Geoscientific Model Development Discussions

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Comment on gmd-2021-5

Anonymous Referee #2

Referee comment on "Global simulation of dissolved ^{231}Pa and ^{230}Th in the ocean and the sedimentary ^{231}Pa/^{230}Th ratios with the ocean general circulation model COCO ver4.0" by Yusuke Sasaki et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-5-RC2, 2021

This study explores the processes that control the distribution of 231Pa and 230Th in the oceans and underlying sediments using COCO V4.0, an Ocean General Circulation Model (OGCM), from Hasumi 2006.

They implemented 231Pa and 230Th in the model using offline tracer simulations based on physical fields from COCO. They implemented bottom scavenging as well as a "dependence of scavenging efficiency on particle concentration" in the model.

General comments

- The most puzzling aspect of this manuscript is the **lack of use of recent modeling results and the almost total lack of comparison with these model simulations** (e.g. Missiaen et al., 2020a and 2020b; van Hulten et al., 2018; Rempfer et al., 2017; Lippold et al., 2012; Luo et al., 2010; Dutay et al., 2009; Roy-Barman, 2009). It is all the more surprising that most of these papers are cited by the authors although mostly as examples of recent publications instead of being analyzed in depth and compared to the COCO model outputs. A more thorough assessment of these new model simulations and how / why they agree / differ from the simulations presented in the manuscript must be done before publication.

- Similarly, the choice of comparing the COCO model outputs with those of one of the earliest models used for 231Pa and 230Th, namely the model from the Siddall et al., 2005 study, is very disappointing as it **misses out all the improvements made by the newer modeling studies and most of the conclusions drawn from these more**

recent simulations, most of which representing a significant improvement from the Siddall et al. (2005) model. The authors need to carefully and thoroughly justify their choice. Nevertheless, an in-depth discussion to compare their model outputs and conclusions with that of the more recent modeling studies is needed and should not be limited, as it is the case in the present manuscript, to a comparison with the Siddall at al. (2005) simulation.

- In the same vein, there is a **great lack of recent literature analysis on 231Pa and 230Th**, *e.g.* the recent review by Costa et al. (2020) or the recent findings of Missiaen et al. (2018) on the effect of the detrital (238U/232Th) activity ratio on the calculation of 231Pa and 230Thxs are neither discussed or cited. A lot of the effects that the authors are discussing in their manuscript is actually discussed in details for 230Th in the review paper by Costa et al. (2020).

- The literature used to discuss the effect of particles type and distribution is neither the first/pioneering papers on the topics nor the latest. The authors should read the review by Costa et al. (2020) and look at the modeling results of Missiaen et al. (2020b) and references therein. These results should be both mentioned in the state-of-the-art section of the Introduction and later discussed.

- Similarly, the older literature is fundamentally overlooked. The term "boundary scavenging" has been defined and used by Anderson et al. (1983b). Part of what the authors seem to define as a discovery on the effect of particle concentration on scavenging is actually perfectly defined and modeled by Anderson and co-authors is this paper and subsequent papers. This leads to a conceptual problem L356-369 (see also comment on L194 below).

- Several sentences or model presentation are very vague, e.g. in equation 4a, there is a term "Transport" (L116-120) defined as representing transport by advection, diffusion and convection. These are 3 very distinct physical processes in their formulation, why is the term "Transport" not explicitly given? What does the term "convection" represent in the oceans. There is no bottom heating so I have great troubles understanding what the authors mean here.

- I am very puzzled by the use of equation (10) (L169) for both 230Th and 231Pa. **The partition coefficient cannot be the same for both radionuclides** as they as have different behaviors. The value of the exponent used here (-0.42) has been given by Henderson et al. (1999) for 230Th and is indeed not valid for 231Pa. I do not see what can be achieved by using the same reference partition coefficient for both isotopes.

- L90: there is one class of settling velocity in the model presented here. There are two classes in van Hulten et al. (2018). Since the authors discuss the effect of the concentration of particles on scavenging, they should discuss the effect of having one vs. more classes of settling speed on their conclusions

- L194: The authors say they included bottom scavenging in benthic nepheloid layers. **This is a very important aspect of the model. However, how this is done is not explained.** More explanations of this very important aspect are necessary, especially considering the objective of the journal.

- Amongst the conclusions, some are included in the equation. The fact that 231Pa is more affected by advection is 1) the basis for using Pa/Th as a proxy for ocean circulation and has already been verified by several models, and 2) is somehow imbedded in the equations of scavenging.

- English should be proofread. The meaning of several sentences remains very ambiguous or unclear.

To conclude on these general comments: the model and its interpretations seems detached from what is already known on Pa/Th both in the water column and the sediment from both modeling and data studies. This manuscript shows a lack of thorough reading (state-of-the-art) of the most recent (last 10 years) literature on the subject and lacks discussion of these recent findings / conclusions. The choice of using one of the oldest model to compare these new simulation results is very odd and thus lacks a great part of the novelty added by more recent studies. There are also **several conceptual problems that need to be adressed**.

Specific comments

- L24-25: if one wants to cover the all date range, there are more recent papers than Bohm et al. (2015), e.g. Sufke et al., 2020 or Waelbroeck et al. 2018

- L30: there is also Henderson and Anderson 2003 review that gives a large range of residence times (see also Costa et al., 2020 for 230Th)

- L36: for the LGM/Holocene comparison, there are more appropriate references, such as Lippold et al., 2014 which is a modeling and compilation of Atlantic data for the LGM vs. Holocene.

- L44 and after: several references missing or not cited appropriately. Many of the references cited cover several aspects of the Pa/Th modeling rather than only a specific aspect as the citation format made by the authors suggests.

- L64: GEOTRACE database: cite

- L76: 43 vertical layers: are these of uniform or different heights. Be more precise.

- L81: how do you assess that you reached a steady state? explain

- L81: Explain why you choose 100 years average rather than another number

- L171: "reference concentration". It is very unclear to me, based on the information given here what is the reference concentration. More details should be given.

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