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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.

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Reviewer #2:

Summary:
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

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have.

We would like to thank referee #2 for their careful review of this paper. Their suggestions substantially improved this manuscript. ***Please see the supplemental pdf to this response for a tracked changes version of the revised manuscript.***

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.

Thank you for your suggestion. We have changed the final section to be called "Discussion and Conclusions." We have also incorporated Section 3 into the Results.

We have split the first paragraph of Section 5 into three smaller paragraphs covering separate topics (i.e., the seasonal cycle discussion, the general CO_2 flux response to modes of climate variability, and the more specific CO_2 flux response to climate variability.)

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?

Thank you for your suggestions. We have removed 3/15–20 and replaced it with a much briefer description of high-resolution simulations:

> *Regional hindcast simulations are beneficial for their higher spatial resolution and more accurate representation of a specific EBUS's dynamics, but they are limited to the analysis of a single EBUS, preventing a synchronous view across EBUS with a consistent modeling tool.*

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).

Thank you for your comments. We have edited 2/18–20 to read more cleanly:

> *So far, relatively few studies have truly assessed the longer-term variability of the air-sea CO_2 fluxes in EBUS, regardless of whether these variations are internal or forced.*

Indeed, very few EBC studies have focused directly on the link between climate variability and air-sea CO_2 fluxes (Chavez et al., 1999; Friederich et al., 2002; Torres et al., 2003; Feely et al., 2006; Takahashi et al., 2003), in comparison to the response of physics and biology to climate variability (e.g., Chenillat et al., 2012; Chhak and

Di Lorenzo, 2007; Di Lorenzo et al., 2008, 2009; Mantua et al., 1997; Cropper et al., 2014; Shannon et al., 1986; Reason et al., 2006; Hutchings et al., 2009; Chelton et al., 1982; Barber and Chavez, 1983; Barber and Chávez, 1986; Lynn and Bograd, 2002; Escribano et al., 2004; Frischknecht et al., 2015, 2017; Borges et al., 2003). We feel that mentioning this in 2/18 and 3/8 is useful to motivate the need for a study investigating air-sea CO_2 fluxes directly.

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).

Thank you for your suggestions. We excised 7/10–11 and 7/27–29 from the manuscript. We removed 6/11–13 and adapted 14/24–25 to read:

> *Due to model resolution, we do not resolve the coastal upwelling that induces vigorous outgassing within the first ∼50km of the coastline, such as in high resolution model solutions by Turi et al. (2014) and Fiechter et al. (2014).*

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.)

Thank you for your comments. Note that the boxes/boundaries used for statistical analysis are only showcased in Figure 1. Those shown in Figure 3 are just general boundaries (which span the full subplots of Figure 1) to point out that these regions exhibit significant unforced variability in CO_2 fluxes. The regions in Figures 6–9 are identical to those in Figure 1, but are just zoomed in a bit more on the region. We also point out in the caption of Figure 1 that these boundaries follow the model grid, i.e., that they are confined to 800km offshore zonally along the coarse grid.

We updated figure captions to clarify these points. To Figure 1, we added a note that the statistical boxes are confined to 800km "in the E–W direction." To Figure 3, we added "Here, the black boxes outline the general domain of the EBUS in this study but do not coincide with the statistical boundaries shown in Figure 1." Lastly, we renamed Section 2.2 to "Upwelling Regions and Anomalies" to account for the description of anomaly generation in the second half of that section.

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 mËĘ-2 yËĘ-1 KËĘ-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 mËĘ-2 yrËĘ-1" seems incomplete, because the reader does not know how large an anomaly in the NPGO is required to give rise to X mol mËĘ-2 yrËĘ-1 of CO2 flux

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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.

Thank you for this suggestion. We updated all cases where regression results were mentioned to account for the definition of the given mode of climate variability (K^{-1} for Nino3, σ^{-1} for NAO, NPGO, PDO). We have removed 12/10 per your suggestion.

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), bu there is no mention of t-tests having been conducted; the only test specifically mentioned is the Mann-Kendall test (Table 1).

