Review of “Northern North Atlantic climate variability controls on ocean carbon sinks in EC-Earth-CC” by Pedersen et al.

Summary:

The authors assess the role of dynamical and physical processes in the interannual-to-decadal variability of CO2 flux in the North Atlantic divided into four sub-regions, using the EC-Earth3-CC model. They start with a spatial composite analysis of CO2 fluxes and variables they identify as potential important drivers of the CO2 fluxes, including mixed layer depth (MLD), sea surface height (SSH), sea ice concentration (SIC), sea surface temperature (SST), sea surface salinity (SSS), air-sea pCO2 difference (dpCO2), and wind speed (W). This is followed by a correlation analysis with different indices. The authors find strong relations with several of the variables indicating complex dynamics, however, the wind speed stands out as the most important one.
Based on the results from the composite and correlation analysis, the authors construct linear regression models with which they can reconstruct up to 68 % of the modelled CO2 flux variability based on physical indicators only.
The authors address an important topic; understanding the variability of air-sea CO2 fluxes. The manuscript has the potential to be an important contribution to the community. However, before being published I think the authors need to work on the objective and the red line of the story, and potentially on their methods of analysis.
More suggestions are found below.

Major comments:

- **Red line of the story:**
  1) The authors start off by “reconstructing” the simulated CO2 fluxes in their different regions of choice by using output of SST, SSS, W, dpCO2, SIC put into equations 1-3. The argue that this reconstruction “for our ability to explain from physical quantities the model variability.” For this , I suppose that the SSS and SST goes into the K0 (the solubility). This gives the impression that the authors would like to reconstruct the simulated fluxes based on these 5 variables.
  2) Then, a composite analysis is performed. Apart from the five variables above (which goes into equation 1-3), the authors also include MLD, SSH.
  3) In the correlation analysis with different indices, additional variables(indices) are introduced.
  4) In the end, for the regression analysis, only a few of the indices in 3) are used.

Starting off as you do under 1), gives the impression that you will separate the contribution of each component in equation 1-3 to the variability. This is, in itself, an interesting question. But in the end, this is not what you do, which confuses the reader and make the story less coherent. Addressing this is a question of how you want to tell your story. Either you can do a local analysis and do a decomposition of each component of these equations, which you also partly do in the composite analysis. When it comes to many of the other variables/indices, it is more about the relation to large scale dynamics/circulation (SSH, SPGi, AMOC), but that also impacts the local drivers (SSS, SST, dpCO2, SIC). If you want to keep all of these variables you could consider dividing the analysis part in to one local and large scale part. Otherwise, I would think that there is already enough material to only choose variables related to equations 1-3.

- **Relation to MLD:**

I think you need to consider removing the MLD from the analysis. Deeper mixed layers does not necessarily lead to increased CO2 uptake (locally). If the deepening of the mixed layer would bring water under (over) saturated in pCO2 to the surface, then it would allow for a local increase(decrease) in the CO2 uptake. However, in many cases it does not look like the pCO2 patterns perfectly match the MLD pattern in your composite analysis. In any case, if you would include dpCO2 in your regression model (see comment below), this effect would be taken into account.
Many of the aspects I mention above are also mentioned by the authors in the MS.

- **Relation to dpCO2:**

You have chosen indices that more or less covers all the parts of equation 1-3, except the dpCO2. If you want to base your study on equations 1-3, I would also include a dpCO2 index that you should try to put in the regression model.
At some point you mention that you want to reconstruct the CO2-fluxes based on physical indicators only. The reason for this is unclear, but in the discussion, you argue that with such a reconstruction model CO2 fluxes could be reconstructed also for Climate Models running without biogeochemistry. I find this argument not very strong since today the majority of ESMs are run with biogeochemistry, and we will unlikely go back to models without. Clarifying this would make the ***objective*** of the story clearer.

- **Relation to SSS and SST**

SSS and SST does not only impact K0, but also dpCO2. In your analysis of composite maps and correlation analysis of with SSTi och SSSi, do you know if the relation with the CO2 flux is mainly through the effect on pCO2 or on the K0?

