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# *Interactive comment on* "Evaluating Southern Ocean biological production in two ocean biogeochemical models on daily to seasonal time-scales using satellite surface chlorophyll and $O_2/Ar$ observations" *by* B. F. Jonsson et al.

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*We thank the two reviewers for their thorough and thoughtful comments. Their input will help us to strengthen the manuscript in several areas.*



## 1 Anonymous Referee 1

This paper compares two relatively coarse resolution models to satellite chlorophyll data and in situ biological O2 air-sea flux estimates. The bulk of the paper is a description of differences between the model and data with a short discussion of multiple potential reasons for data/model mismatch. The paper would be of interest to a wider readership with the addition of model analysis to more conclusively pinpoint the reasons for mismatch or at least a more thorough discussion of the implications of their findings.

*This is a fair and appropriate concern. We plan to identify the major model-data mismatches for each of the two models, and evaluate the reasons for those mismatches by comparing model fields of mixed-layer irradiance and mixed-layer nutrient concentrations with observations of these properties. We will also examine the iron fields and compare them with the limited data that are available. This comparison will allow us to attribute data-model mismatches in Chl and NCP to model errors in the simulation of properties controlling Chl and NCP in the Southern Ocean: N and P availability around the Subtropical Front, iron sufficiency and limitation throughout the Southern Ocean, SiO2 availability to support diatom blooms, and mixed-layer depth (along with other factors controlling average irradiance in the mixed layer).*

The purpose of this paper and its intended audience needs to be clarified. Most of the paper involves a detailed description of the model/data comparison shown in a series of 8 figures. However, there is only speculation about the reasons for model/data misfit. The paper would be much more interesting to modelers if it not only pointed out model discrepancies but also contained some analysis that demonstrated the causes. If the intended audience is mainly observational scientists, it would be more interesting if the paper contained greater discussion of the implications of their results. For example, the last paragraph of the abstract summarizes some interesting conclusions, but these are not actually discussed in the body of the paper that I could find.

*There are several helpful points in this paragraph. First, we agree with the recommendation to discuss causes of data-model misfits; this is dealt with in our response to the previous (first) paragraph of this review. Second, the comment about clarification of the audience is related to the reviewer's comments in the third (next) paragraph of the review. We agree, as discussed below. Third, the link between Chl and net community production is something we have worked on, and the subject of a paper in preparation. We have other data dealing with this question, but it is beyond the scope of the present paper. We agree that we need to align text and abstract, and tentatively plan to add in a discussion of this point in the text.*

The introduction is overly long and poorly focused. Although much of the paper is about model/data chlorophyll comparisons, chlorophyll is barely mentioned in the introduction whereas the bioflux is discussed at length. It's unclear how several sections are relevant to the rest of the paper, such as paragraph 4 on high-resolution eddy resolving models. Also, the section could be significantly shortened just by tightening up the language.

*We agree with the reviewer and will rewrite the introduction. This tightening should also help to clarify the purpose of the paper.*

Some other tests of the feasibility of aggregating data into a climatological year are warranted. Is the authors' conclusion that this is acceptable for their analysis sensitive to the choice of dates examined (15 Nov 15 Dec vs. other choices)? How different is the timing of the onset of the spring bloom between different years and does the "blurring" of that onset in a climatology affect the comparisons in the paper? Can anything be said about the errors in creating a climatology by examining the satellite chlorophyll data rather than only the model results? Similarly, can the authors demonstrate that the regions chosen are reasonably zonally homogeneous? Would it be better to mask some of the data near the coast from the zonal averages, since these are influenced by "processes outside the models' domain"? Some of the comparisons made are vague. For example, "the magnitude of peak Chl concentrations is simulated rather



well." Could this be quantified? The color scale is quite coarse in the upper range of Chl, so it's not clear from the figures that red in both the data and model really represents a good fit.

*Our main reason for comparing model and satellite Chlorophyll in this way is to allow for comparisons with NCP, where sparsity in observations is a major challenge. We will perform tests where we create time series from October to April for different years at a couple of latitudes to better compare the inter-annual variability. We will also generate statistics of the zonal variability. Finally, we will quantify the magnitudes of respective Chl range.*

Most of the "Conclusions" section does not discuss conclusions from the broader paper, but functions more like an appendix to support the choice of comparing model / data bioflux rather than D(O2/Ar). This should be separated into an appendix and the conclusions section strengthened to discuss implications of the paper's findings.

*We will improve the "Conclusions" section, along with the discussion.*

O2 supersaturation relative to Ar is defined with different notation from other papers here. It would be better to stick with D(O2/Ar) or DO2/Ar rather than DO2Ar (where D represents Delta here).

*We will change the text accordingly.*

Paragraph 9 of Introduction. The mixed layer "biological" O2 supersaturation . . .

*We will change the text accordingly.*

Paragraph 10 of Introduction. Cite some of the "In some studies, . . ."

*This is a typo that will be corrected.*

Section 2.1. Given that the original grid is much finer than the aggregated model grid, it's confusing to me why there are more holes at the coarser resolution. Do the authors require a certain percentage of good pixels within the aggregated model grid to not disqualify the observations?

*There are fewer holes at the coarser resolution; our phrasing is unclear and will be improved.*

Figures 7-10, panel d. It would be better to split the map at a different longitude so that the New Zealand data does not appear on both sides of the map, maybe 120oW rather than the dateline.

*We will change the figure accordingly.*

## 2 Anonymous Referee 2

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 $\Delta O_2/Ar$ 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 $DO_2/Ar$ 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?

*This is, of course, fundamentally true. However, models are used for different purposes and must be evaluated in relation to these usages. Seasonal dynamics might not be that important if the aim is to predict basin-wide decadal trends. Likewise, a model that resolves the gulf stream well but locates its position somewhat too far south can still be of great use. The fact that the models provide a reasonable range give us an important insight into where the models are in error since it suggests that ecosystem processes in the region are constrained rather well. The timing issue is important for many current scientific questions, such as the dynamics around spring bloom initiations, the seasonality of $CO_2$ uptake, and how marine ecosystems might be affected by climate change. The errors in timing shown in this manuscript could be connected to physical processes such as mixed-layer dynamics and vertical transport of nutrients, or to how phytoplankton uptake kinetics are parameterized. We believe that this manuscript provides a strong argument for the lack of specific physical processes in these types of global models (one explanation) and that our results represent a first step. It is necessary to further evaluate the models and to better assess the exact sources of errors. Such analysis is, however, a significant undertaking, and unfortunately beyond the scope of this manuscript.*

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.

*This is a good point; we will add this to the discussion*

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.

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.

*We will expand these statements when we rewrite the discussion.*

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.

*We mean the latter and will clarify this in the text.*

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?

*NCP is specified as net production - net consumption of phytoplankton biomass. This will be clarified in the text.*

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?

*As mentioned earlier, this is a question about what part of the model we are evaluating. The biogeochemical processes can be well simulated but the models might still generate poor results due to problems with the physical model. Errors in timing and regional misfits are examples of problems that could be traced to the physical models due to the representation of lateral currents, or to how the mixed-layer dynamics are parameterized. We find it encouraging that the ranges of NCP are represented so well in the models, even when resolved meridionally. This can give us some clues as to when the models can provide robust information.*

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

*That is correct; we will change the text accordingly.*

9647, line27, do you mean to say whereas heterotrophic processes?

*That is correct; we will change the text accordingly.*

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.

*We agree and, will change the manuscript accordingly.*

Figure 1: report units for NCP and bioflux

*We will change the figure accordingly.*

Interactive comment on Biogeosciences Discuss., 11, 9629, 2014.