

Interactive comment on “Sea-air CO₂ fluxes in the Southern Ocean for the period 1990–2009” by A. Lenton et al.

Anonymous Referee #1

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General comments: As part of the RECCAP (REgional Carbon Cycle Assessment and Processes) project, this manuscript combines different approaches to quantify and assess the magnitude and variability of sea-air CO₂ fluxes between 1990–2009 in Southern Ocean. I think this is an excellent assessment of the different approaches for quantifying the Southern Ocean CO₂ sink. My only somewhat significant concern is that the authors correctly point out that the number of observations is very limited and likely contains a seasonal bias in most areas, yet much of the manuscript is based on comparing the various approaches to these limited observations (e.g. through the Taylor Diagram). I wonder how much the assessment might be biased by the lack of adequate observations. I am not sure what the authors could do about this other than acknowledge the short coming better in the text. I also have some minor comments listed below. I recommend publication after minor revisions.

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Specific Comments:

P287 – Here and other places inter-annual is spelled two different ways (with or without a dash). Please be consistent.

P288; line 4 – The date 1958 is very specific. I suppose this comes from the start of the Mauna Loa measurement program, but I think it implies a specific start date that is not intended here.

P291; line 2- Incorrect grammar, “Analysis of long-running of atmospheric CO₂. . .”

P291; line 25:27 – redundant. . .you already told us about positive and negative fluxes.

P292; line 8 – “global” stated twice

P292; line 14 – shouldn't this point to figure 2 and not figure 1?

P293; line 11:23 – do the models use the same gas exchange parameterization as the other approaches? How important is this? What about winds?

P296; line 4:10 – Figure 1 seems to only show 4 regions. If one includes the whole Southern Ocean as a region that still only gives 5 regions, not 6.

P299; line 9:11 – The authors state that the large deviation between biogeochemical models is due to how the fluxes are simulated, but there is no further explanation. This seems an important point that is worth a bit more explanation and justification.

P305; line 10:11 – The authors say that taking the mean of multiple models can improve the situation. What situation? The first part of the sentence says that individual models do not adequately represent both the seasonal cycle and the annual flux. Taking a mean of multiple models obviously doesn't help the individual model situation so please clarify what situation is helped.

P309; line 26:27 – What is “+ve” and “-ve”? I do not see where these terms are used anywhere else in the text. It seems odd to introduce new terms at the very end of the

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paper.

Table 1 – It is good to have a list of the models and their references, but is the surface area the only important aspect of the model that should be highlighted? For example, are any of the models isopycnal models? Do they all use the same ecosystem structure? Do they use the same winds and gas parameterization? Do they all have a sea ice model?

Table 3 – (MAD) listed twice

Figure 6 – this and subsequent figures should say what the shaded regions are.

Figure 7 – what do the different color lines mean and what is going on with the purple line in 7B?

Interactive comment on Biogeosciences Discuss., 10, 285, 2013.