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Interactive comment on "Strong sensitivity of Southern Ocean carbon uptake and nutrient cycling to wind stirring" *by* K. B. Rodgers et al.

Anonymous Referee #1

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Summary:

Rodgers et al. performed sensitivity hindcast simulations with NEMO-PISCES by including an ad hoc parametrization of wind stirring to investigate the influence of shear-induced turbulence on summer mixed layer depths, associated changes in ocean biogeochemistry, APO and air-sea CO2 fluxes, with a focus on the Southern Ocean. The authors show that mixed layer dynamics and contemporary air-sea CO2 fluxes are quite sensitive to the induced wind stirring. In particular, deeper summer mixed layers and reduced contemporary air-sea CO2 fluxes are simulated in the Southern Ocean when including wind-stirring. The authors also show that changes in wind stirring may also influence the phasing of the seasonal APO cycle in the sub-polar Southern Hemisphere, which, if large enough, may be detectable by the existing network of atmo-

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spheric monitoring stations.

Evaluation:

It is well known that Southern Ocean mixed-layer dynamics are not well represented in climate models with too shallow summer mixed layer depths. This has probably large implications for present-day and future air-sea CO2 gas exchange. However, the sensitivity of MLD and associated changes in ocean biogeochemistry to wind stirring hasn't got much attention yet. That is an important gap in our understanding. Thus, this paper provides an important and interesting contribution to the field and the paper may be of interest to a wider community. In addition, the potential of APO to study climate-driven changes in the phasing of the season cycle is very interesting. The paper is also clearly organized.

Recommendation:

I recommend acceptance of this manuscript after major revisions. I am particularly worried about the robustness of the results (see detailed comments below). Furthermore, it took me quite a while to read the paper given the length of the paper (but I leave that to the editor to decide if the paper needs a shortening).

Major comments:

1. The authors show that the differences between the experiments with and without wind stirring is of the order 0.9 Pg C yr-1, which is surprisingly large, and potentially quite interesting. However, I am a bit worried that the large difference is actually an artifact of the specific experimental setup. Please correct me if I am wrong. As far as I understand the experimental setup described in the Methods section 2.1, the authors impose the wind stirring in year 1958 and run the model till 2006. However, the authors do not allow the system to reach a new equilibrium first with the imposed wind-stirring. I assume that the initial shock to the system (e.g. initial changes in air-sea CO2 fluxes) is pretty large. Unfortunately, the authors never show the entire simulation period 1958

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to 2006. In my opinion they should run the wind stirring case first with a repeated nominal forcing (or looping several times over the period 1958 to 2006) before they compare the two runs. Another possibility would be to perform a wind-stirring control simulation where one does not increase the CO2 concentration over the period 1965-2005 to show the trends/drifts in the control simulation. Because of the experimental setup, I am also not sure what the trend differences in the air-sea CO2 flux between the different setups tell us (0.05 Pg C yr-1 decade-1 in WSTIR vs. 0.125 Pg C yr-1 decade-1 in CNTRL)? Does it indicate a drift in the WSTIR case or does it indicate that WSTIR shows a different sensitivity to increasing CO2? I think this should to be addressed in the manuscript.

2. I understand that the wind stirring parametrization is ad hoc. However, as the model simulations cover only a short time period I would encourage the authors to explore more the uncertainty range in the used ad hoc parametrization by doing additional sensitivity experiments with different vertical mixing length scales. It is not obvious to me how the authors "tuned" their mixing length scales of equation (1) to get "realistic" summer mixed layer depths. Why is there still a large bias in summer MLD (c.f. Fig. 2d)? The authors should provide much more detail why they used this particular setup in equation (1). Furthermore, why did the authors impose the wind-stirring also in winter? This has apparently large negative consequences (too deep MLS in winter time).

3. It may be helpful to include additional subpanels in Fig. 1, which show the depth profiles of TKEBD93 and S of equation (4) separately.

4. Overall, there are too many small inconsistencies in the text, which precludes me to accept the manuscript in its present form. See specific comments below.

Specific comments:

p.15035 l.11: What do you mean with 'species'. Please specify.

