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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.

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We want to thank the reviewers for their constructive comments and questions pertaining to the manuscript. We appreciate the extensive time and effort both reviewers took for their reviews, and we believe that addressing their comments and questions can serve to strengthen the manuscript. We would very much like to resubmit a version of the manuscript that incorporates these suggestions, and detailed below are the ways in which we plan to respond to their comments. Before proceeding to the more general suite of comments of the reviewers, we wish to first comment on two general issues that were addressed by both reviewers. Both of these pertain to experimental design underlying wind stirring sensitivity experiments.

First, Anonymous Referee #1 suggested that we might consider conducting additional

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model sensitivity studies to complement the analysis in the submitted manuscript. In particular, the concern was expressed that for the sensitivity study with WSTIR and CNTRL, the WSTIR case still exhibits biases when evaluated against ARGO-derived MLDs over the Southern Ocean. This is an important point, but it is important to be clear here that forward ocean models such as NEMO (or MOM-derived models) were not designed to be used as ocean state estimates, but rather are best considered as tools for evaluating ocean/climate sensitivities to process-based perturbations. It is important to remember the CNTRL simulation has been tuned to be close to the present day ocean state for both physical state and biogeochemical variables. Thus, tuning this global 2-degree version of NEMO (ORCA2) to have summer and winter MLDs that better match observations would undoubtedly require an iterative set of experiments where a number of other parameterizations are tuned (including the Gent-McWilliams eddy parameterization). Moreover, the tuning of the circulation and more generally the physical state variables does not in itself guarantee an improvement of the model to changes in model parameter settings. The full suite of reanalysis atmospheric fields used to force ocean circulation models are known to themselves suffer from a number of large biases and uncertainties, and these could certainly be part of the bias exhibited by the model with winter MLDs being too large. The critical point here is that changing the representation of vertical mixing in the model caused important changes in the biogeochemical state from the equilibrium state, demonstrating that our understanding of the dynamical controls on ocean biogeochemistry remains limited.

A number of community efforts are underway to provide improve state estimates using data assimilation (ECCO etc.) and we believe that our specific process-focus here is complementary to such efforts. Nevertheless, it is important to remember the parameterization at the center of the analysis presented in the manuscript has already been tuned to improve the physical state of the ocean.

We will be very clear in revising the manuscript to state emphatically that our focus has been on evaluating the sensitivity rather than performing a more general tuning of the

WSTIR case, and that the results should be interpreted within this context.

Second, both reviewers raised concerns about the question of whether the time interval over which we considered the sensitivity to wind stirring (1958-2006) is of sufficient duration to distinguish between spurious signals associated with adjustment and the steady state response of the Southern Ocean carbon cycle to our ad hoc wind stirring parameterization. We appreciate that the reviewers raised this question, and we fully intend to clarify this point in the revised text. We wish to emphasize that we decided to use a fully interannually-varying set of atmospheric forcing fields instead of a more classical steady-state spinup with climatologically-varying fields for two reasons. The first is that in this we fully respect all of the non-linearities in the ocean response to the atmospheric forcing. The second but related point is that it facilitates a set of direct comparisons with observations, including trends. The negative side of this approach, of course, is that there is a spurious transient at the onset of the perturbation simulations that is not realistic.

Importantly, we are very much interested in both the adjustment period and the longer-term steady-state response of the system to wind stirring perturbations. The adjustment over decadal timescales is directly pertinent to the type of decadal secular-trend in the strength of reanalysis winds over the Southern Ocean as reported by Le Quéré et al. (2007). Such increases in the wind strength (considered as an annual mean and integrated over the Southern Ocean) are necessarily associated with increases in storminess and the high-frequency energy (timescales less than 10 days), and it is this high-frequency energy in the winds that may be expected to drive increases in both inertial oscillations and swells over the Southern Ocean. The central hypothesis that we have tested is that by improving the transfer of mechanical or kinetic energy to the ocean, both the steady state and the variability/trends are perturbed.

Importantly, there is also the question of whether the perturbations described for decadal timescales persist into a significant steady-state difference between WSTIR and CNTRL. This is an important question, but we have found with experience that this sensitivity of Southern Ocean biogeochemistry to wind stirring is persistent over 150 years with earlier test runs of the NEMO-PISCES configurations presented here. It is also a result we have obtained with simulations recently performed using a similar ad hoc parameterization for wind stirring in GFDL's ESM2G Earth System Model. For model configurations (both NEMO-PISCES and GFDL's ESM2G) there is an initial "shock" over a period of 1-5 years following application of the ad hoc wind stirring perturbation, and for that reason we have chosen to not include the first few years where the shock occurs in the time series of integrated CO2 fluxes.

