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Interactive comment on “Variability in air-sea O₂ and CO₂ fluxes and its impact on atmospheric potential oxygen (APO) and the partitioning of land and ocean carbon sinks” by C. D. Nevison et al.

Anonymous Referee #2

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Review for Biogeosciences

Paper bgd-2007-0089: Variability in air-sea O₂ and CO₂ fluxes and its impact on atmospheric potential oxygen (APO) and the partitioning of land and ocean carbon sinks by C. Nevison, N. Mahowald, S. Doney, and I. Lima

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Summary:

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The authors use the atmospheric transport model MATCH forced by modeled or observed climatological oceanic O₂ and CO₂ sea-to-air fluxes to estimate their impact on Atmospheric Potential Oxygen (APO). The MATCH model results are in overall good agreement with observed seasonal and spatial patterns in APO. Both O₂ and CO₂ flux variability are important to explain variability in APO, but O₂ fluxes generally dominate the signal. By analysing various model sensitivity simulations, uncertainties (e.g. due to transport-induced variability) in the APO-based method for partitioning land and ocean carbon sinks could be estimated.

General Comments:

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This is an interesting, well-written paper that deserves publication more or less as it stands now. The analysis of uncertainties in the APO-based CO₂ land-ocean sink partitioning is timely and of interest to the readership of Biogeosciences. I have very few, mostly minor comments that the authors might want to consider.

Specific Comments:

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(1) Introduction, page 2879, lines 7-10: "Fossil fuel combustion...exerts a small influence on APO, although this is reduced by ~80% relative to the original influence of O₂/N₂" – I don't understand this. Please clarify what the effect of fossil fuel combustion is on APO and why it is not excluded from the definition of APO, given that APO is "allowing the oceanic contribution to be largely isolated".

(2) Introduction, page 2880, line 22, equation 4: The factor $\beta \cdot \gamma$ before $\alpha_{\text{bio}} \cdot F_{\text{ocean}}$ within the bracket on the right hand side of the equation is probably

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wrong and needs to be deleted.

(3) Introduction, page 2881, lines 6/7: "The ability to monitor these changes is essential to accurately predicting future levels of CO₂" – this statement needs some more explanations or, otherwise, should be removed. As it stands now it seems somewhat out of context.

(4) Introduction, page 2881, line 25: I suggest to rephrase the sentence "Compounding the solubility difference is the fact that..." – I think I know what the authors want to say, but I am still not sure whether that's how I understand this sentence.

(5) Methods, page 2884, line 18: "F_O₂_thermal was scaled down by a factor of 0.7" – what's the reason for this? Please add more informations on why this scaling factor from Jin et al. is applied.

(6) Methods section in general: it would be helpful to somewhere provide an overview over the different model setups used and simulations performed (perhaps in a table?). I would even suggest to generate a combined table with setups/simulations and a summary of the main results of the study, e.g., with regard to the uncertainties in land-carbon sinks or impact on APO etc. Currently the results section is very dense, including a lot of interesting material in text and figures. Especially for the non-specialist reader, a summary table would be very helpful.

(7) Results, page 2888, lines 5-8: comparison between the "more sophisticated ocean ecosystem model" and the "P-restoring OCMIP ocean biology parameterizations tested by Naegler et al." – does this comparison only refer to the WHOI-NCAR model? Are these biological changes the only changes between the models or did the physics change too? Please explain.

(8) Results, page 2888, line 12ff: "seasonal rectifiers" – this needs to be explained in more detail here. There is some (a bit technical) discussion on the next two pages what this seasonal rectifier effect actually is, but I think it is necessary to explain it

already here as this discussion is probably the most difficult part to understand for a non-specialist reader.

(9) Results section in general: see comment (6); adding a summary of the main findings in a table would be very helpful.

(10) All black-white Figures: the grey shadings are very hard to see when printed on (white) paper. Please use darker grey or switch to symbols or colors.

(11) Figure 3, caption: "First column" can be removed; reference to Takahashi et al. includes a black bar instead of a year.

(12) Figure 5, caption: "Top row" and "Bottom row" need to be changed to "Left column" and "Right column"

(13) Figure 6, caption: "...taking the slope of the deseasonalized time series as a central difference." – I am not sure if I understand this correctly. What exactly is done here to normalize the time series?

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