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

Anonymous Referee #1

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Gregor and co-authors investigate the variability of the Southern Ocean CO2 uptake strength from 1998-2014 analyzed for 9 regions (SO divided in basins and Fay and McKinley biomes). The authors combine 5 realizations to form a multi-model mean which is used to investigate seasonality and year-to-year variability of the delta pCO2 and the CO2 flux. 3 of the realizations are independent whereas 2 are simply higher resolution versions of 2 other methods. The authors find that the seasonal variability is the strongest mode of variability in the SO. Additionally, the authors confirm results from past studies that the SO was losing some of its uptake capacity in the early years of their analysis period, then the uptake increased in the subsequent period, whereas in the final years of their analysis period the reinvigoration of the sink stopped again. The authors investigate the cause of this variability in the sink strength by analyzing

anomaly periods for winter and summer season separately. From this analysis the authors conclude that the drivers are seasonally decoupled with wind being the dominant driver for winter variability and biology being the dominant driver for summer variability.

I found this study to be interesting, comprehensive and in general suitable for the journal. The authors do not only present results from new methods to confirm previous results, they also deepen the analysis by looking at anomalies rather than trends (as previously done) and investigate different seasons.

I do however have some major issues with the presented manuscript:

The study confuses trends and variability. When do the authors talk about trends and when about variability? At the moment, these two terms are mixed up. E.g. take figure 5 all panels. There is clearly some year-to-year variability causing in some years more and in some less uptake but overarching in all panels of figure 5 and 6 one can clearly see an increasing CO2 sink from 1998 onwards, i.e. an increasing trend throughout the entire time period. Furthermore, wording used like "decadal trends" and "interannual trends" contribute to the confusion. What is a decadal trend? Is it the slope of a regression line when considering at least 10 years of data? What are interannual trends? The same only considering 3 years? In the latter case one cannot speak of a trend at all.

Despite being able to do so, the authors do not add uncertainties. There are many sources of uncertainty ignored by the authors, e.g. the measurement uncertainty (which is however neglectably small), the extrapolation error of the method, the building of the multi-model mean creates an error and finally the calculation of the air-sea flux adds another source of uncertainty (through wind and transfer velocity choice). At the moment, the results are presented overconfidently. It is not clear how much of the explored variability is significant and how much is simply statistical jibberish. I am aware that there is no "perfect" way to represent all uncertainties, but in a data sparse region like the Southern Ocean a study like this needs to add the best possible uncertainty estimate, otherwise, many of the conclusions drawn cannot simply be accepted.

On page 11 line 312 I found that the authors claim statistical significance between the mean uptakes, reporting a p-value. It is not clear to the reader what test was used and how significance has been determined. Also, when adding uncertainty, the authors will notice that an uptake of -0.17 PgC/yr is unlikely to be identified as statistically significantly different from -0.19 PgC/yr in the data sparse Southern Ocean.

Despite the uncertainty of the CO2 itself there are other sources of concern related to uncertainty. Chlorophyll is e.g. also presented without discussing uncertainty. How is cloud coverage and missing chlorophyll data effecting the results? Also, wind products have been shown to have different trends locally in the Southern Ocean. This has not been mentioned.

I am also wondering to what extend the use of different products hampers the conclusions of the manuscript. SST from Reynolds is based – to the extent of my knowledge – from satellite and in-situ data, whereas ECCO MLD is from an assimilation model. I would expect some disagreement between these products that have nothing to do with "real world" disagreement. This is not a massive concern, but certainly needs to be mentioned as well.

Another source of concern is the length choice of periods P1-P4. Periods P1-P3 are all of the same length, whereas P4 is substantially shorter. The authors claim that substantial year-to-year variability occurs on various timescales (4-6 year and 10 year modes respectively), hence the variation of periods aliases this analysis.

All the above points raised are of major concern and must be addressed before the manuscript can be considered for publication.

Minor comments:

Title: the title only mentiones CO2 fluxes whereas in the manuscript largely discusses delta pCO2

Line 29: “paucity of observations (Landschutzer et al 2015)” – This is not a good reference. Bakker et al 2016 (the SOCATv2 reference) would be a better reference for such a paucity.

Line 62: remove 2nd that

Line 95: wrong reference (Bakker et al 2016)

Table 1: How has the RMSE for the 0.25x0.25 degree product been calculated? SOCAT offers a gridded product but on 1x1 degrees. Did the authors grid the 0.25 product themselfes? If yes, has this been done the “SOCAT” way, i.e per cruise weighted or differently? Same with the 16-day timestep. More information is needed here.

Line 130: How different would the results be if a different transfer velocity was chosen? This may have significant effects on the uncertainty.

Line 134: where are [ice] data from? I am missing a reference here.

Line 175-176: “attributed this difference to the clustering step used by the SOM-FFN that created large discrepancies in the Atlantic sector.” I have not found any convincing evidence for this in any of the cited papers.

Line 236: Either the authors used the wrong wording or there is a misunderstanding, but I do not see where the authors find that the delta pCO2 is zonally asymmetric within each biome. It looks like the other way around: Figure 4 looks like there is a strong zonal symmetry (besides indeed in panel a).

Lines 249-254: This paragraph is not clear. Please rephrase

Line 273: interannual trend – what is that? Interannual variability. A 3-year trend? A trend that changes sign every year?

Lines 275: decadal mode: How are you able to say this. You have 1998-2014 data , i.e. 17 years of data. How can you detect a decadal mode from such a short timeseries?

Line 305: “decadal trend” – what is this? A trend of at least 1 decade?

Lines 311-312: “-0.19 and -0.17 PgC yr-1 for the Atlantic and Pacific sectors respectively (where the latter are significantly different with $p = 0.01$).” how was this calculated, and given the many, many uncertainties that go into such a flux number one cannot possibly believe that this these numbers are indeed statistically significant.

Line 340: What about acidification induced changes linked to changes in the buffer capacity (see e.g. Hauck et al 2015)? A contribution within 17 years is plausible.

Line 439: I suppose ENSE is ENSO – but please either way spell out abbreviations when first using them

Line 505: “The fact that Chl-a is the dominant driver of interannual ΔpCO_2 variability should not be surprising” – the authors have not proven that chlorophyll is the dominant driver. From a pool of selected variables, it showed the largest correlation – this can barely be called a “fact” in science.

Figures 5 and 6: Uncertainties need to be added. Without uncertainty I do not trust that the observed variability is significant.

Figures 7 and 8: Wind stress anomalies are interesting, but direction would be equally interesting and provide more evidence.

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