More generally, we certainly agree that adjustments of the deep ocean circulation occur on longer time scales. In fact, we have found with experience that these changes are not impacting very strongly on the ~50 year response, except for the region of interest in the upper boundary layer of the ocean.

In summary, the persistence of the Southern Ocean biogeochemical state perturbations to the imposed ad hoc perturbation for 150 years in both the NEMO-PISCES and GFDL ESM2G models offers strong support for the results presented in the submitted manuscript.

Anonymous Referee #1

Major Comments ————–

I am a bit worried that the large difference is actually an artifact of the experimental setup.

The reviewer has raised the question of whether the large carbon flux sensitivity in the model runs (0.9 PgCyr-1) represents drift as a consequence over the ~50 years over which the perturbation was applied, rather than a more general sensitivity or robust model uncertainty.

This point has been addressed above, in the more general comments preceding the specific responses to individual reviewers. However, it can be stated here that our first

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interest is in the integration duration and he sensitivity of the results to this integration duration. We will be sure to state in the text of the revised manuscript our philosophy behind the experimental design, so that the reader does not misinterpret our results and the interpretation of our results.

I understand that the wind stirring parameterization is ad hoc. . . I would encourage the authors to explore more the uncertainty range in the used ad hoc parameterization by adding additional sensitivity experiments.

The reviewer points out the importance of quantifying more generally the uncertainty range associated with the ad hoc wind stirring parameterization. Again, we agree with this important point that this sensitivity should be explored, and we are just now starting to explore this extensively with GFDL's ESM2G model. There we are exploring more generally as well the sensitivity to the horizontal structure of the applied ad hoc wind stirring perturbation.

We do believe that it is appropriate to apply the ad hoc mixing parameterization in winter as well as summer since it is related to the mechanical forcing provided by the winds and not to heat fluxes or other processes that change sign with the march of the seasons. The deeper mixing in winter for WSTIR relative to CNTRL largely reflects preconditioning or erosion of the stratification below the base of the mixed layer that occurs during summer (addition of potential energy), with only minimal erosion of stratification locally in winter give the limited depth scale over which the parameterization is applied. In fact, during the testing phase and the preliminary runs conducted as part of this study, we did test this explicitly, and found that turning off the ad hoc mixing parameterization during winter does not impact winter mixed layer depths.

We intend in the revised manuscript to emphasize more clearly this important point of a non-local impact of summer wind stirring (addition of potential energy) in summer in contributing the stratification of the Southern Ocean in winter. Indeed, we think that this is a new result and a strength of our study, and that we have helped to underscore the

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complex nature of dynamical controls on winter mixed layer depths due to a number of interacting feedbacks, and it is our hope that our manuscript will motivate follow-up studies focused on ocean dynamical processes.

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

In the revised manuscript, we will include in Figure 1 information pertaining to the depth profile of the perturbation, to facilitate interpretation in subsequent figures.

Specific comments regarding inconsistencies

We respond to the points in the response to the minor comments of Anonymous Referee #1.

Minor Comments ——————-

What do you mean with "species"

We will change the text to refer rather to "...other ocean biogeochemical tracers".

In the abstract be much more specific...rather than saying "strong sensitivity" or "large sensitivity"

We will make the abstract more specific and quantitative in the revised manuscript.

Explain in more detail the impact of inertial oscillations and swells and waves on shear-induced turbulence

We will add a few sentences to address this shortcoming of the manuscript. These new text will communicate more effectively that potential energy is added to the ocean through the effect of shear-induced turbulence, and that this stands in contrast to eddies which are fed by potential energy in the ocean.

Include a reference to Sallée et al. (2013), and to a paper that explicitly shows the summer MLD biases in ocean only models.

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We will include a reference to Sallée et al. (2013). We will also be clearer about the Huang (2012) paper that is already referenced, and state that this does explicitly show the biases in summer-time MLD in ocean-only models.

What does “the ocean is at rest” mean?.

This means that the U, V, and W are initially zero, rather than quasi-eqiulibrium. We will clarify this point with an additional sentence.

You may add that the atmospheric CO2 concentration has also increased

We will clarify this point by adding text as suggested.

The “full transient” would be preindustrial to today. Please change wording.

We will change the text to fix this problem.

Please specify the units of latitude.

We will specify clearly that the units are degrees.

How was the tuning done?

The method used for tuning will be explained in the resubmitted version of the manuscript.

Please specify that “z” means depth in the text.

We will make this change.

I suppose you mean interannual-to-decadal variability?

Yes, we will add this.

Typo: “from”

This will be fixed.

Typo: closing bracket

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This will be fixed.

Typo: needed to simulate

This will be fixed as well.

Typo: needed to simulate

This will be fixed as well.

It is surprising 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.

This point is addressed in the responses to the Major Comments of Anonymous Referee #2 below.

