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Earth System Dynamics
Discussions

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

Anonymous Referee #1

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In the manuscript 'Predicting near-term changes in ocean carbon uptake' Lovenduski and coauthors assess the predictability of the ocean carbon sink over the last decades using CESM-DPLE, a new large ensemble decadal prediction platform developed at NCAR. By realizing 40 decade-long ensemble members each year from 1954 to 2015, the authors estimate that the global ocean carbon sink is predictable up to 7 years in advance which is in the line of recent published estimates. The authors also investigate the drivers of this predictability and explain that it arises from the predictability of carbon-related fields (DIC and Alkalinity, setting ΔpCO_2 and hence carbon fluxes).

The paper is well-written and the analyses are sounds. I much appreciate this work which explores the predictive capability of the current generation of ESM. Nevertheless, I think this paper needs some clarification that have to be addressed first, and which

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Discussion paper

prevent me of accepting this paper in its present form.

General Major Comments: 1- My first comment concerns the assessment of the initialization procedure which is the core of decadal prediction. Here the other briefly describe this step but do not provide a complete evaluation. ESD paper are not limited by the length. Thus I recommend to include a new section to discuss the initialization procedure because: (1) full fields restoring implies a model drift; this is an interesting to document how it impacts the biogeochemical fields, especially the carbon related fields and nutrients fields that could generates non-linearity in the drift (2) your initialization strategy fails at capturing the recent variability in the ocean carbon sink as suggested by SOM-FFN dataproduct. It could be interesting to show other variables such as SST or the AMOC to support the fact that your initialization procedure is doing a good job. 2- Further discussions is needed when discussing the drivers. It could be interesting to document if your model gives a longer predictability horizon for SST, DIC, Alk . . . than that of ocean carbon sink and compares this result to the persistence. At least for DIC and Alk which have a long-lasting memory it would be helpful to demonstrate that your model's predictability beats the persistence for those fields otherwise it might suggest that the predictability in ocean carbon sink is supported by the persistence of DIC and Alkalinity anomalies. 3- Finally, further discussions are needed to discuss how this work compared with previous works based on different prediction system such as Li et al. (2016), Séférian et al., (2018) [using ESMs] but also all the recent studies focusing on ocean physical variables (e.g., Kim et al., 2012)

Specific comments: Page 1 Title: I suggest to modify the text because "near-term changes" could also implies anthropogenic carbon sink. This latter is rather well captured by ESM without initialization. I suggest to use "multi-annual variations" instead. L2 : please define somewhere what your mean by "near-term" L4: of an Earth system model L4: initialized forecast=please explain the initialization somewhere in the abstract and avoid the terminology initialized forecast because of forecast is generally initialized L9: moderate predictive skill= please explain the predictive skill measure L11:

initialized predictability= predictability also implies initialization. I suggest to remove 'initialized' L21: I suggest to include observational references instead (e;g., Landshutzer et al., 2016)

Page 2 L7-15 Please expand the discussion by including the key limits of the decadal predictability that were highlight in the literature. For example, the first attempt from Keenlyside et al and Smith et al in 2008 which were challenged 10 years after by the recent observations. Besides, you could include a better rationale of the first attempt in the Earth system community such as Li et al. 2016, Séférian et al. 2018 for the ocean carbon sink and Séférian et al. 2014 for the net marine productivity. And the use of statistical model such as Betts et al. (2016, 2018) for atmospheric CO2. L15 Please add Resplandy et al. (2014) which describes how far the decadal variability in ocean carbon fluxes differs between models

Page 3 L7-13: Please expand this paragraphâĂŤ see my major comments L20: a reasonable job is not enough to determine if a model is fitted for purpose. Could you please provide further details such as the spatial correlation, the RMSE . . . L23: 10-14 Kelvin ? This is a really small perturbation. Have assessed if this initial perturbation lead to populate the full range of model variability as diagnosed from the piControl ? It is important to tell the reader if you chose to populate this uncertainty or instead to stay close from the initial conditions L33: Please provide a figure of the ensemble with the drift as in Kim et al 2012 in addition to the de-drifted ensemble. You could add on panel on figure 1 to show that.

Page 4 L9-11: I'm a bit puzzled here. Unless I misunderstood McKinley et al 2016 and Lovenduski et al. 2016 used a CMIP5-style CESM and hence CMIP5 forcings to performed their analyses. Here, CESM-DPLE is setup for CMIP6 and hence use CMIP6 forcings, right ? If it does, several external forcing have been revised between CMIP5 and CMIP6. This is the case for the volcanoes (which influence the predictability). Could you please comment this point ?

