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

Anonymous Referee #3

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This paper focus on detecting the anthropogenic signals of both thermocline temperature and o2 (i.e. physical and biogeochemical properties) from a suite of CMIP5 models under the future projections. The study is (to some extent) based on the previous study of Hameau et al., (2019) extending to multi-model perspectives detecting the ToE of the thermocline temperature and o2 to assess the robustness of the results. The authors also introduce the relative ToE concept, results in reducing the inter-model spread compared to the traditional ToE and allows them to conduct more robust comparison.

In general I think it is important to aim on understanding changes in both physical and biogeochemical tracers together to better understand the resulting changes in marine ecosystems and combining multi-tracers could provide additional insights. I think the topic and contents of this study fits into the scope of the special issue in Biogeo-

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sciences. However, I have comments on the current manuscript.

General Comments

1. Abstract: I suggest to include some discussion (possibly in the section 4) on following up the statement "... the detection of anthropogenic impacts become more likely when using multi-tracer observations" in the abstract. Combination of two tracers will definitely provide additional information on further implications from both physical and biogeochemical perspectives. Despite the fact that two of these properties emerge on different timescales, what would authors expect to see from (or should be aware of for monitoring) future multi-tracer observations?

2. I agree that the ToE comparison among the models are not straightforward and the advantage of relative ToE is to "reduce the inclusion model uncertainty in the metric" (as stated in section 3.2 in details). However, in section 3.4 (results on ToE comparison between the two variables), the author calculate the difference between the "absolute ToE" for each models. I thought this will still include more model bias (from global ToE, which is subtracted in relative ToE). Since the author introduced an improved ToE metric, it might be better to come up with a metric comparing "relative ToE" from the two variables. This might not be straightforward but can you think of further metrics based on comparing two relative ToEs? If authors think the this will not make a difference, please explain in more details.

3. Regarding to the terminology used in the manuscript, the "internal natural variability" and "natural variability" are mixed used in the manuscript. It is not always clear what exactly the terminology defines in this context. From what I understand, the internal natural variability meant here is the variability stemming from internal climate system (specific example will be ENSO, PDO etc.) and the natural variability includes the natural "external" forcing (such as volcanic eruptions) correct? It was mixture of terminology (particularly in the discussion, which I saw the two terms were inter-changeable in some sentences) and I suggest to clearly define the terminology in the beginning

and use the term in a consistent manner.

4. I suggest the authors to explain some of the statistics in more details in the method. The noise (N) is defined as the standard deviations from the pre-industrial control simulations from each models but did the author defined standard deviations based on the temporal standard deviation using full control simulation period? This may not be a huge difference but I assume periods differs among the model. Also, the CESM1 in this study uses the last millennium spinup but is this different from the preindustrial control simulations (I assumed yes)? I think introducing a schematic based on for example Figure A1 c) d) (or similar figure) will help explaining the N and S, and at which point you define the ToE used in this study in a more visualized way.

Specific Comments

- Page 2, L24-25: I suggest to cite one of the Oschlies review paper in addition to Cocco and Bopp's papers (underestimating the trend and variability of o2 in the model simulations).

- Page 4, Method, Earth system models section: I am guessing this will not affect much on the overall results but why did you use your own CESM1 (with different spin-up procedures) for multi-model comparison instead of using CMIP5 CESM? In addition, is the CESM1 used in this study the same as the one in the early Hameau et al., (2019)?

- Page 9, L1: What do you exactly mean by "combining climate sensitivity to anthropogenic forcing and natural variability in one metric"? I understand combining the anthropogenic forcing and natural variability part but I was not fully sure about the climate sensitivity statement.

- Page 12, L18: "an increasing ventilation" (following Gnanadesikan et al., 2007): Strictly speaking I would not state "an increasing ventilation" but it is more of a consequence of reduced upwelling as discussed in Gnanadesikan et al., 2007.

- Figure 1. I understand from the Figure 1 that the SD reduces for the relative ToE

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but I also have some impression that two metrics could still give similar information. It might help to show additional map of ToE SD difference between Figure 1 (b) and (c) for example to show the bias (spread) reduction using this metric.

- Figure 6. For consistency, I suggest to used the same hatching as the previous figures to show the regions that one of the variables has not emerged by 2099 rather than saturated colors.

- Figure 8. I like this summary figure aiming on incorporating emergent signals of both thermocline temperature and o2, along with AOU information (mainly indicating the water mass age information). Minor things on this, I think the x-axis is supposed to be -AOU (it puzzled me for a moment) and x-axis label should be corrected.

References:

[1] Hameau, A., Mignot, J., and Joos, F. (2019), Assessment of time of emergence of anthropogenic deoxygenation and warming: insights from a CESM simulation from 850 to 2100 CE, Biogeosciences, 16, 1755–1780, https://doi.org/https://doi.org/10.5194/bg-16-1755-2019.

[2] Oschlies et al., (2017), Patterns of deoxygenation: sensitivity to natural and anthropogenic drivers. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 375 (2102). p. 20160325. DOI 10.1098/rsta.2016.0325.

[3] Gnanadesikan, A., Russell, J. L., and Zeng, F.: How does ocean ventilation change under global warming?, Ocean Science, 3, 43–53.

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