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Interactive comment on "Evaluating the ocean biogeochemical components of earth system models using atmospheric potential oxygen (APO) and ocean color data" *by* C. D. Nevison et al.

Anonymous Referee #2

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The authors present a comprehensive evaluation of the ocean biogeochemical components of 6 CMIP5 models against observed APO and Satellite estimates of phytoplankton productivity. The goal here is to offer the APO datasets, in particular, as a new constraint on the models. The authors use a transport matrix method so as to speed the process of atmospheric transport substantially. They compare this method to a direct method and only consider regions where this works well. Atmospheric transport uncertainty is smaller than variance across the ocean biogeochemical models for the high latitude sites. This is important, since the utility of APO has generally been questioned by the fact that one must do this transport calculation. The authors could point this out more clearly, i.e. in conclusions. On the whole, this is a nice analysis that

should be published after minor revisions.

Major comments: 1. The transport matrix is a good step, and I support its use for this paper. Going forward, the authors might consider developing such a matrix approach based on regions different from the square boxes of TRANSCOM that do not capture the biogeography of the ocean well. Fay and McKinley (2014) offer global biomes that would be preferable. For this paper, the authors need to clarify if the aggregation across these square biomes could impact their results and the model-to-model differences that are found. Specifically, if models don't have their major biogeochemical gradients across the TRANSCOM region boundaries, could this influence these comparisons? I also ask that TRANSCOM region boundaries be included in at least one panel in Figure 4. Fay, A. R. & McKinley, G. A. Global open-ocean biomes: mean and temporal variability. Earth Syst. Sci. Data 6, 273–284 (2014).

2. It is unfortunate that the Ventilation and NCP signals cannot be distinguished; and at the same time the NCP estimates from satellite are so uncertain that we have a reasonably loose constraint here. Showing the APOvent estimated as a residual would be helpful in Figure 3 to add to the text discussion and to better highlight this issue.

3. The conclusions state that the major issues are ATM uncertainty and uncertainty in EP100. The paper suggests to me that the ventilation separation is also quite important, and that the ATM transport is a smaller issue at the high latitudes where this paper focuses. The ATM transport issue at lower latitudes may be more an issue of the TRANSCOM region definitions and how to turn a forward model into a matrix transport approach – but this is really more a technical issue with respect to the challenge of running atmospheric models than about uncertainty in ATM transport. Overall in the conclusions, the authors need to clarify better the many issues that they reveal with their analysis so as to leave the reader with a clearer picture of the value of APO in ESM evaluation, and the remaining challenges to increasing its utility. This discussion might be well-served by a clear separation between Northern high latitudes, mid/low latitudes, and Southern high latitudes.

BGD

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Minor comments: See attached PDF.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C5672/2014/bgd-11-C5672-2014-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 11, 8485, 2014.