Kiel, 28th February 2017

Dear Editor,

Please consider our revised manuscript "Simulating natural carbon sequestration in the Southern Ocean: on uncertainties associated with eddy parameterizations and iron deposition" for publication in Biogeosciences.

The reviewers comments were extraordinarily constructive! They resulted in changes to the text marked in bold. We elaborate on this point-by-point below.

Thank you (and the reviewers) for your (their) time and work!

Yours sincerely,

the authors

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**Point-by-point - corrections triggered by Review #1; RC1**

**1) Table 1: In most climate model experiments where the zonal wind stress has been increased, the increased wind speed has not been applied to the heat and fresh water flux terms. I suspect this is also the case for these experiments because the air-sea heat exchange is described as relatively constant (Pg 9, l 5). This definitely needs to be clarified and stated.**

**-A:** We applied the increased winds to all bulk formulas including those for heat and freshwater. Clarified on pg.4, ln.4.

**2) Pg 8, l 30-32. A constant GM coefficient can only produce marginal eddy compensation (Fig 6a). A variable GM coefficient is required to produce significant eddy compensation, but some choices do not (Fig 6c).**

**-A:** Our impression (although we have no proof) is that a constant but high GM coefficient would already suffice to produce significant eddy compensation. As it stands, we think that the respective sentence (now pg.9, ln.4) " ... a more complex 5 definition of the thickness diffusivity (such as in E&G and FMCD) does necessarily amount to an increase of the (parameterized) eddy compensation relative to the original pragmatic choice ..." is correct (i.e. this is what we see in our admittedly coarse-resolution model configurations).

**3) Fig 7c shows different rates of decline in oceanic carbon uptake in the four different experiments performed. I think the linear slopes over years 20-70 should be calculated and compared. This will produce some change between the E&G (blue) slope and the CON and FMCD slopes that is about 20% as large as the slope change in the IRON (green) slope. Is a 20% change "rather robust" as described on pg 9 l 19? It is also unfair to the IRON simulation to say it has the wrong sign of air-sea carbon fluxes (pg 9 l 27), because if the experiment were extended another 10 years, then the sign of the IRON curve in Fig 7c would almost certainly be negative. A better comparison would be the linear slope values. Should spatial maps of the oceanic carbon uptake changes be shown?**

**-A:** Trends are now calculated (pg. 10, Tab. 2) and discussed on more quantitative grounds (pg. 9, ln. 25).

As concerns the presentation of spatial maps of the oceanic carbon uptake - we decided not to add them to the manuscript. Note however that they are now accessible to the public because we included them in our response to the reviewer (http://editor.copernicus.org/index.php/bg-2016-460-AC1.pdf?_mdl=msover_md&_jrl=11&_lcm=oc108lcm109w&_acm=get_comm_file&_ms=55564&c=118347&salt=16528897282134400271).

**4) Pg 11, l 1. A caveat of the present results is that the horizontal resolution of the ocean model is very coarse at 3 deg. Most climate models use a resolution of 1 deg or finer. At NCAR, we now rarely use our 3 deg ocean model because it just doesn't have enough resolution to represent several aspects of the ocean circulation, including the Southern Ocean. I would like to see a comparison like this using 1 deg resolution ocean models to see whether the present conclusions hold, because comparisons with 0.1 deg ocean models with biogeochemistry are still a few years away.**

**-A:** We agree that there is a caveat and we voice the respective information in the revised version of the manuscript more prominently (pg. 11, ln. 20).

As concerns the comparison with higher-resolution models: we are currently working on a 0.1 deg configuration with full biogeochemistry: c.f. http://89.27.255.63/?page_id=90 and https://www.youtube.com/channel/UCnuABRT7qWGgM6bvMzLpr6A and we hope that we can present the respective comparison soon in an additional publication.

**5) Figs 8-10. I would prefer to see observations and then the model minus observations differences, especially in the SSTs in Fig 8.**

**-A:** We show now model - minus observation in Fig. 8 (SST, pg. 23). As for the biogeochemical species (Fig. 9-10) we decided to stick to the original presentation for no other reason than that we feel that this is the more "wide-spread way" in the biogeochemical modeling community. We agree, however, that there are good reasons to go beyond "wide-spread ways"!

