Biogeosciences Discuss., 10, C6863–C6866, 2013
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Biogeosciences
Discussions
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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 #2

Received and published: 9 December 2013

Rodgers et al present a sensitivity study of Southern Ocean air-sea CO2 fluxes to changes in surface induced mixing not included in current ocean and earth system models such as ocean swells and waves, and near inertial oscillations. The goal of this is to better simulate observed summer mixed layer depths, which are widely accepted to be too shallow in ocean models. In this study they use observed summer mixed layer depth to develop and an additional parameterization that is added to the TKE mixed layer scheme to ameliorate or reduce this summer bias. The authors then proceed to look a number of metrics including air-sea gas exchange, oxygen and chlorophyll bloom and onset timing to explore the sensitivity to the Southern Ocean carbon cycle to this parameterization. They show that the Southern Ocean is quite sensitive to the increase in mixing. However I do have some concerns about how the parameterization was implemented and some interpretation of the changes, particularly winter

changes. That said it is pleasing to see the analysis and focus on the Southern Ocean seasonal cycle particularly, bringing in other complementary tracers beyond carbon to understand underlying mechanisms. That said paper is quite dense in sections and took more than a couple of reads to unpack the detail in parts, but overall it is well written – the grammar and structure are very good, but the figures need some attention. Therefore at this stage I would recommend that this paper be published with major corrections.

Major Comments:

The authors show that their new parameterization improves the summer mixed layer which is clearly too shallow in current models, however my major concern is not the summer but the winter. The winter mixed layers (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. The only mechanism I can think of here is through buoyancy fluxes i.e. that the enhanced mixing destratifies the water column leading to a net heat flux out of the ocean thereby explaining the stronger than observed density? But before one could make this argument one would need to see the heat content or surface fluxes to believe this - otherwise should this be implemented in this form during the winter? This need to be addressed by the authors. I think a map of the winter MLD is also warranted here.

Certainly these large differences in MLD in the winter you simulate, will increase the supply of interior ocean water to the surface, could explain the change in the character of the integrated southern ocean fluxes from uptake to outgassing, the 0.1 PgC/yr net uptake in the period 2000-2006, in part the large nitrate increase seen in Fig 11, and the other changes you see in the WSTIR simulation, rather than the summer changes which will be will much less. However without the vertical sections of DIC, ALK and nutrients it is impossible to know how the 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.

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 (see comment below also)

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

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 (which is a great point to make) which could be potentially just stated? That said I am not specialist on APO and whether this is well known or not is unclear to me.

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 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 interior nutrient distributions on density classes associated with SAMW formation in light of the above statement?

Minor Comments

P15037 l:19 The reference that the winds are too far northward is certainly true in most CMIP3 models but with the ozone included in CMIP5 I am not sure if this remains a valid comment for most models.

P15038 l:22 I like the idea but I am not convinced that these simulations say anything significant about increased storminess in the Southern Ocean

P15039: Equation 1 why have you used this light limitation term?

P150140 l:21 Do you mean that both simulations are run with the same observed atmospheric history?

P15041: The equation could be written a bit more clearly.

P15042 l:6 change eroding to erodes

P15043 l:2-3 Rephrase- as this is unclear

P15049: Suggest changing Chlorophyll and blooms to Chlorophyll bloom and onset

P15049, l:26-28 This comparison is not very quantitative – it would be nice to see a bit more a rigorous comparison – as from the plots it is difficult to see this.

P15053, l: 19-20 This increase in Fe/DIC ratio under low light is seen outside the Southern Ocean, recent studies do not see this increase in the SO see Strzepek (2012; L&O)

Figures: Overall it is very confusing as the authors switch between air-sea and sea-air fluxes through the figures e.g. Fig 5 and Fig 6– please chose a sign and be consistent. Also could the authors please add legends to the line plots.

Fig 6. Please make the x-axes consistent and consider repeating the climatology (x2)

Figs7&8: The colour bars make the interpretation very difficult – I would recommend removing the winter months and just plotting the SO. Also what are the white sections?

Fig 9(c&d) Suggest changing caption to Repeated seasonal. . ....

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