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Interactive
Comment

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.

Anonymous Referee #1

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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.

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.

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.

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.

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.

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).

Paragraph 9 of Introduction. The mixed layer “biological” O2 supersaturation . . .

Paragraph 10 of Introduction. Cite some of the “In some studies, . . .”

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?

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.

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