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Interactive comment on "Is deoxygenation detectable before warming in the thermocline?" *by* Angélique Hameau et al.

Anonymous Referee #2

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This paper presents an analysis of the local time of emergence of an anthropogenic temperature and oxygen changes in the global oceans. In a recent study, the same authors (with the exception of Frolicher) used a single model to investigate the same topic. This paper went a step further by using an ensemble of Earth System models (ESMs) included in CMIP5.

The idea of using a single metric, the time of emergence (ToE) to determine the pint in time when the anthropogenic signal becomes larger than natural variability, is simple and appealing. The authors applied ToE to temperature and O2. Because ToE varies a lot among ESMs, they introduced the concept of relative ToErel by subtracting the global area-averaged ToE from ToE at each model grid point. Nevertheless, the results on ToE and ToErel would likely be sensitive to the threshold value (2) selected

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in Equation (1) as well as the way how S (anthropogenic signal) and N (internal natural variations or background noise are calculated. Although similar calculations were reported in previous papers, the authors need to describe how S and N were calculated and examine the sensitivity or robustness of the model results. There are also questions why the same methodology can be used for different regions of the global oceans? Can you use the same methodology for the tropics and mid-latitudes?

The simple concept of ToE or ToErel also has its drawback, making it hard to interpret the model results. The authors provided little or no interpretations of the major models results (Figures 1-7). After reading the manuscript, I was left with an impression that it was a purely numerical exercise.

Some detailed comments:

(1) First paragraph in Section 3.1.1 on page 7. Why does ToErel (T) show early emergence in low latitudes and between 30o and 60o S and late emergence in the western tropical Pacific? Before jumping to ToErel, tell us the global mean ToE first. Why is there no emergence in the subtropical gyres of the Indian and the Pacific oceans?

(2) Second paragraph in Section 3.1.1 on page 7. Why is the spread among ESMs small in some regions but large in other regions?

(3) Third paragraph. Can you use individual model projections to obtain a quantitative estimate on the robust/uncertainty in estimating the mean ToE from the ESM ensemble?

(4) First paragraph in Section 3.1.2 on page 7. Why is ToErel (O2) relative homogeneous?

(5) Line 3 in the third paragraph in Section 3.3 on page 9. Large warming of more than $\sim$4 Co is projected in the northern North Atlantic and round the subantarctic water. Can this projection be trusted? Many ESMs showed biases when simulating historical periods. Were these biases removed before the ToE analysis was applied?

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(6) Section 3.4. It was a good idea to check changes in AOU in order to better distinguish the O2 and temperature signals. Can you check if ventilation of the thermocline indeed decreases in regions with decrease in [-AOU] rather than relying on cited references? The authors were on the right track here to get at the mechanisms but did not go far enough. Similar mechanistic analysis should be done to explain the other results.

(7) Second paragraph in Section 4 (page 12). Most ESMs do not have fine resolutions to simulate the oxygen minimum zones (OMZs) well. As the authors indicated, the ESMs diverge in their projections for the physical and biogeochemical changes in OMZs. Some models even showed an opposite trend to the observations in recent decades. This raised an important concern about the merit of even using such models to investigate ToE because they will lead to misleading results. Why didn't you remove those ESMs that did not capture the past changes?

Given these concerns, I cannot support the manuscript for publication in Biogeosciences in its present form, and recommend that the manuscript be returned for resubmission or major revisions.

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