

## ***Interactive comment on “<sup>231</sup>Pa and <sup>230</sup>Th in the ocean model of the Community Earth System Model (CESM1.3)” by Sifan Gu and Zhengyu Liu***

### **Anonymous Referee #1**

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The authors present the implementation of Pa and Th into the community model CESM1.3 in two variants, a biotic and biotic, and compare the control simulations to observations. A classical hosing experiment is carried out and some preliminary analyses are given. The implementation is an important step for later use of this model by the paleoceanographic community, but the description is not sufficiently detailed to be publishable at this stage. Major revisions are recommended.

Comments: 1) The authors seem unaware of the recent paper by Rempfer et al (2017, EPSL) which describes in detail how Pa and Th are implemented in their 3D ocean model. Their description is more comprehensive and complete in the sense that an interested reader has all information available to carry out the model development in another model. This comprehensiveness is also a hallmark of the earlier paper by Siddall et al (2005, EPSL). The paper here, however, does not provide the detail this

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reviewer is expecting of a GSMD contribution. The paper needs to take the Rempfer study into consideration and describe carefully in which way the authors' approach is the same, or where it deviates, and why. In the latter case, all parameter values are to be given, as this is a contribution to GSMD (with emphasis on Development which means that a developer can take this paper and create a Pa, Th model component from this information). At the current stage, the paper does not provide this information.

2) Comment 1) does not only apply to the model description only but also to the one example Gu and Liu show, the effect of a collapse of the AMOC on Pa and Th. Rempfer et al (2017) carried out a water hosing experiment and analysed in detail how changes in the Pa/Th ratio inform about circulation changes in the North Atlantic. A critical comparison of the present results with Rempfer et al. is missing.

3) The authors state on line 134 that their implementation is based on Siddall et al (2005). Does this mean that it is identical, i.e. all the parameter values are the same? If not, a Table with the parameter values would be needed for complete information. As stated above, this would be a requirement for GSMD; too many studies are published nowadays with incomplete information.

4) The text on lines 144ff does some forward referencing to the equations. This should be avoided. First set the context, then introduce the equations and describe every parameter and variable that occurs in these equations. This would ensure easier reading. For example, eq 4 shows many parameters whose values are not given. On line 167 the authors say that eq 4 can be derived from (1) and (2). This is not obvious from the formulations of (1) and (2). Rather eq 4 is a variant of eq 10 of Siddall et al (2005). Again more detail and clarity are needed here.

5) A central point of this paper is the implementation of Pa and Th in abiotic and biotic formulations. In order to appreciate this, more description and analysis should be provided. For example, the prescribed and simulated particle fluxes in different ocean provinces should be shown and compared. It should be quantified how and where

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they differ in order to better understand the consequence of these choices for Pa and Th. Given the present level of information in the paper, one can be convinced that the agreement of the two approaches for the control simulation is satisfactory. However, in the transient experiments, differences are rather large depending on the location where the variations are analysed (Fig. 7). Without a more detailed description, the reader is unable to understand the differences. For example, it would be most useful in Fig. 2 below the first row to add panels of the biotic simulation for direct comparison. Implicitly, this information is provided in the scatter plots e)-h), but it would be easier for the reader to see the spatial distribution for the concentrations of the four constituents next to one another and to compare abiotic with biotic this way.

8) The authors follow the approach of Siddall et al (2015) and Rempfer et al (2017) to compare their control simulation with observations. Information is incomplete here as to which data has been used for this comparison. A table in the paper or in the supplementary material summarizing which data has been used would be helpful.

9) Further to 8) reference to the important effort of GEOTRACES is missing. GEOTRACES offers a wealth of relevant new data. They were used in Rempfer et al (2017) and should also be incorporated into this study for a better and more comprehensive comparison.

10) Information is missing under what conditions Exp\_1 and Exp\_2 were run. Were these abiotic or biotic simulations? Also, this is not evident in Fig. 5.

11) In Fig. 2b high values of Th\_d are noted in the Southern Ocean. This is in contrast to Siddall et al. (2005, their Fig.2) and should be discussed. Is this also occurring in the biotic simulations (see also comment 7. Might the opal fluxes be too high there?

12) Lines 237-240: This statement is not instructive, nor is it very useful. It is noted that the author have performed only one quite simple sensitivity experiment, and this is increasing or decreasing K which changes all partition coefficients simultaneously. This limited perspective does, of course, not shed too much light on this important

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question. At least some more thoughts by the authors should be offered here, if not some more pertinent sensitivity tests with their model.

13) Section 4.3. Here, a deeper analysis is required, in particular a comparison with the recent paper of Rempfer et al (2017). They provide an interesting spatial consideration of correlation and Pa/Th-AMOC sensitivity in the North Atlantic Ocean in order to shed light on the controversy whether, and to what extent, Pa/Th changes reflect AMOC changes. The paper here would be able to make an important further contribution to this question, but this opportunity is missed. The authors may argue that this is a paper for GSMD, and hence addressing scientific questions is not the primary purpose. This reviewer might agree with this view if the necessary information for model developers. At this stage, unfortunately, neither is the case.

14) On line 307 the authors argue that the abiotic version captures the major features of the transient simulation. Considering Fig. 7c, d, e, f this statement seems overstating the agreement. Important differences in the transient signal are evident. This should be discussed and explained.

15) From Table 1 it is evident that dust input was not considered in these experiments, although this is not explicitly stated in the text. It would be important to inform the reader why this choice was made, or better, quantify the effect on the Pa and Th concentrations if dust input is included in the simulations.

16) line 383-385. The authors seemed to copy this part from another of their GSMD papers.

17) Throughout the paper, the English should be carefully revisited, in particular in section 4.3. In that section, more paragraphs would ease the reading.

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