Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-363-RC2, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Interannual drivers of the seasonal cycle of CO_2 fluxes in the Southern Ocean" *by* Luke Gregor et al.

Anonymous Referee #2

Received and published: 6 November 2017

The authors present here a lengthy paper in which they aim to provide evidence that the included machine learning products for pCO2 are an appropriate proxy for investigating interannual variability in pCO2 and carbon fluxes in the Southern Ocean. They use these products to analyze transitions between regimes and also investigate the drivers of the changes. However, the paper needs to be proofread prior to submitting as it has errors throughout which distracts from the science. The job of a review is not to be a technical reviewer but to analyze the science. Unfortunately it is difficult to follow the science with the errors included throughout the paper.

The authors utilize an ensemble of five products for this study, however 2 of the products are just a repetition of two other products produced at a higher resolution. There is no evidence given that this results in 5 independent products for

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this ensemble. At the very least, more discussion is required for it to be accepted that the high and low resolution versions of the same product can be seen as individual ensemble members. Additionally, the analysis should be revised to include the SOM-FFN created with SOCATv3 so that all members are produced using the same dataset. The SOM-FFN product is available based on SOCATv4 now and the authors should at least update it to the version based on SOCATv3 (see https://www.nodc.noaa.gov/ocads/oceans/SPCO2_1982_2015_ETH_SOM_FFN.html).

This paper is wrought with inconsistencies, incomplete definitions, and missed words which distracts the reader throughout. Some examples include the use of MIZ in text but AZ in figures, along with a consistent use of acronyms without first defining them (PFZ, AZ, MIZ, etc). Additionally, using the Fay & McKinley biome boundaries but then referring to the regions as SAZ, PFZ, AZ/MIZ is confusing and inaccurate. The biomes and frontal regions are not interchangeable and the authors need to be consistent throughout the study as to which they are using.

In Figure 2, the different extents into the ice covered regions of each product will affect the comparison shown in 2c. Ensure that equal regions are being compared. Also, showing how the products compare to the available data in Figure 2 would be helpful. Figure 3b is not discussed in the text at all. The definition of the “signal” as the largest difference in trend for a particular gridcell should be referenced as typically it is the mean trend/value that is the “signal” and the noise is the spread around that signal (either standard deviation, standard error, etc). Figures 5 and 6 need to be improved dramatically. The difference between the dark and light curves in Figure 5 is not defined. Additionally, there is no indication of which product (or average of the ensemble) is being plotted here. The captions are wrong (referenced subplots j-r which do not exist). The background gray shading to designate the difference regimes/timescales is difficult to distinguish. Perhaps another method to highlight those would be helpful. Additionally, trends included on these figures should have confidence intervals or uncertainty values included. Figures 7-8 continue with captions that do not correspond to

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the figure (“wind stress (d-e), SST (f-h)” when wind stress is in (d-f) and SST in (g-i), etc.

Below are some obvious technical corrections which I have not already addressed above. I reiterate though that the entire paper needs to be revised and improved, as it is difficult to follow the science with incorrect references to figures and missing words in sentences.

Line 43: “not due to changes in overturning” Line 46: “The led to an oceanic dipole. . .” Line 62: repeated word Line 141: PFZ and MIZ not defined. MIZ and AZ used interchangeably throughout paper Line 149: missing a word in “the variability of between the ensemble members. . .” Line 157: how are “scores” calculated? Line 199: Should reference Figure 3c I believe Line 213: Figures 2a-c and 4a-c are not the correct reference figures for the points being addressed. Line 221: “CO2” Line 255-260: Figure 5 caption is inaccurate to what is shown in Figure 5 Line 289: Should reference Figure 5a,d rather than Figure 5 b,e. Line 291: remain consistent with capitalizing Southern Ocean Line 307: Do you mean the seasonal cycle amplitude? Line 342: “it was advanced that the explanation. . .” is awkward Line 350: “These studies have in linked the wind stress variability. . .” is awkward and needs to be rewritten. Lines 373-376: caption for Figure 7 and Figure 8 need to be corrected to accurately reference the figures. Line 387-388: “. . .and surrounds (Figure 7d,j)” is awkward. Consider revising sentence Line 397: “(-ve shift)”. Is this supposed to be negative shift? If so, simply spell out to improve clarity. Line 439: “ENSE” perhaps should be ENSO?

Overall, the science presented in the paper is interesting and could provide a interested look at using various machine learning methods to gain an understanding of the Southern Ocean carbon drivers. However, the lack of proofreading prior to submitting is clear and must be improved before a complete review of the manuscript can be undertaken.

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