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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 #1

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General comments

The manuscript addresses the important issue of climate signal emergence in ocean oxygen concentrations using the Community Earth System Model (CESM). The authors employ the widely used Time of Emergence (ToE) signal-to-noise metric. There is a new focus in the analysis on the impacts of natural external forcings such as volcanism over the last millennium, and projections are analysed over the 21st century (for dissolved oxygen and temperature fields). The work is accomplished, certainly relevant to the scope of BG, and some interesting conclusions are reached regarding spatial patterns in Time of Emergence (ToE). The methods are appropriate and well

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justified, including a careful discussion of the de-trending (de-drifting) procedure. It's worth noting that this work is not entirely novel, and as the authors point out many other similar ToE studies have been done on CMIP5-class models, but as above I'm confident that the focus on natural external forcing and the last millennium is sufficiently distinct to justify publication. However I have two general issues which require the authors attention in revision along with specific / technical comments listed below.

1. Primarily the manuscript requires major revision in terms of language and readability. The paper would be significantly enhanced by a reduced wordcount and tightening of the text, along with a close check for typos and grammar. There is need for improvements to sentence structure throughout. I have flagged some specific issues in the Technical Comments but this is not comprehensive and will need to be re-reviewed with this in mind. In addition, I suggest that the sections of the Results which test ToE methodological assumptions such as noise estimates (e.g. Sect. 3.3.2 – 3.3.3) should be (at least) condensed or potentially moved to the Supplement. The Discussion also includes lots of sub-sections and is not very readable in its present form.

Some of this is certainly a matter of opinion, but it would be great to see a more streamlined manuscript which focuses on the central ToE results and the separation of O2 changes into AOU, solubility components.

Also there are lots of maps in the Results which do not necessarily add much to the paper (e.g. Fig 4. panel) – are they all necessary? e.g. do we need surface, thermocline and full depth averaged maps? (see below).

2. The authors spend a lot of time talking about uncertainties in noise estimates and methodological approach, however only briefly mention (Sect. 4) the potentially major influence of model structural error in their analysis. Specifically, the study does not validate the CESM simulated pattern of historical oxygen change using observations (only the mean state in Fig A2) or compare the magnitude of simulated internal variability to e.g. long-term oxygen time series data. It is likely that the widely reported

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(e.g. Stramma et al., 2012) lack of model-data agreement in reproducing observed low-latitude deoxygenation plays a major role in the authors reported (low) ToE in the tropics (e.g. Fig 2a). This limitation is true of all ToE studies and somewhat unavoidable, however it needs to be explored more as a major source of uncertainty in the study. To this end I suggest the authors include a comparison of CESM simulated historical deoxygenation to observations (e.g. vs the global Schmidtko et al. [2017] dataset) since this influences considerably the interpretation of ToE estimates derived for the 21st century.

The authors also note in Sect. 4 that observed variability in oxygen (at HOTS) may be a factor of two larger than simulated by CESM. Along with the forced trend mentioned above if the noise is underestimated this will considerably impact upon CESM derived ToE estimates. The authors also need to more thoroughly address this source of uncertainty in the manuscript.

Specific comments

Line 9 – 10: "natural variability [. . .] are systematically larger than internal variability". This needs to be clearer. I think you mean control estimates are not good enough as they don't include natural external forcings like volcanism, not that model simulated variability is smaller than observed? (or both?)

Page 1 Line 12: do you mean "anoxic" or suboxic?

Introduction Page 1 line 18. Do you need to reference all these studies to introduce the well-known concept of ventilation age? And then the next statement that warming leads to solubility driven deoxygenation is not referenced.

Page 2 Line 5: Long et al. do not use optimal fingerprinting in their assessment. Please distinguish between studies which use optimal fingerprinting and ToE (these approaches are substantially different since optimal detection studies include an observed change and ToE are primarily model based)

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Page 2 Line 6: For completeness there is another ToE ocean biogeochemistry study by Christian (2014) PLOS ONE

Page 3 Line 10. As the authors note, multiple studies have done ToE on biogeochemistry for CMIP5-class models. It is necessary in the Introduction to highlight what this study does differently and why it is important. I'm confident this can be done e.g. focus on natural external forcings, millennial scale simulations etc.

Page 3 line 32: here and elsewhere I think the use of the terms like "natural variations in external forcing factors" is confusing. Please use concise, clear terminology e.g. "natural external forcings"

Page 8. Section 2.2.3: More justification for the chosen standard deviation noise thresholds is required along with reference to the associated statistical confidence levels required.

Figure 1. The different y-axes between left (last millennium) and right (future forcings) and upper (global) and lower (surface) are understandable but confusing given the amount of detail (AOU vs O2sol) in the panel. This Figure needs to be reworked for clarity to focus on key results. E.g. is surface and full depth averaged oxygen concentration important? Or just the thermocline?

Page 9 – 10 Sect. 3.2. and page 10 – 11 Sect 3.2. These sections are overlong and should be more concise to focus on the key messages.

Figure 4. There is not much added information in this panel – could the Figure be condensed ?

Figure 5, Sect 3.3.2 and Figure 6, Section 3.3.3: Suggest to (at least) condense these predominantly methodological Sections.

Page 13 Sect. 3.4.1 Lines 8 to 18. I suggest to move this extra analysis of Fig 1 back to Sect 3.1 or simplify Figure 1 and add another Figure here to look at AOU/solubility contributions.

Page 16 Line 1 - 4. See overall comments on model uncertainty regarding noise and forced response.

Section 4. The Discussion should be re-written to be more concise and focused.

Technical comments

Abstract Line 1. “aggravate” is a little unclear here suggest to reword

Introduction and throughout: many references are used to support each point. Please be more selective – 13 references for one statement is quite a lot.

Introduction Page 1 line 20. Suggest to cite Capotondi et al. 2012 on stratification

Introduction Page 3 line 22. “by appropriately adding [..]” this is unclear please rework sentence

Introduction Page 3 line line 25 “are considered not at all” rephrase please

Introduction Page 3 line 33 “these externally forced variability” rephrase please

Introduction Page 3 line 34 repetition of “last millennium climate simulations” rephrase please

Introduction Page 4 line 12 remove “the” before ‘anthropogenic”

Page 4 line 25: please rephrase (also replace with "rely")

Page 5 line 3. Replace “stands for”

Page 5 line 19. Of carbon?

Page 6 line 5: Should supplemental figures be named in the order they are introduced? (applies to later sections)

Page 10. Replace “outweight” with “exceed”

Page 11 Line 26 “larger”

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Page 13 – 14. Formatting issues with subscripts

Page 15 Line 25 missing word after “enables”

Page 17 Line 2 . Missing parentheses

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