Thank you for your suggestion. We added the following description of our correlation/t-test methodology immediately following 5/26:

> *We use a Pearson product-moment correlation for all linear correlations performed in this study (e.g., between area-weighted CO_2 flux anomalies and climate indices for each EBUS). Our null hypothesis is that the two time series being compared are uncorrelated, following the Student's t-distribution with a significance level of $\alpha = 0.05$.*

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We also further clarify the description of autocorrelation for the reader, replacing 5/27–28 with (see also response to Reviewer 1):

> *Autocorrelation is prevalent in climate indices such as the NPGO and ENSO (Di Lorenzo and Ohman, 2013), and our annual smoothing further enhances autocorrelation in CalCS and CanCS air-sea CO_2 fluxes (see Sections 3.3.1 and 3.3.3). To compensate for this autocorrelation, we replace the t-statistic sample size N with an effective sample size N_{eff}, which quantifies the number of statistically independent measurements: . . .*

Lastly, we add a description of the Mann-Kendall test following 6/2:

> *We use a one-sided Mann-Kendall test to assess significance in trends (e.g., the long-term diffusion of anthropogenic CO_2 into EBUS). Our null hypothesis is that the trend is not significantly different from zero, with* $\alpha = 0.05$.

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.

We removed 6/19–21 ("CO_2 fluxes in SOM-FFN are being informed . . .) to avoid confusion for the reader.

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.

Thank you for your suggestion. 6/15 was changed to:

> *The CO_2 flux climatology in the Atlantic systems is more biased in the CESM-LENS, with a tendency for spurious or stronger outgassing than is suggested by the observational product.*

We feel that the statement immediately following 6/21 describes the nature of the bias, and it would be redundant to mention the outgassing bias in 6/21 directly. We changed

the beginning of 3.2 per your suggestions:

> *CESM-LENS simulates the pCO_2 seasonal cycle well for the Pacific EBUS, with larger error in the Atlantic regions. In the CalCS, . . .*

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"?

Thank you for bringing this important point up. Our use of "internal" over "natural" was intentional in this manuscript. "Natural" variability encompasses both the internal/unforced contribution as well as natural external forcing from volcanic eruptions and the solar cycle. Although we agree that "unforced" is an appropriate alternative to "internal," the authors decided it was best to use "internal" for this study.

We have updated the abstract (1/7) to add (unforced) following internal. We have also updated the suggested sentence to "... highest unforced variability ..." for clarity.

Further, we have added a very careful description of "internal" and external (both natural and anthropogenic) forcing to the introduction, following 2/18:

> *Fundamentally, one can differentiate between variability arising from the processes that are purely internal to the climate system, and those that*

represent "external forcings", i.e., processes that impact the climate system from outside. The latter external processes can be further separated into natural and anthropogenic. The former includes variations induced by e.g., volcanic eruptions or changes in solar activity, while the latter includes changes in the concentration of greenhouse gases and other radiatively active constituents, or human-made changes in albedo. The internal variability can arise from within a subsystem itself (e.g., baroclinic instabilities leading to the formation of mesoscale eddies), or from the unforced interaction between components of the climate system.

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).

Thank you for this suggestion. We replaced nearly every occurrence of "value(s)" with something more specific. We updated both the main text as well as figure labels and captions in Figs. 5–10.

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

Thank you for this suggestion. We have added "air-sea" as a prefix to "CO_2 flux" in the Figure 10 and Table 1, 2 captions as well as in a few places throughout the text (2/18, 3/16, 3/34, 5/12, Sections 3.1 and 4.2, 8/3)

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

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you. We have updated the manuscript to reflect your suggestion.

2/11 "more efficient biology" greater biological production

Thank you. We have updated the manuscript to reflect your suggestion.

2/21 delete "fractional"

Thank you. We have updated the manuscript to reflect your suggestion.

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.

Thank you for this clarification. We use the language "decadal-scale" based on section 2 of Hutchings et al. (2009). Note also that the original NAO paper (Hurrell, 1995) suggests that the NAO is a source of low-frequency variability that imparts "large decadal climate variations over the North Atlantic."

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

Thank you. We have removed this sentence from the manuscript per your suggestion.

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)

Thank you for your suggestion. We have edited 7/13 and 12/15 to reflect these changes.

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.

