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Interactive comment on "On the role of climate modes in modulating the air-sea CO_2 fluxes in Eastern Boundary Upwelling Systems" *by* Riley X. Brady et al.

Anonymous Referee #2

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I think this is a good paper that is publishable with minor to moderate revision. Some details of the methodology are insufficiently explained. The English is generally good, although there are some quirks of usage that suggest an inexperienced lead author whose more senior coauthors put rather less time into editing the text than they could have.

Overall structure:

I think that "Conclusions and Discussion" would be better entitled "Discussion and Conclusions", and Section 3 should be incorporated into the Results. The first paragraph of the Discussion covers a lot of different topics, and rehashes a lot of the Results. It

would be better to lead off with a summation of the main points, and then further discussion of each, broken into a larger number of shorter paragraphs each focused on a specific topic. The Introduction meanders about a number of related topics in a way that could easily give the reader the impression that high-resolution regional hindcast simulations were employed (3/15-20). There is nothing wrong with mentioning the utility of such tools, but ideally one should try to structure the Introduction in a way that focuses on (1) what is the problem at hand? (2) what tools were used to address it? and (3) what is novel in the analysis that sets it apart from what is already in the literature? Similarly, I don't think that the idea that previous EBC studies have not focused on CO2 (2/18, 3/8) is either accurate or relevant (and the sentence on 2/18-20 is simply ugly). Statements like 7/27-29 are also unnecessary; this section is properly part of the Results, and statements like this belong in the Discussion (see also 6/11-13, 7/10-11).

Methods

I don't think the definitions of the boundaries of the boxes are sufficiently explained. The outer boundaries of the boxes in Figure 3 are not parallel to the coastline, so "from the coastline to 800 km offshore" hardly seems adequate. Turi et al only use the 800 km figure in general terms, in reference to the approximate domain of influence of coastal upwelling on the thermocline depth. In Figure 1, the boxes seem to indicate an approximately (but not exactly) constant distance in the E-W direction. The boxes in Figure 3 are quite different. The regions considered in Figures 6-9 are similar, but not necessarily identical, to those in Figure 1. A clearer explanation is warranted, especially given that many of the analyses shown are for regional means over these boxes. (Note also that the second half of Section 2.2 has nothing to do with the topic specified in header.)

For the regressions onto the climate indices, it should be be more clearly stated what the independent variable and its units are. References to a "1 degree El Nino" or 1 SD of NAO are better than nothing but not really adequate. If NINO3 is defined as a temperature anomaly in K, then a regression coefficient of CO2 flux on this index will have units of mol mˆ-2 yˆ-1 Kˆ-1 (e.g., 11/2). Similarly, the NAO has units of SLP. I'm not sure what the units of the NPGO are. But a statement like "The direct regression of DeltaF onto the NPGO results in an anomalous uptake of 0.10 mol mˆ-2 yrˆ-1" seems incomplete, because the reader does not know how large an anomaly in the NPGO is required to give rise to X mol mˆ-2 yrˆ-1 of CO2 flux anomaly. The statement (12/10) that stronger winds in the CanCS "leads to the highest relative CO2 flux anomaly of any system" seems misleading because the independent variable for these regressions is different in each case. Maybe there is some basis for making this comparison, but it has not been clearly explained.

The statistical tests applied are inadequately explained. The discussion of autocorrelation (5/27) appears out of nowhere without context. I agree that autocorrelation is important and you have to correct for it, but up to this point there has been no mention of statistical testing at all. First explain what test you are using to determine whether X is significantly different from 0, and state clearly what physical quantity X represents, then explain the effective sample size. The effective sample size is said to "replace the t-statistic sample size" (5/27), but there is no mention of t-tests having been conducted; the only test specifically mentioned is the Mann-Kendall test (Table 1).

Model evaluation

I find parts of the discussion of model validation against SOM-FFN confusing. On the one hand, it is the best observational benchmark available, on the other hand, discrepancies are explained away as resulting from errors in the observational data product (6/17-21). I can't make sense of "CO2 fluxes in SOM-FFN are being informed by remote biogeochemical provinces more often than other regions of the ocean". I can guess at what is being stated here, but without a more specific explanation of what sort of bias it imparts to the data product, I don't think it helps to achieve the task at hand, which is to evaluate how physically realistic the model solution is. Describing a model as 'biased' without specifying the nature or sign of the bias (e.g., 6/15, 6/21) is not very useful. The beginning of 3.2 is misleading (CalCS shows very good results, HumCS

much less so) and poorly worded. How about "CESM-LENS simulates the pCO2 seasonal cycle well for the Pacific EBUS, with larger error in the Atlantic regions"? (and change "Beginning with the CalCS" to simply "in the CalCS"). Again, this paragraph mixes up model evaluation, analyses of the model solution that do not have any analogue in the observations, and literature review. Again, I think this whole section should be included in the Results, and a clear separation of Results and Discussion attempted.

Terminology

"internal" variability in a model simulation is an analogue of "natural" climate variability in the real world. I would prefer that the latter term be used except when the reference is specifically to climate model simulations. I find terms like "have some of the highest internally driven CO2 fluxes globally" very awkward. How about "have some of the highest unforced variability of CO2 flux of any part of the world ocean"?

