure 4 caption: "CESM-DPLE initialized forecast lead year 1" needs to be revised and includes information of the counterpart of the correlation, e.g., "CESM-DPLE initialized forecast for lead year 1 with the reconstruction".

15. Figure 9: are the correlations based on detrended time series?

References: Boer, G. J., et al.: The Decadal Climate Prediction Project (DCPP) contribution to CMIP6, Geosci. Model Dev., 9, 3751-3777, https://doi.org/10.5194/gmd-9-3751-2016, 2016. Goddard, L., et al.: A verification framework for interannual to decadal predictions experiments, Clim. Dynam., 40, 245–272, doi:10.1007/s00382-012-1481-2, 2013. Hawkins, E., Dong, B., Robson, J., Sutton, R., and Smith, D.: The interpretation and use of biases in decadal climate predictions, J. Climate, 27, 2931–2947, doi:10.1175/JCLI-D-13-00473.1, 2014. McKinley, G. A., Pilcher, D. J., Fay, A. R., Lindsay, K., Long, M. C., and Lovenduski, N. S.: Timescales for detection of trends in the ocean carbon sink, Nature, 530, 469–472, http://dx.doi.org/10.1038/nature16958, 2016. Smith, D. M., Eade, R., and Pohlmann, H.: A comparison of fullfield and anomaly initialization for seasonal to decadal climate prediction, Clim. Dynam., 41, 3325–3338, doi:10.1007/s00382-013-1683-2, 2013.

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