

Interactive comment on “Evaluating Southern Ocean biological production in two ocean biogeochemical models on daily to seasonal time-scales using satellite surface chlorophyll and O₂/Ar observations” by B. F. Jonsson et al.

Anonymous Referee #2

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This paper presents results of two coarse-resolution ocean global resolution general circulation models (OGCMs) for 4 sectors of the Southern Ocean in comparison to climatologies of satellite-derived chlorophyll from MODIS and a climatology developed from $\Delta\text{O}_2/\text{Ar}$ and biological O_2 flux observations collected on multiple cruises between 1999 and 2009. The introduction and motivation of the paper are compelling; problems with large scale optimization of OGCMs and how this restricts estimation of smaller scale mixing and seasonal-scale biological processes are presented. As dissolved O_2 cycling is affected by both mixing and biological processes at sub-seasonal scales, the

C5192

DO_2/Ar tracer could provide some means of diagnosing how coarser resolution models perform. On the other hand, assumptions of the equivalence of bioflux and NCP are known to be in error in regions of substantial mixing with subsurface waters, and perhaps models could be used to diagnose the error introduced by these assumptions.

Unfortunately, the rest of the paper does not seem to present as clear of a message. On the whole, the models seem to replicate only the ranges of chlorophyll and bioflux for each latitude, but they seem unable to replicate the timing (in either chlorophyll or bioflux) in any sector. The authors suggest that the overlap in range is a signal that the processes responsible for determining NCP are well constrained in the model, but looking at Figures 3-10 I would have to disagree. Isn't a right answer at the wrong time still a wrong answer?

Much of the paper is spent pointing out the many inconsistencies between the models and observations, but not much time is spent discussing the overall trends and what they might mean in terms of model performance. Very sweeping statements regarding construction of the ecosystem in each model or differences in mixing parameterizations are offered as potential explanation for model underperformance, but the discussion ends there. I'm left wondering what we learn from this exercise. In the end, I don't feel the stated objective of the paper (page 9635, lines 23-26) is met. A discussion for what the results “tell us about net community production and the summertime exchange between the mixed layer and the mesopelagic” seems to be lacking.

I feel the approach used here, dissolved gas modeling and the use of tracer-based constraint, is important work, but I just found the paper left the reader wanting for some more mechanistic insight. Instead, I'm left with the sense that the coarse-resolution models are capable of replicating only the most basic of patterns, and there is no real clear indication as to what might fix this. If the authors could develop that side of the story a bit more robustly, I think it would be a much stronger paper, and one that would be well-cited in future studies.

C5193

A few more specific comments are offered below:

Abstract, 9630, lines 21-25: these statements are interesting, but they do not seem to be actually discussed in the manuscript.

9633, lines 4-6: O₂ bioflux . . . is the result of NCP in the mixed layer and will be significantly diminished. . .” do you mean the NCP rate will be diminished, or the estimation of NCP from O₂/Ar will be diminished in the presence of vertical mixing. I expect you mean the latter, but it is unclear the way it is currently written.

9638, line 12: The model has been augmented to predict surface concentrations of gases, which must be dependent on bio/ecosystem model . How is NCP specified in the model? Nutrient supply?

9645, line 17 “We find that BGCCSM generally predicts the meridional variability of ranges in O₂ bioflux, suggesting that processes constraining NCP are simulated well” – but if the timing is off are the processes still well simulated?

9645, line 21: Equatorward of 60S the models. . . capture the fact that bioflux is seldom <0 – seems like the opposite is true

9647, line 27, do you mean to say whereas heterotrophic processes?

9650, line 5 and following. It seems odd to be presenting this experiment for the first time in the conclusion section; it would be better suited to the ‘discussion’ in section 4.

Figure 1: report units for NCP and bioflux

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