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

The aim of this MS is the examination $^{231}Pa/^{230}Th$ in order to reconstruct past AMOC strengths. The $^{231}Pa/^{230}Th$ method is somewhat under debate due to the fact that (at least) two competing factors control sedimentary $^{231}Pa/^{230}Th$: AMOC and particle fluxes. Although the AMOC seems to be of first order control on $^{231}Pa/^{230}Th$ the question is still valid when and where particle fluxes may take over control or may at least influence the circulation signal significantly. From this point of view I welcome very much this new attempt of deciphering the behaviour of $^{231}Pa/^{230}Th$ in the ocean and to implement both key isotopes in a climate model.

The authors build up on previous work by (Siddall et al., 2005).They have improved the CESM by implementing a coupled biogeochemical module. The output of the model is compared to observations in order to "reality-check" the model and the chosen parameters. Further they compare the output of the coupled marine ecosystem model with a prescribed particle field in order to gain knowledge about the sensitivity of the applied particle flux forcing.

The main point of criticism I have here is their comparison to observational data, which I find is too nebulous and not supported by newer data. There is an obvious lack of consideration of recent papers. More recent studies would provide a much better basis for comparison and reality-checks of the model. The references for the observational data given in the MS are quite old holding mostly data obtained by the noisy counting-method resulting in large analytical uncertainties. Instead the model should be cross-checked with newer sedimentary and water column data. I don't see much benefit from comparing "biotic" against "abiotic" ^{231}Pa and ^{230}Th particle-fluxes (Fig. 2), as long as the absolute values have not been tested against new observational data. The authors urgently need to test the output of the model versus recent sedimentary data (e.g. (Böhm et al., 2015; Bradtmiller et al., 2014; Burckel et al., 2016; Henry et al., 2016; Hoffmann et al., 2013; Jonkers et al., 2015; Lippold et al., 2011; Lippold et al., 2016; Lippold et al., 2012; Luo et al., 2015; Negre et al., 2010; Roberts et al., 2014; Rutgers van der Loeff et al., 2016)),
water data (e.g. (Deng et al., 2014; Hayes et al., 2014; Hayes et al., 2013; Hayes et al., 2015a; Hayes et al., 2015b; Kretschmer et al., 2011))
and most importantly other modelling studies (e.g. (Dutay et al., 2015; Lippold et al., 2011; Rempfer et al., 2017)).
I find the terms "biotic $^{231}Pa/^{230}Th$" and "abiotic $^{231}Pa/^{230}Th$" quite confusing. Since there is no biotic ^{231}Pa and ^{230}Th these terms should be used only to distinguish between the usage of particle fields in the model.
Given that (Rempfer et al., 2017) recently provided insights into an upgraded approach by (Siddall et al., 2005) and (Siddall et al., 2007), including a bio-geochemical-module in the model, I do not see much advance provided by the here presented MS. I did not find a reference to (Rempfer et al., 2017), maybe because this is a very recent publication, but I don't think the authors should neglect this paper in a new version.
Although I welcome very much the provision of the Fortran code the reader is left alone with the comparison between model and observations (Fig.3) without sufficient information about the values, observational error bars and references. The color code in Fig. 3 may hold some information about the water depths, but since (already) older publications demandingly have shown, that the correlation of $^{231}Pa/^{230}Th$ with water-depth seems to be a manifested pattern of AMOC in the $^{231}Pa/^{230}Th$ distribution (Burckel et al., 2016; Gherardi et al., 2009; Gherardi et

al., 2010; Hoffmann et al., 2013; Luo et al., 2010; Luo et al., 2015) this feature is required to be reproduced by a meaningful model. But I'm not able to see this from the provided figures. By the way, the diagrams are way too detailed (in terms of graphic resolution) demanding a lot of computer resources and slowing down even my reasonably new computer just by scrolling down.

The table for the K values (Table 1) needs to be accompanied by references, because these values vary within a wide range according to the studies by (Chase et al., 2002, 2004; Hayes et al., 2013; Hayes et al., 2015b; Kretschmer et al., 2011; Kretschmer et al., 2008; Luo et al., 1999, 2003, 2004) and others. I think, a well selected digest of values can be found at the new study by (Rempfer et al., 2017).

Besides the shortcomings of the MS regarding the observational data, I also find patterns in the model output, which are not observed in reality to my knowledge. E.g. the appearance of a high opal/POC field in the NW-Atlantic. Further, I see an obvious mismatch of model and observations in Fig. 5, which is not explained.

In summary, it is hard for me to see that the here presented model approach provides any new insights on the $^{231}Pa/^{230}Th$ method. Due to the lack of information about the model-data comparison it is not possible to assess the quality of the model and the applied parameters. Consequently I suggest revising both the model runs and the MS thoroughly before publication can be considered.

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