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p.15035 abstract: I think the authors could be much more specific here. The authors should quantify things (or at least state the sign of changes). Currently, the abstract often says 'strong sensitivity' 'large sensitivity', etc.

p. 15037: l. 15-18: Please explain in more detail what the impact of upper-ocean inertial oscillation and ocean swell and wave on shear-induced turbulence is.

p. 15037 l. 20: Please add a reference to Sallée et al (2013). Please also add a reference to a paper that explicitly shows the summer MLD biases in ocean-only models.

P. 10540 l.14: What does 'the ocean is at rest' mean? Quasi-equilibrium? Please specify.

p. 15040: l. 11-21: You may add that the atmospheric CO2 concentration has also been increased.

p. 15040: l. 21: The 'full transient' would be preindustrial to today. Please change wording.

p. 15041: equation (1): Please specify the units of latitude.

p. 15041: l. 13: Again, how was the tuning done? Please be more specific.

p. 15041: Please specify that 'z' means depth in the text.

p. 15042: l. 10: I suppose you mean interannual-to-decadal CO2 variability.

p. 15024: l. 11: typo 'from'

p. 15024: l. 18: typo 'closing bracket'

p. 15043: l. 11: include 'needed to simulate'

p. 15043. l. 15: it is suprising that NCEP reanalysis winds are used for TM3, but the DRAKKAR forcing set is used for the NEMO-PISCES model. Please add a discussion of possible inconsistencies.

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p. 15044: How do you define the MLD in the models and the observations? Please use the same definition for both (e.g. density criterion or similar).

p. 15044. l. 5-14: Please refer to the lines if you refer to the Figure panels. This would make it easier for the reader to follow the arguments. For example: on line 10: '(green line in Fig. 2d)' or on line 11: '(red line in Fig. 2c).', etc.

p. 15045: l. 5-8: You state that: "There is a period during re-stratification phase after winter where WSTIR-simulated MLDs agree well with the observed timing". What are the implications of that?

p. 15046: l. 21-25: I am bit puzzled here. You compare a difference in the mean state with a change in the trend (Le Quéré et al.). Please explain.

p. 15047: You state that uptake is generally stronger everywhere for CNTRL than for WSTIR? But figure 5d indicates the opposite (values of WSTIR-CNTRL are almost everywhere positive). I think you show the sea-air CO2 fluxes in Fig. 5 (not the air-sea CO2 fluxes). And shouldn't be the units something like mol C m-2 yr-1? I am also wondering why there are so large differences in air-sea CO2 fluxes between WSTIR and CNTRL in the Kuroshio and Gulf stream region?

p. 15048: l. 2: I am actually surprised how similar the different model setups are, at least in comparison with the CMIP5 model spread (see Fig. 15 in Anav et al. 2013). Please comment on that.

p. 15048: What about uncertainties in the observational-based product of Takahashi et al. (2009). Is it possible to include in Fig. 6A other observational-based estimates here (e.g. from Majkut et al. 2013?)

p. 15049: l7-10: I do not understand this argument. Why is the WSTIR perturbation run too short to provide insights into the uptake capacity of the Southern Ocean to anthropogenic carbon.

p. 15052: I would delete Figure panels 9c and 9d, as Figs. 9a,b already show that the main contribution to climatological APO variations stems from O2.

p.15057: The authors state that the global CO2 uptake is only 0.1 Pg C yr-1 over the period 2000 to 2006 in WSTIR, which is very likely inconsistent with observational-based estimates. Please give some examples what processes you think have been tuned in the ocean biogeochemical models?

p. 15066: typo in Figure header 'exteded'

p. 15068: show the entire simulation covering the period 1958 to 2006.

p. 15070: Use the same labels for the x axis in all subpanels.

p. 15073: Use the same color for WSTIR and CNTRL as in figures before.

References:

Anav, A., et al., Evaluating the Land and Ocean Components of the Global Carbon Cycle in the CMIP5 Earth System Models. J. Climate, 26, 6801–6843 (2013).

Sallée, J.-B. et al., Assessment of Southern Ocean mixed-layer depths in CMIP5 models: Historical bias and forcing response. J. Geophys. Res. Ocean, 118, 1845-1862 (2013).

Interactive comment on Biogeosciences Discuss., 10, 15033, 2013.