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Biogeosciences Discussions

Interactive comment on "Detecting an external influence on recent changes in oceanic oxygen using an optimal fingerprinting method" *by* O. D. Andrews et al.

Anonymous Referee #2

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This paper applies a statistical technique to the output of two state-of-the-art climate models participating in the IPCC 5th Assessment Report (the CMIP5 model suite) to investigate whether observed changes in ocean oxygen content can be explained by anthropogenic forcing or are simply due to natural variability. Using an optimal fingerprinting method they find that the changes are inconsistent with internal variability (as simulated in the climate models). Specifically, these results are robust for depth- and zonal-mean O2 changes for the global ocean and for zonal-mean changes for the global ocean and Pacific basin. Changes in the Atlantic basin are found to be consistent with natural variability.

The paper addresses an important question in climate science. Much research suggests that deoxygenation will occur as the climate warms, and while observations do in fact suggest this, it has remained unclear whether the changes taking place are natural or anthropogenic.

This is a paper that's definitely appropriate for BG but I have concerns. Primary among them is the fact that Figs. 2 and 3 comparing model and data don't inspire much confidence in even the most state-of-the-art climate models. Not only is the magnitude of the changes severely underestimated by the models, in some cases even the sign is incorrect. These aren't point-wise comparisons where one would hardly expect the models to perfectly replicate reality. Instead, these are plots of very large scale features. It is unclear to me whether it makes any sense to apply sophisticated statistical techniques to tease out small signals to such models at all. On a similar note, I would have liked to see how well the models do with respect to the O2 distribution itself. A figure comparing the simulated O2 distribution at different depths or vertical profiles with data would be very helpful.

Second, I found the description of the optimal fingerprinting method very hard to understand. A clearer, intuitive description of the method would be greatly beneficial to those of us who lack the requisite mathematical and statistical knowledge.

Third, to the extent I understood their approach, the largest uncertainty seems to be in the selection of values of the noise parameters v_i and v_0. Models tend to underestimate variability and the ocean is typically undersampled so this is an issue with the observations as well. I found the discussion of this and the dependence of the results on their choice of these parameters inadequate. Incidentally, a simple time series of control and historical model simulations and observations at places where such data exist (the ocean time series stations?) would be very helpful here.

Also on the topic of their method, I was surprised by the sheer number of EOFs used (O(40)). I did not quite follow how this number is selected but in most other contexts I

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Interactive Comment

have seen the Akaike Information Criterion (AIC) used. Why not here?

Sec. 3.1 first sentence: "marked zonal" should be "marked meridional" I think.

In summary, the authors tackle an interesting and important topic. On the one hand, the authors should be given credit for approaching the problem rigorously and making use of climate model simulations. On the other, what is one to make of results based on models that obviously perform very poorly even at the largest scales (by the particular metric of reproducing O2 changes). That said, on balance I am inclined to support eventual publication of the paper.

Interactive comment on Biogeosciences Discuss., 9, 12469, 2012.