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***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 #1

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The present study investigates the detectability of externally forced oceanic O₂ changes with respect to natural variability. The authors use an optimal fingerprinting method and two CMIP5 Earth system models to determine the cause of changes in the marine O₂ distribution as recorded by WOCE and earlier O₂ data.

Overall evaluation: The topic of the study is extremely relevant for a broad scope of readers from climate modellers to conservationists. However, I do have a major point of criticism regarding the degree of uncertainty introduced by the lack of knowledge about the natural background variability of O₂ and the way this is (mainly not) discussed in the paper. Apart from that the paper is well written and I am confident that it will be

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Interactive Discussion

Discussion Paper



publishable after my claims have been addressed.

Major point of criticism: After reading the paper my impression is that the authors are using their undeniable statistical skills in a somehow selective manner. On the one hand they are using truncated EOFs and an Optimal Fingerprinting method, on the other hand they do very little to constrain the crucial parameters $v(0)$ and $v(i)$ and fail to discuss the uncertainty associated with their choice of simply using the simulated O₂ variability as a reference. The authors claim that “The instrumental record of dissolved O₂ measurements is not sufficiently long to get a reliable approximation of internal climate variability (v_0), and also includes perturbations driven by external forcing.” However, surface O₂ data are available for HOT (1989-2012), BATS (1988-2012) and ESTOC (1994-2012). Of course this is not really sufficient and it includes perturbations, but it could give us at least some idea whether or not the two ESMs do a reasonable job in reproducing the interannual O₂ variability. What would an underestimation of the internal variability (as mentioned in the Discussion) mean for the potential detectability and the present conclusions of the paper? A signal-to-noise study has to come up with a reliable estimate for the noise. If the available data cannot provide such an estimate we cannot make a reliable statement about the signal detectability.

Minor points:

p. 12471 / l. 16 A Corrigendum has been published for the Schmittner et al. [2008] paper reporting an error in the calculation of light limitation. The authors might want to check if the results of the paper can be cited in this context.

p 12474 / l. 1 What is meant by “is taken into account in this data using an a priori noise estimate, ...”?

p. 12487 / l. 8 It is not really “our understanding of the background internal variability” but the authors choice to use the simulated O₂ variability as a reference.

Figure 5: y-label should read “Change in ...”

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Figure 6: On the one hand the authors make the effort to derive the reference variability shading from the “diagonal of the autocovariance matrices estimated by sampling model piControl simulations” but they do not discuss the fact that a simple factor 2 in the estimate for the background O2 variability would result in very different conclusions regarding the detectability of O2 changes.

Looking at Figures 2-5, the models do a rather poor job in reproducing the observed changes. This is not the authors fault but some of the discussions of the model-data comparisons sound more like wishful thinking.

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

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