- **Section 4.3:**

I think that you need to be very careful before you can project the results from your regression model based on interannual-to-decadal variability to climate change scenarios, which act at completely other time-scales. You already saw a big difference between your interannual and decadal time scales. I think that for this purpose ESMs run with biogeochemistry under future scenarios are more appropriate (you could of course consider doing a decomposition of drivers in future scenarios, but that is out of the scope of this paper I believe).
Something that I think you should emphasis more in the discussion is our ability to understand and predict interannual-to-decadal variations in ocean CO2 uptake. Some references here include:

Ilyina, T., Li, H., Spring, A., Müller, W. A., Bopp, L., Chikamoto, M. O., et al. (2021). Predictable variations of the carbon sinks and atmospheric CO_2 growth in a multi-model framework. *Geophysical Research Letters*, 48, e2020GL090695. **https://doi.org/10.1029/2020GL090695**

Li, H., Ilyina, T., Müller, W. *et al.* Decadal predictions of the North Atlantic CO_2 uptake. *Nat Commun* **7**, 11076 (2016). https://doi.org/10.1038/ncomms11076

Fransner, F., Counillon, F., Bethke, I., Tjiputra, J., Samuelsen, A., Nummelin, A., & Olsen, A. (2020). Ocean biogeochemical predictions—initialization and limits of predictability. *Frontiers in Marine Science*, *7*, 386.

Minor comments:

L114: change maos to maps
L126: consider adding (CC) after carbon cycle
L128-129: atmospheric *chemical* composition ?
L130: The reference after NEMO3.6 should be Madec et al. , Döscher is for the full Earth System Model and should come after EC-Earth3 in the beginning of the sentence.
L132: Move the Aumont reference to just after PISCES-v2, and remove the Döscher reference, since it is not the reference for PISCES, but for EC-Earth
L133-136: It sounds from your description like the difference between EC-Earth3 and EC-Earth3-CC is that the CC one is run in an emission-driven mode. This would make sense because it would allow for an interactive carbon cycle and that carbon can be exchanged between the different model components. But in this case, since it was not run in an emission driven mode, what is the difference with EC-Earth3? It sounds like you in fact have been working with EC-Earth3 (which also include PISCES)?
L137: Here you come back to a description of PISCES, which you already started above. Please merge the two description parts.
L139: write (P : N : C = 1 : 16 : 122) instead of (P / N / C, 1 / 16 / 122)
L139: if you are mentioning the ratios of P:N:C you should also mention how the other two nutrients are treated in the model. However, since you are not specifically looking into how the biological pump impacts the CO2 flux, you may remove this detail.
L146: U is the wind speed at 10 m above sea level?
L151-152: I would not use the percentage sign since it is the sea ice fraction
L151-152: you have not defined kgCO2 and k'gCO2
L154-162 and Figure 1: Please plot the gridded CO2 flux from the Landschutzer data set next to your model CO2 flux, as well as the difference between the two. It will add much value to your paper to show the model performance compared to the observational-based dataset. Referring to figure 21b from Döscher does not add much value, since the reader needs to go to this paper and the map projection and colour map in that paper may be different.
L167: I do not see why you refer to figure 1b here?
L179: what kind of filter do you use for the filtering?
L208-210: "The level of reproducibility of simulated integrated fluxes from other parameters (Eq. 1-3) (F) and in particular their variability across time-scales from archived averaged monthly data will constitute an upper limit for our ability to explain

from physical quantities the model variability." This sentence is long and difficult to grasp. Consider reformulating and dividing in two. What does the (F) mean? Should it be after the simulated fluxes (F)?
Equations: The equations need numbering on the right-hand side.
L212: what do you mean by integrated flux? Should it be simulated flux?
L221-224: Specify that the SSS and SST dependency in this case is in the solubility constant (?). Otherwise, the reader may think about how these factors impacts pCO2 variability, which you are not investigating here.
Section 3.1: Consider changing the title to "Regional CO2 flux variability"
L248-253: You have provided explanations earlier for why most of these variables are expected to be important. However, you have not provided any arguments for SSH and MLD. Please provide a short line or two explaining how they can impact the CO2 flux.

Section3.1: when comparing the spatial pattern of the composites of co2 flux with other variables, you write "correlates", but you do not mention anywhere that you have done a spatial correlation of these composites. You may consider if such a correlation would be of value. If you choose not to do one, I suggest that you write "coincides" instead of "correlates".

L292-294: see major comment 2

L332-334: How is more saline water introduced to the system?

L357-356: see major comment 2

L530 Using monthly mean fields of SST, SSS and wind speed and pCO2 ?

L566-570: I do not see this positive relation very clear your regions, especially between the pCO2 and the MLD. For the Subpolar East the negative relation is very clear on interannual time scales yes.