I think "values" is one of the most overused and abused words in scientific writing. Search out all occurrences and if possible replace with something specific. For example, on 10/8 one could replace "r-values" with "correlation coefficients" (see also 9/13-14, 11/22) and on 13/19 "mean values" could be "mean uptake". On 8/11, one can't even tell what physical quantity is being presented (it is the internal variability component of the standard deviation of the CO2 flux, but the reader has to go to the table caption to find this out).

There are many locations where "air-sea" could be added before "CO2 flux" in the interest of clarity (e.g., Figure 10 caption).

Some details

1/21 "Upwelling delivers deep waters with respired nutrients to the surface, fueling primary production and ultimately supporting fisheries that are highly productive with respect to the small surface area they cover" Upwelling delivers waters rich in nutrients to the surface, fueling primary production and ultimately supporting fisheries that are highly productive relative to the small surface area that they cover

2/5 "contributing to the magnitude and determining the direction of air-sea CO2 fluxes" determining the sign and magnitude of the air-sea CO2 flux

2/11 "more efficient biology" greater biological production

2/21 delete "fractional"

3/5 Is it accurate to refer to the NAO as a "decadal-scale oscillation"? I thought it was more like a white noise process with a very flat frequency spectrum.

4/20-21 "In contrast ... CO2 fluxes." I would delete this entire sentence.

7/13 "These dual peaks are driven by an interchanging importance between thermal and non-thermal effects." These two peaks are driven by an alternating dominance of thermal and non-thermal effects. (see also 12/15 and 12/18)

7/22 "the BenCS pCO2 seasonal cycle nearly 180 degrees out of phase" This actually true of CanCS as well, although the amplitude is significantly underestimated in the model.

9/23 change "During a positive NPGO event" to "During the positive NPGO phase"? (and delete "of the system")

9/24 add "transport" after "DIC"

9/27 "Because the system-wide contributions of SST and sDIC to the anomalous flux nearly balance each other, minor contributions from wind, salinity, sAlk, and freshwater flux push the system in favor of anomalous uptake" I think this is an overinterpretation. It looks to me like the SST contribution is larger than the sDIC, and even if the 4 smaller terms cancelled each other out the net would still be negative.

10/27 change "influencer" to "influence"

11/4 change "are in opposition to one another" to "are of opposite sign"

11/14 change "advected warm waters from the equatorial Pacific" to "warm water advected from the equatorial Pacific"

11/17 change "intensification of wind magnitude" to "increase in wind speed"

11/23 "This encircles the climatological position of the Azores High, the atmospheric subtropical gyre which forces the CanCS." I have never heard the Azores High referred to as an "atmospheric subtropical gyre", although it is a large-scale anticyclone. But I have never heard this terminology before, and it's generally bad practice to take existing terms and assign them new meanings without a compelling reason. I also don't think "encircles" is a good choice of words. How about "represents" (or "indicates" or "coincides with")?

11/32 "the linear Taylor approximation aligns exactly with the direct regression" I can't tell what this means.

11/33 "The NAO describes modifications to the intensity of atmospheric gyre circulation between the Azores High and Icelandic Low" The NAO represents fluctuations in the intensity of atmospheric circulation between the Azores High and Icelandic Low

12/21 change "when sDIC and SST are of equal magnitude" to "when the sDIC and SST associated terms are of about equal magnitude"

12/27-30 "The major EBUS ... variability in CO2 fluxes." another truly awful sentence: rewrite or delete

13/13 change "diffusion" to "mixing"

13/7 delete "to" before "roughly"

13/25 I think this statement requires a data or literature reference.

13/34 "the relative contributions of variables to anomalous CO2 fluxes" the relative contributions of different physical processes to anomalous CO2 fluxes

14/4 "While not observed in our historical modeling study, modifications to modes of climate variability associated with the major EBUS could directly influence the magnitude of internally generated anomalies in CO2 fluxes in the future." I don't see how we know this. Such trends might exist in the ensemble data even if no one has yet attempted to detect them.

14/9 "we only present the leading mode of climate variability" Similarly, this may be true but I don't think it is demonstrated by the data shown in this paper. The authors simply focus, in each region, on what they EXPECT to be the most important mode; they don't actually test whether this is true.

14/10-12 "we explain", "we were able to explain" not an appropriate use of first person (I suggest that the wording of all discussions of explained variance in this paragraph be reviewed.)

14/19 change "a coarse single model ensemble" to "a single coarse-resolution model"

14/24 "do not directly resolve the coastal upwelling process which induces vigorous outgassing within the first O(10km) of the coastline" do not resolve the coastal upwelling that induces vigorous outgassing within the first $\sim$10 km of the coast

14/28-29 I agree with this sentiment, but you have to get the boundary conditions for the downscaling model from global models. So if those models have huge biases in the positions of major transition zones, it's not clear that having high resolution within a regional domain is going to do any good. This is a problem in the northwest Atlantic as well, as coarse resolution models have large and persistent biases in the location of the Gulf Stream separation from the coast.

Figure 5 contains a great deal of information, and the caption could be a bit clearer. Violin plots may not be familiar to some readers, and exactly what is shown in the right hand panels could be spelled out. Similarly, the caption to Table 2 could contain a great deal more detail.

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