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

The density criterion used for the model is 0.01 kg/m3, and for the observations it is 0.03 kg/m3. The experience of one of the coauthors (de Boyer Montégut et al., 2007, J. Clim.) in comparing observationally-based MLD products with models that this inconsistency is relatively minor, and leads to differences of at most a few meters. The method used to compute MLD is the same for both the model and observational sources, being based on the average of MLDs from instantaneous profiles. As a consequence of diurnal variability in the real ocean, a higher criterion (that of 0.3 kg/m3) is necessary when working with observations (de Boyer Montégut et al., 2004). Nevertheless, it has been confirmed that the MLD obtained from the model with both criteria is almost identical (within about 5m maximum difference for the monthly mean state over most of the region).

Please refer to the lines in the Figure panels

We will follow this suggestion.

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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?

As mentioned in the general responses outlined above, our primary interest is in the sensitivity in phasing of the seasonal cycle, rather than claiming that WSTIR represents a viable state estimate. Nevertheless we are interested in the fact that the CNTRL experiment exhibits a significant bias, and that this bias is less for the WSTIR experiment. The implication of this is that wind stirring doesn't only impact mean MLDs, it also contributes to setting the phase of re-stratification and de-stratification, and this suggests that the phase itself is not strictly controlled by buoyancy forcing.

I think you show the sea-air fluxes of CO2 in Figure 5. And what about the sensitivity in the Gulf Stream and Kuroshio regions?

We will certainly remake Figure 5 and use the opposite sign in defining air-sea CO2 fluxes, to be consistent with the other figures. The sensitivity in the fluxes between WSTIR and CNTRL reflects the fact that the wind stirring parameterization is in fact applied globally (Figure 1).

Say something about Figure 15b of Anav et al. (2013)

The reviewer makes an important point about the CMIP5 model spread in evidence over the Southern Ocean in Figure 15b of Anav et al. (2013). Independent sensitivity studies by one of the coauthors (Olivier Aumont) indicates that the sensitivity associated with growth rates in the biogeochemistry model (in particular the parameter settings in Geider et al. 1998) has a large impact on ocean biogeochemistry, and this is left as a subject for future investigation. We will state this clearly in the text. In fact, it is our hope that our study will be complementary to other studies where one parameterization is changed while all others are unchanged in a forced (uncoupled atmosphere) configuration, so as to get at this very important but complicated problem of why the spread is so large for the CMIP5 models.

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Interactive Comment

What about uncertainties in Takahashi et al. (2009)? How about using the product of Majkut (2013)?

The reviewer raises a good point about uncertainties in Takahashi (2009). Unfortunately the study of Majkut et al. (2013) has not yet been published, so that product is not yet available. Other products such as Park et al. use the Takahashi product as a climatology about which interannual perturbations are calculated, so it will suffer the same suite of biases.

I do not understand this comment. Why is the WSTIR perturbation run too short to provide insight into the uptake capacity of the Southern Ocean to carbon?

This point is addressed in more detail in the general comments above.

I would delete Figure panels 9c and 9d

Following the comments made by the reviewer, we assume that this refers to panels 10c and 10d. We agree that these panels should be removed, and by this means panels 10a and 10b can be folded in to become panels 9e and 9f, thereby decreasing the total number of panels in the paper.

Please give examples of processes that may have been tuned in the ocean biogeochemical models

We will specify clearly the biological processes that have been tuned in model runs such as the CNTRL case.

Typo in figure header

We will fix this.

Show the entire period 1958-2006

We will show this.

Use the same labels for the x-axis

This will be corrected.

Use colors consistently.

We will do this.

Anonymous Referee #ïijŠ

Major Comments —————-

The winter mixed layer (WSTIR are nearly doubled and nearly 100m deeper than observed or control simulation (>120m). I cannot understand how the authors' parameterization for the processes being simulating is capable of such a strong deepening away from the surface. . . I think a map of winter MLD is also warranted here.

Referee #2 raises a point that is similar to that raised by Referee #1, thereby underscoring the importance of clarifying this point in the revised manuscript.

However without the vertical sections of DIC, ALK, and nutrients it is impossible to know how large the impact of deepening the winter mixed layer could be. Therefore I think that these sections need to shown, or at the very least included as an Appendix.

We agree with the reviewer that the manuscript would benefit from vertical sections showing ocean tracers, and we will include these within Supplementary Materials, given the concerns regarding the length of the manuscript.

Also in light of these comments I am concerned that we are seeing a transient response rather than a steady state response as if will take some time for upper ocean interior to adjust to changes in geochemical distributions that would be expected with a longer-term deepening of the mixed layer.

