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Interactive comment on "Assessment of time of emergence of anthropogenic deoxygenation and warming: insights from a CESM simulation from 850 to 2100 CE" *by* Angélique Hameau et al.

Anonymous Referee #2

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The authors has presented an interesting study, with a number of valuable analyses and interpretations regarding the Time of Emergence for oxygen and temperature in the ocean. Although the material presented in the manuscript should be of value to the broader research community, it would benefit greatly from revisions to improve clarity, and also to become better anchored in a discussion of existing scientific literature. Suggestions for improving the scientific clarity and impact are detailed below.

MAIN COMMENTS

The Introduction is not sufficiently focused and was a bit all over the map (encyclopedias on multiple topics), and should be streamlined. It would be of great value to state

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Discussion paper

clearly why trend detection is priority when interpreting observations, and to relate this to the realm of uncertainty in climate change projections. There are several sources of uncertainty and/or ambiguity in trend detection relating to the "noise" component of trend detection. One issue (that emphasized in the manuscript) is the distinction between natural and internal variability, with there being a need to understand and quantify this distinction. This analysis is great, and warrants emphasis. But a second issue is related to the way in which noise is calculated. In most studies that also emphasize initial condition large ensemble methods, the method of Deser et al. (2014) is used to estimate noise, with this typically involving linear trends calculated over decades rather than STD of annual means to calculate ToE. As the amplitude of inter annual and decadal variability is typically expected to be distinct, at face value it is not obvious how to connect the estimates give here with more general research using large ensemble simulations. It's very important to emphasize this point, while it seems also OK to point out that there is nothing inherently flawed or wrong with the method proposed in the manuscript, it is just somewhat different, and more similar to methods that have been applied cross multiple models in inter comparison studies.

As a related point, I believe it would be valuable to communicate the implications of this study to the broader community, given the discrepancies noted above. Is there a way to bridge the different methodologies with large ensemble methods, perhaps by sampling (randomly?) decadal trends from a 1000+ year Last Millennium simulation? That would still be different from what is done with large ensemble runs. Do the authors recommend at all major modeling centers that are embarking on large ensemble simulations also include Last Millennium simulations? Or for CMIP6 protocols (where the historical period goes through 2014) can potential biases be estimated by comparing full historical ensemble runs with greenhouse CO2-only (for example, for estimating internal variability) runs over 1850-2014?

MINOR COMMENTS

(1) References Near the top of Pg. 3 the authors refer to Hawkins and Sutton (2012),

where the methods considered for calculating Time of Emergence are not the same as those typically used with large ensemble methods (Deser et al., 2014). This should be clarified with regard to the comments above. Also, are the authors sure that the Long et al. (2016) paper used the same method to calculate noise as that of Hawkins and Sutton?

(2) Model configuration Is the CAM4 model considered here the same as the atmospheric model component used by Kay et al. (2015)? More generally, how do the model components differ, and if so, how might this itself impact variability?

(3) ToE, Pg. 9, line 8 and ensuing paragraph The section header "ToE" needs to be expanded into "Time of Emergence (ToE)", or something similarly appropriate. It should also be stated explicitly in this paragraph which year is used as a "reference" for the ToE calculations (ToE relative to what year?)

(4) Fig. 2: Pg. 9, describing text The patterns and timescales should be compared with existing published literature for oxygen and temperature, with any caveats about the methods used to calculated noise.

(5) LM, pg 11, noie 25: The authors need to spell out LM as Last Millennium here.

(6) pg 11, lines 30-34 It would improve the clarity of the presentation if a bit more detail were provided here. What are the percent differences, and over what regions?

(7) Section 4.1 on pg 16 The references described here are not appropriate for linking biogeochemistry and climate modes, with the exception of the Bacastow reference. For the case of ENSO it would be appropriate to reference the study of Winguth et a l. (1990s?). I believe for the SAM there were the studies in 2006-2007 of LeQuere, Lenton, and Lovenduski, and for the PDO you might consider the study of McKinley (2006).

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