

## ***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***

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The paper “<sup>231</sup>Pa and <sup>230</sup>Th in the ocean model of the community Earth system model (CESM1.3)” by S. Gu and Z. Liu is presenting the implementation of <sup>231</sup>Pa and <sup>230</sup>Th in their general circulation model. It is mainly following the procedure defined by previous work Siddall et al (2005) and Dutay et al (2009). The implementation of the tracers in the model is described and results are compared to observations. However some severe weaknesses are found in the manuscript. The comparison with observation is insufficient, it is strictly following the analysis performed by Siddall et al in 2009, while it now exists, thanks to the GEOTRACES project, new data set. Moreover, the paper does not only show the implementation of the tracer in the model and its validation, which is the scope of the GMD journal, it also proposes the response to hosing experiments that is paleoclimate studies that are applications that are not

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devoted to this journal, Climate of the Past would be a more appropriate journal if this study was more correctly analysed. For all these reasons I propose to reject this paper from publication in GMD.

Specific comments: Page 4 section 2.2. The authors show particle flux surface horizontal distribution without concrete comparison with observation. This diagnostic is interesting but it is not sufficient for the proposed study. The model uses particle concentrations and results are strongly dependent to the quality of these fields. It now exists observations to validate the particle fields (Lam et al, 2015) that were not available for Siddall et al (2005) and Dutay et al (2009). A more detailed analysis of the vertical particle concentration distribution at large scale is required. Page 5 section 2.3 Abiotic and Biotic name for simulations are not appropriate. These names suggest that the tracers are subject to different processes while it is not the case. The two approaches are the same except that the particle fields are fixed in the Abiotic run. None biogeochemical process affects the tracer except adsorption and desorption onto particles, so the appellation Biotic run seems exaggerated. Line 162: No validation of particle fields is performed while it affects strongly the model results. Observations are now available (see for instance Lam et al 2015)

Pages 7 and 8 section 4, results Definition and way of estimation of the residence time given for the tracers should be explained.

Comparison of Atlantic zonal averaged model results with observations is no more adequate. It is strictly following analysis performed by Siddall et al (2005) and Dutay et al (2009) a decade ago, but now many new observations are available in the different basins thanks to the GEOTRACES program. This validation is not appropriate any more. Discussion concerning the ratio <sup>231</sup>Pa/<sup>230</sup>Th is very poor. More detailed analysis must be given. For instance what causes low ratio in the north atlantics south of Grennland: convection?

Page 10 and 11. This part is already an attempt to use the model development for

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scientific question. It is not the purpose of GMD papers. This part should be more deeply analysed and submitted to another more appropriate journal (eg climate of the past)

In conclusion, GMD journal propose to publish model development, and authors can follow procedure previously published with other model. However it can not accept copy of papers published a decade ago and following same analysis while new appropriate observations are now available

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