Thank you for catching this. 7/19 has been modified to read "However, CESM-LENS simulates a damped seasonal cycle that is approximately 180 degrees out of phase for pCO_2 in the CanCS ..."

9/23 change "During a positive NPGO event" to "During the positive NPGO phase"?

(and delete "of the system")

Thank you. We have updated the manuscript to reflect your suggestion.

9/24 add "transport" after "DIC"

Thank you. We have updated the manuscript to reflect your suggestion.

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.

Thank you for this comment. Note in Table 2 that the SST contribution is only slightly larger than the sDIC contribution (-0.12 mol m^{-2} yr^{-1} vs. 0.11 mol m^{-2} yr^{-1}). Accounting for ensemble spread, some individual ensemble members have sDIC contributions that slightly outweigh SST, so the minor terms are an important contribution toward causing uptake anomalies.

10/27 change "influencer" to "influence"

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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")?

Thank you for your suggestion. We agree that it is best to avoid assigning new labels. We've updated 11/23 to read "large-scale anticyclone" and have changed out "encircles" to "coincides with." We have also changed reference to "subtropical gyre" with "large-scale anticyclone" where appropriate (12/23)

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

Thank you for bringing this up. We have excised this statement to avoid confusion for the reader.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you for your suggestion. We've excised this sentence as well as the sentences that follow it (12/27–33)

13/13 change "diffusion" to "mixing"

Thank you. We have updated the manuscript to reflect your suggestion.

13/7 delete "to" before "roughly"

Thank you. We have updated the manuscript to reflect your suggestion.

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

Thank you for this suggestion. For clarification, our comments on changes to the CO_2 flux seasonal cycle were based on our analysis of the CESM-LENS. We have added statements to this line clarifying that these are results from CESM-LENS to avoid confusion for the reader.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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.

Thank you for your suggestion. We edited the text to be more clear, following 14/1–4 with: "These modifications to modes of climate variability suggested by the literature could directly impact the response of EBUS CO_2 flux anomalies to internal variability, thus affecting the conclusions of our study."

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.

Thank you for noticing this. We modified the manuscript to read: "We present the mode of climate variability that has the largest influence on CO_2 flux ..."

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.)

Thank you for your suggestion. We updated the manuscript to read "we account for" in both cases.

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

Thank you. We have updated the manuscript to reflect your suggestion.

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.

Thank you. We have updated the manuscript to reflect your suggestion. Note that we used $\sim$50km which is a more accurate depiction of the length scale of outgassing if

one is not to use O().

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 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.

Thank you for your comments. We agree that one requires boundary conditions from global models to downscale to higher resolution. However, Machu et al. (2015) show significant improvement in the physics and biogeochemistry of the Benguela Current through dynamical downscaling (i.e., even when inheriting biases through the coarse boundary conditions). Further, other techniques can help to ensure that the high resolution regional model has less bias than the coarse global model. For instance, Small et al. (2015) reduce the Benguela warm bias by also increasing atmospheric resolution and adjusting alongshore wind stress curl to be more realistic for the system. (Manuscript unchanged in response to comment)

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.

Thank you for your suggestion. The following has been added to the end of Figure 5's caption:

The interior of the violin plot displays a box plot with the ensemble mean denoted as a white dot. Shading around the box plot reflects the ensemble distribution of correlations, which are mirrored on either side.

The caption for Table 2 was updated to:

Estimated contributions of individual terms to air-sea CO_2 flux anomalies, ΔF, in response to a mode of climate variability using Equation 4. Each row under the Individual Terms header depicts the ensemble mean contribution and ensemble spread for Figures 6–9. The column "CalCS–PDOn" reflects results from the nearshore box in the CalCS in Figure 7, and "CalCS–PDOo" the offshore box. Σ is the sum of all contributing first-order terms (i.e., all rows under Individual Terms or the right hand side of Equation 4). ΔF is the direct regression of CO_2 flux anomalies onto the specified mode of climate variability (i.e., the left hand side of Equation 4).

We also included units in Table 2 and Table 3 to reflect that these are responses to 1 unit of the given mode of climate variability (e.g., mol m^{-2} yr^{-1} σ^{-1})

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