Page 5 L10-11 What happens if you consider the full observational time-series ? Besides could you please explain why the correlation of the uninitialized simulation slightly increases with lead time ? On Figure 3 8 and 9 please indicate the correlation limits (R*) on the graphs and indicates the level of confidence (and the number of degree of freedom used for the t-test) employed; this information is missing. L15: why are you talking about emissions and terrestrial CO2 uptake. Your model set-up employs CO2 concentration as prescribed in the forcing, correct ? L25: linearly detrended forecast: have you done this detrended at grill-cell scale or have you applied the same detrending globally ? L26: as you suggested that your modelling plateform is able to predict ocean carbon sink up to 7 years in advance it could be usefull to show what happens at lead time greater than 1 year. Could you please replace the figures showing lead-time (LT) 1 by LT 7 and/or moving LT1 Figures as supplemental data

Page 7 L2: please explain what are your forecast skill and what is the limit for a skillfull predictability at a given confidence L11: predictability or persistence âĂŤ please see my major comments L23: You could state that this biomes does not see an impacts of your initialization procedure. Maybe the sea-ice influence regions are not restored to the observations? This is why I suggest to further develop this point with a new section in the msâĂŤ see my major comments

Page 8 L1: forecast and the uninitialized= remove 'forecast and' L15-21: Please further discuss the limit of your approach= for example your initialization procedure fails at capturing the observed variability. You estimate a predictability of 7 years with only 20 years of data (which is not enough). I suggest the authors to discuss this point and hence to highlight the most of the results presented in this work relates to the potential predictability rather than an effective predictability.

Betts, R. A., Jones, C. D., Knight, J. R., Keeling, R. F., Kennedy, J. J., Wiltshire, A. J., . . . Aragão, L. E. O. C. (2018). A successful prediction of the record CO 2 rise associated with the 2015/2016 El Niño. Philosophical Transactions of the Royal Society B: Biological Sciences, 373(1760), 20170301. https://doi.org/10.1098/rstb.2017.0301

Betts, R. A., Jones, C. D., Knight, J. R., Keeling, R. F., & Kennedy, J. J. (2016). El Niño and a record CO2 rise. Nature Climate Change, 6(9), 806–810. https://doi.org/10.1038/nclimate3063 Keenlyside, N. S., Latif, M., Jungclaus, J., Kornblueh, L., & Roeckner, E. (2008). Advancing decadal-scale climate prediction in the North Atlantic sector. Nature, 453(7191), 84–88. https://doi.org/10.1038/nature06921 Kim, H. M., Webster, P. J., & Curry, J. A. (2012). Evaluation of short-term climate change prediction in multi-model CMIP5 decadal hindcasts. Geophysical Research Letters, 39(10), L10701. https://doi.org/10.1029/2012GL051644 Li, H., Ilyina, T., Müller, W. A., & Sienz, F. (2016). Decadal predictions of the North Atlantic CO2 uptake. Nature Communications, 7(May 2015), 11076. https://doi.org/10.1038/ncomms11076 Resplandy, L., Séférian, R., & Bopp, L. (2015). Natural variability of CO<inf>2</inf>and O<inf>2</inf>fluxes: What can we learn from centuries-long climate models simulations? Journal of Geophysical Research: Oceans, 120(1). https://doi.org/10.1002/2014JC010463 Séférian, R., Berthet, S., & Chevallier, M. (2018). Assessing the Decadal Predictability of Land and Ocean Carbon Uptake. Geophysical Research Letters, 45(5), 2455–2466. https://doi.org/10.1002/2017GL076092 Séférian, R., Bopp, L., Gehlen, M., Swingedouw, D., Mignot, J., Guilyardi, E., & Servonnat, J. (2014). Multiyear predictability of tropical marine productivity. Proceedings of the National Academy of Sciences of the United States of America, 111(32). https://doi.org/10.1073/pnas.1315855111 Smith, D. M., Cusack, S., Colman, A. W., Folland, C. K., Harris, G. R., & Murphy, J. M. (2007). Improved Surface Temperature Prediction for the Coming Decade from a Global Climate Model. Science, 317(5839), 796–799. https://doi.org/10.1126/science.1139540

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