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Biogeosciences Discussions

Interactive comment on "Simulating natural carbon sequestration in the Southern Ocean: on uncertainties associated with eddy parameterizations and iron deposition" *by* Heiner Dietze et al.

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This manuscript should definitely be published, but I have several comments that need to be addressed.

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.

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).

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?

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.

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

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.

Minor Comments:

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.

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.

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.

4) Pg 5, l 28. Coriolis.

5) Pg 7, l 2. Rationale.

6) Pg 8, l 26. Respective.

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

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