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Interactive comment on "^{231}Pa and ^{230}Th in the ocean model of the Community Earth System Model (CESM1.3)" *by* Sifan Gu and Zhengyu Liu

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We thank the reviewer for his/her time for constructing the comments.

In the following, we have addressed all comments.

"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 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). A the current stage, the paper does not provide this information."

Thanks for pointing out the paper by Rempfer et al., (2017). We have made substantial changes describing how Pa and Th are implemented in our model and the difference between Rempfer et al., (2017). We add a short review on previous modeling efforts (Line 80-89) and the similarity and difference between our method and previous studies (Line 200-207). Eq. (3) is the conservation equation for Pa and Th, which is how Pa and Th calculated in the model. The calculations of Pa and Th are based on this equation. In section 2.3, we explicitly describe each term in Eq. (3) and the values of different parameters, which includes all the information for reproduce the model development in another model. Also, we add Table 1 and Table 2 to show the abbreviation and values of different parameters used in text.

"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."

In the revised version, we compare our results with Rempfer et al. 2017. We get similar particulate Pa/Th response in North Atlantic (their Fig. 8 and our Fig. 12) and we add a discussion in the text (Line 423-444).

"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 fro GSMD; too many studies are published nowadays with incomplete information."

Sorry we did not make this clear enough. Yes, the parameters in the implementation is the same as Siddall et al., (2005). Values of different parameters are given in the text when it first appears. To make it clearer, we add a table to summarize the parameters and variable in Table 1 and Table 2.

"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."

Thanks for pointing out the problems in the equations. We have rearranged the context and the equations as suggested. We explicitly show how can be calculated from Eq. 1 and 2 (Eq. 4, 5 and 6).

"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 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

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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."

First of all, the term abiotic and biotic seems to be not appropriate since Pa and Th are not actually involved in the biological activity. We change the term to “p-fixed” and “p-coupled” which clearly indicates the difference between two versions.

Thanks for suggesting ways to show that the p-fixed and p-coupled versions give almost identical results in CTRL. We follow this advice and show directly the results of these two versions in Fig. 2, 3 and 4. Clearly, readers can see that p-fixed and p-coupled are similar in CTRL.

For the HOSING experiment, we also add Fig. 8 to show the differences in particle production during AMOC_on and AMOC_off, which will help the discussion about the p-fixed and p-coupled Pa/Th differences in HOSING.

"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."

Thanks for suggesting to use a table to show the data used. We have added Table 3 to show the references used in model data comparison. Most of the data are also used in Rempfer et al., (2017).

"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."

Thanks for pointing out available GEOTRACES data. We have included those in the observation (Table 3). We show model results along two GEOTRACES transects as in Rempfer et al., (2017) (Fig. 2 and 3) for direct comparision. Our results are similar as the case Re3d in Rempfer et al. (2017), which does not include boundary scavenging and sediment resuspensions.

"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."

Sensitivity experiment Exp 1 and Exp 2 are abiotic simulations for computational efficiency (Line 212-213). Exp1 and Exp2 are carried under the same forcing as CTRL (Line 221-222).

"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?"

Since the GEOTRACE transects are more appropriate for model data comparison, we replace the zonal mean figure with the GEOTRACE transects and move the zonal mean figure to the supplementary information (Fig. S3). The high values of Th_d in the Southern Ocean around 60S in the model is consistent observations (Fig. S3b) since observations of Th_d from 60S-55S are much larger than Th_d from 55S-40S. In addition, our model is in much higher resolution than Siddall et al., (2005). The maximum Th_d locates at around 60S, decreasing if further southward in our model. Similar pattern also appears in Siddall et al., (2005). Their Th_d maximum is at around 55S, decreasing southward (but only two grid available in their model).

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

Thanks for pointing this out. We have removed this part. The poor performance in simulating particulate Pa and Th is also in Siddall et al., (2005) and Dutay et al., (2009). Rempfer et al., (2017) only shows Pa_p/Th_p and does not show individual Pa_p and Th_p. It's possible the performace is limited by our choice of modeling scheme since the process in controlling Pa and Th activities are essentially the same among our study, Siddall et al., (2005) and Dutay et al., (2009). Although individual Pa_p and Th_p do not agree well with the observations, the ratio of Pa_p/Th_p in our CTRL experiment show similar results as in Rempfer et al. (2017) and sediment Pa_p/Th_p distribution agrees with available observations. And the ratio of Pa/Th is what we are interested in.

"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."

Thanks for pointing out the interesting spatial dependence behavior of Pa/Th in the hosing experiment in Rempfer et al., 2017. Our model, with much higher resolution, shows similar spatial pattern as theirs (Fig. 12). We add discussion of this spatial dependence in Line 423-444. This spatial dependence is mainly caused by AMOC, since the pattern in p-fixed and p-coupled are similar.

"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."

We agree that there are many differences between p-fixed and p-coupled response to freshwater forcing. If we compare Fig. 10 b and d, in North Atlantic, the sediment Pa/Th overall show increase in both p-fixed and p-coupled (except opal maximum region). In Fig. 9, the transient evolution figure, if we neglect the initial drop in p-coupled (red), the long-term trend between p-coupled and p-fixed are the same. Therefore, over low productivity and long time scale, the p-fixed capture the major features of sediment Pa/Th change and suggest that AMOC change is dominant. But on short time scale and over high productivity region, p-coupled response behaves quite differently from p-fixed. We discuss the differences in the revised manuscript (Line 381-420).

"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."

Thanks for the suggestions. Dust is not included in the calculation. We use the parameters used in the control experiment in Siddall et al., (2005), which the partition coefficient for dust is 0. They also did sensitivity experiment and find dust flux is unimportant for Pa/Th fractionation. We have modified Fig.1, Table 1 and text (line135-137) accordingly.

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

Sorry for the mistake in the code availability part. We have fixed the error.

"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."

Follow this suggestion, we re-write section 4.3. In the revised version, we first discuss the p-fixed sediment Pa/Th response in the Atlantic, which generally increase during AMOC_off (Line 339-358) and the magnitude of increase is related to particle distribution (Line 359-370). Then we discuss the p-coupled response. The change in sediment Pa/Th between AMOC_on and AMOC_off in p-coupled are similar to p-fixed in most North Atlantic (Line 371-380), but there are differences especially on short time scale and over high productivity region (Line 381-420). At last, we discuss the change in particulate Pa/Th in North Atlantic and show the depth dependence of the change (Line 423-444).

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-82/gmd-2017-82-AC1-supplement.pdf

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