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Interactive comment on "Mechanisms of the Sea–Air CO_2 Flux Seasonal Cycle biases in CMIP5 Earth Systems Models in the Southern Ocean" *by* N. Precious Mongwe

Anonymous Referee #1

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The Manuscript "Mechanisms of the Sea-Air CO2 Flux Seasonal Cycle biases in CMIP5 Earth Systems Models in the Southern Ocean" by Precious et al provides an empirical partitioning of flux between correlations with sea surface temperature change (presumably due to solubility) and influences DIC concentration including biological, wind, circulation terms in determining air-sea fluxes in the Southern Ocean from CMIP5 models. The authors find that the CMIP5 generation carbon cycle models can be classified into two groups – one whose phasing of the seasonal cycle agrees with an observationally constrained estimate but is overly strong, and another that has the opposite phasing. The manuscript is thus helpful in this empirical anecdote in spurring further assessment of the mechanistic commonalities and differences between there models.

The manuscript is also valuable in contrasting the representation of Antarctic and Subantactic zones in these models, and there relative zonal symmetry between the three basins compared to the observationally constrained analysis.

Unfortunately, I find the title beginning "Mechanisms..." to be a disappointing overreach as rather than including quantification of the solubility, transport, and biological pump mechanisms, the authors rely entirely on the empirical seasonal relationship correlation of dSST/dt and cursory analysis of mixed layer entrainment as metrics of model mechanisms. Throughout the manuscript, the authors assert that their SST correlation demonstrates that the seasonal cycle "is determined by the role of temperature". Correlation isn't causation... SST correlates with many other things – including mixed layer depth and productivity, the mechanistic significance of correlations with SST is complex. For example, the authors could calculate the change in equilibrium DIC under temperature change and convert their dSST/dt metric into a dDIC_solubility/dt metric to quantitatively assess the role of solubility. Of course, this would involve the assumption of infinite wind and neglect the year-timescale of DIC equilibration, and including the role of wind in modulating the timing of CO2 flux would complicate the authors simplistic interpretation. Beyond that, interior budgets could be constructed, leading the authors to developing a simple box model of mixed layer DIC to be able to reproduce the various GCM results through the combination of gas exchange, thermal, transport, and biological mechanisms. While such a more mechanistically based box model analysis could prove very valuable in uncovering the mechanistic differences between the models, it is probably outside the scope of the present manuscript.

The current reliance on qualitative correlation leaves the analysis with a modest utility in describing basic model results as a repackaging of previously published analyses, but of marginal utility in providing novel insight into the causes of model CO2 flux seasonality and implications for future Southern Ocean carbon uptake. At a minimum, I suggest the authors quantify the role of SST change on DIC solubility to be able to confidently assess whether the role of temperature in the temperature correlated group can be attributed to solubility, Beyond that, the authors should include some quantification of model biases relative to observational uncertainty, and include several of the available observationally constrained products to assess that uncertainty.

Specific comments

14 – “has” should be “may” here as this is an assertion/hypothesis, not a conclusion

16 – Why “specialized” suggest removing or replacing with a more specific word.

24 – I think “with a dominance of DIC regulation” should be “with a dominance of other factors driving DIC regulation”

28 - “resolve” which way, higher or lower?

37-38 – What latitude criterion is used for this “third” estimate?

43-44 – “Evolution of the Southern Ocean CO2 sink is expected to change. . .” is confusing as evolution is already a change, and doesn’t specify how – suggest something like “The Southern Ocean CO2 sink is expected to diminish under anthropogenic warming”

66 – Remove “from September,”

82 - Two very relevant additional manuscripts discussion these term balances include:

Nevison, C.D., Manizza, M., Keeling, R.F., Stephens, B.B., Bent, J.D., Dunne, J., Ilyina, T., Long, M., Resplandy, L., Tjiputra, J. and Yukimoto, S., 2016. Evaluating CMIP5 ocean biogeochemistry and Southern Ocean carbon uptake using atmospheric potential oxygen: PresentâĂŘday performance and future projection. Geophysical Research Letters, 43(5), pp.2077-2085.

Jiang, C., Gille, S.T., Sprintall, J. and Sweeney, C., 2014. Drake Passage Oceanic p CO2: Evaluating CMIP5 Coupled Carbon–Climate Models Using in situ Observations. Journal of Climate, 27(1), pp.76-100.

135 – The assertion that “The ocean-atmosphere CO2 gradient is known to be the main driver of FCO2 variability” Is true in a regional sense but is certainly not try in a temporal sense in most regions where wind variability can dominate like in the equatorial Pacific:

http://onlinelibrary.wiley.com/doi/10.1029/2005JC003129/full

The delta pCO2 argument was that is you average over large enough scales, the mixed layer equilibration time of CO2 was short enough (about a year) that CO2 fluxes were determined by the net balance of biolgy and thermal factors rather than the wind. On a seasonal scale, ignoring the role of wind seems like a fatal flaw. Rather, the authors should argue that the wind variability in this region is small before disregarding it. This is likely true in the Southern Ocean where winds are strong in all seasons.

181 – While I am glad the authors are considering mixed layer entrainment, it seems remiss here to ignore the biological and other circulation terms such as upwelling and consider them all lumped together as “DIC” terms.

184 – A brief description of the Orsi definition should be provided here.

249 – The statement that the models “do not capture any of the basin-specific features” is a fairly strong, but non-quantitative statement. This should be much more specific – like, the observational reanalysis shows a stronger flux in the Atlantic than the Indian and Pacific while the models show similar fluxes in each basin.

255, 267, 269 –in all three cases, I ask the same question, “How much”. It is not sufficient to anecdotally say that there are “some differences”, “an overestimation”, “large standard deviation” – all of these are relative statements that mean nothing if not quantified. The major question is if they are large relative to the uncertainty in the observations.

256 – Please quantify the time span – 2 months, 3 months?

259 – “mid-summer (Dec-Feb)” does not make sense as Dec-Feb is the entire summer. . . what is the time span?

260 – “end of autumn (March)” does not make sense as March is the beginning of Autumn.

278 – I do not understand the phrase, “the overall model biases are not consistent with the seasons”

282 –again, “less differences” and “small negative biases” should be quantified to be meaningful.

319 – “justifies our a priori separation” comes across as inappropriate self-congratulation. The salient point is that the separation quantifies the separation between the two classes of models in terms of the relative dominance of SST-Flux correlation.

370-371 – Unless the authors are going to quantitatively explore the mechanisms of SST drivers on CO2 flux including solubility, phytoplankton growth rates, etc, the statement “the seasonal cycle of FCO2 is determined by the role of temperature” should rather be “the seasonal cycle of FCO2 is correlated with temperature”

380-391 – This paragraph pertaining to the role of solubility is missing quantification of the role of solubility changes. The simplest

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