**Pg 12, l 2. I disagree. Figs 1, 3 and 5 clearly show that the FMCD choice has a better spatial representation of eddy kinetic energy compared to observations. It also shows a much stronger eddy compensation, which is more in line with eddy-resolving model results. I think it looks a much better choice than E&G or a constant: it really is about time to go beyond using a constant GM coefficient in global climate models.**

**-A:** O.K. We pushed too far in the appendix. In the revised version of the manuscript we will remove the sentence " ... This, in its turn, suggests that the simulated sensitivities of any of our configurations towards changes in the Southern Ocean, are equally likely" (c.f. pg.12, ln. 20).

**1) Pg 1, l 21. The changes in the Southern Hemisphere atmosphere have been driven by changes in the ozone hole as well as by greenhouse gases: Polvani et al (2011), J. Climate, 24, 795.**

**-A:** We added the respective information (and citation) on pg. 1, ln. 21.

**2) Pg 2, l 7. There is also recent evidence that the Southern Ocean carbon sink has been "reinvigorated": Landschutzer et al (2015), Science, 349, 1221.**

**-A:** We added the respective information (and citation) on pg. 2, ln. 9.

**3) Pg 5, l 10-12. There aren't observations of the Southern Ocean MOC, and Bryan et al (2014) should also be referenced here.**

**-A:** We add the respective information (and citation) on pg. 5, ln. 14.

**4) Pg 5, l 28. Coriolis.**

**-A:** O.K.

**5) Pg 7, l 2. Rationale.**

**-A:** O.K.

**6) Pg 8, l 26. Respective.**

**-A:** O.K.

**7) Pg 10, l 8. Reference Swart et al (2014), Biogeosciences, 11, 6107.**

**-A:** We added this reference to the revised version on pg. 10, ln. 21.

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**Point-by-point - corrections triggered by Review #2; RC2:**

**Pg. 2, Lines 5-7: I would cite more recent studies here and include recent analyses of observations. Up to the mid 2000s there is evidence from models (e.g., Le Quéré et al., 2007; Lovenduski et al., 2007) and observations (please cite Landschützer et al., 2015) that Southern Ocean carbon uptake may have slowed relative to the expected increase due to the increase in atmospheric CO2. More recent observational studies (please cite Landschützer et al., 2015; Munro et al., 2015; and Xue et al., 2015) suggest that the sink may have strengthened over the last decade**.

**-A:** We added the respective information/references on pg. 2, ln.9.

**Pg. 2, Line 8: I would say something more general like the "the link between variability in surface winds and Southern Ocean carbon uptake remains inconclusive"**

**-A:** Changed on pg. 2, ln. 10.

**Pg. 2, Lines 16-22: I would also mention current observational/model studies that have examined carbon uptake associated with mesoscale eddies within the Southern Ocean (please cite Song et al., 2016). This paper includes an analysis of the Drake Passage Timeseries which represents the densest dataset of pCO2 observations within the ACC. Observations are compared to results from a high-resolution (approximately 0.1 degree) simulation of the Southern Ocean region surrounding the Drake Passage. Both observations and model output indicate how a shifting balance of physical and biogeochemical processes drive air-sea carbon flux during different seasons and gives important context to the complexity of the topic presented here.**

**-A:** Song et al., 2016 is cited now on pg. 11, ln. 25.

**Figures: Fig. 7 is the most important in the paper particularly Fig. 7c. I think it would be helpful to include a Table summarizing these results with the linear rate of decrease in C uptake with uncertainty over the 50 years of increased winds. Alternatively, you could present the difference in C uptake with uncertainty between the last twenty years of spin-up and the last five or ten years of increase winds (i.e., years 46-50 or 41-50).**

**-A:** We added a table on pg. 10 and discuss it on pg. 9, ln. 25.

**Figs. 8-11 might be more appropriate in a supplemental information section if allowed so that the reader focuses on the figures most important to the overall story.**

**-A:** We want to keep the information in the appendix.

**TECHNICAL COMMENTS:**
**Pg. 2, Line 2 ...**

**-A:** We applied all your corrections in the revised version of the manuscript. Thank you for combing through so thoroughly.