Here Anonymous Reviewer #2 raises a point that has also been raised by Anonymous Reviewer #1, specifically regarding the question of whether the strong sensitivity in the modeled carbon fluxes reflects a transient response rather than a steady-state response. This is addressed in the more general comments above.

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Could the authors could show the numerically the relationship of S with TKE e.g. vertical section of zonally averaged versus depth plot to aid in the interpretation.

As stated above, we will include in Figure 1 information pertaining to the depth profile of the applied perturbation.

While the link to APO was interesting I am not clear that adds a great deal to the paper beyond suggesting that changes in the future seasonal cycle maybe detected by APO.

We apologize for having not been sufficiently clear with our interest in APO. In addition to the question of future changes in the phasing of the seasonal cycle of Southern Ocean biogeochemistry (the issue mentioned by the reviewer), we believe that the sensitivity studies presented here offer a means to interpret high-resolution APO monitoring over the Southern Ocean over the last two decades. Specifically, we chose here to first focus on the climatological difference in phase in the seasonal cycle of APO between WSTIR and CNTRL in order to establish the sensitivity to dynamical perturbations to the ocean. The results here support the more general idea that continuous APO measurements can provide a means to identify the timing and possibly the rate of re-stratification and de-stratification over the Southern Ocean, and in this sense are directly connected to the central focus of the manuscript.

That you drive the atmospheric simulations with a different forcing product than the ocean model seems inconsistent to me. Can you comment on this?

The reviewer has also suggested that there is an inherent inconsistency between the oceanic and atmospheric models used in this study. Specifically, the ocean model is forced with DRAKKAR (effectively a tweaked ERA-40 forcing), while the atmospheric forcing is derived from the NCEP reanalysis. While this does introduce a small inconsistency, our primary interest with these simulations is to use the seasonal structure of APO to evaluate the two versions of the NEMO model. Blaine et al. (2005) compared APO simulations from a suite of ten different atmospheric transport models using the same air-sea fluxes as boundary conditions, including some models driven by NCEP

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fields and some driven by ERA-40. At the high-latitude Southern Hemisphere stations, the atmospheric transport models had slightly different seasonal amplitudes, but wre in good agreement regarding the phasing of the seasonal cycle. Therefore, we consider it highly unlikely that this inconsistency between the oceanic and atmospheric models impacts our conclusions.

Reference: Blaine, T.W. (2005), Continuous measurements of atmospheric Ar/N2 as a Tracer of Air-Sea Heat-Flux: Models, Methods, and Data, PhD Thesis, University of California, San Diego, La Jolla, 225 pp.

The last section on the implications of wind induced stirring on surface and interior nutrient concentrations (Section 3.5) seems inconsistent with earlier statements in the paper, given that you say earlier that the simulations are too short to say anything about the uptake capacity of the Southern Ocean (this well maybe true given the upper ocean adjustment that needs to occur). My question is then, how can you say with confidence anything about the interior nutrient distributions on density classes associate with SAMW formation in light of the above statement?

We again apologize for not having been sufficiently clear on this point regarding timescales, and our somewhat careless characterization of timescales. When considering the uptake capacity of the Southern Ocean for carbon, we were rather referring to potential future changes in the future (effectively the 21st century) which one cannot predict from reanalysis-forced hindcast experiments. As for the question of the nutrient response in SAMW, the nutrient response is clearly a transient as the ventilation/adjustment timescales of the SAMW layer are significantly longer than the $\sim$50 year perturbation shown in the manuscript.

Minor Comments ——————-

Issue with winds being too far northward in CMIP3 models, but perhaps not in CMIP5

We will remove this unnecessary sentence.

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I am not sure that these simulations say anything significant about increased storminess.

We agree with this comment, and will correct the text to clarify this point.

In equation 1, why have you used this light limitation term?

We will explain in the text that this is done specifically to correct for biases used in previous versions of PISCES.

Do you mean that both simulations are run with the same observed atmospheric history?

We will clarify this point in the text.

The equation could be written a bit more clearly.

The formatting of the equation will be corrected.

Change eroding to erodes.

Will do.

Rephrase, as this is unclear.

Unfortunately the reviewer is referring to different line numbers than what is in the online PDF of the manuscript, so it's not clear what is being asked here.

Suggested rewording with regard to chlorophyll.

Will do.

This comparison is not very quantitative.

Unfortunately the reviewer is referring to different line numbers than what is in the online PDF of the manuscript, so it's not clear what is being asked here.

This increase in Fe/DIC ratios under low light is seen outside the Southern Ocean, but

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recent studies do not see this increase in the Southern Ocean.

This is a good point, and we will comment on this aspect of the model behavior within the context of the published literature.

Improving Figures.

We will follow the reviewer's suggestions for making the figures easier to read, thereby improving the clarity of the presentation.

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