

## ***Interactive comment on “Carbon isotopes and Pa / Th response to forced circulation changes: a model perspective” by Lise Missiaen et al.***

### **Anonymous Referee #2**

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This is a report on the implementation of the Pa/Th sedimentary proxy in the ocean-climate model of intermediate complexity, iLOVECLIM, in addition to the previously included stable carbon and radiocarbon isotope ratios. The reconstruction of past circulation states has suggested substantial changes from that observed in the modern ocean, with potentially significant implications for past climate change. It is therefore important that model simulations can capture the observed sedimentary evidence and demonstrate the ocean physics that might be consistent with this evidence. In this case, the incorporation of multiple isotopic tracers with different distribution and influences adds a valuable layer of sophistication to such modeling efforts.

In addition to demonstrating the model's ability to reproduce the observed modern distributions of Pa/Th and carbon isotopes, the authors report on the results of what is

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now a relatively standard “hosing” experiment, wherein freshwater is imposed on the surface of the high latitude North Atlantic within the model domain, in order to weaken convection and the overturning circulation. Changes in subsurface water masses and the strength of the overturning have the result of redistributing the sedimentary Pa/Th and carbon isotopes, which the authors then interpret and compare to existing data. They identify different responses of in the respective tracers. One major finding is that in the hosing experiment, changes in both carbon isotopes lag Pa/Th by a few hundred years.

Overall, this study is an important step forward in terms of the state of the art of implementation of circulation proxies and should therefore be worth accepting for publication in *Climate of the Past*, following revisions that should address the following points.

A more careful data-model comparison is needed to validate the simulated Pa and Th, which is the main advance made in the model. The paper compares bottom water particulate Pa/Th with a core top compilation (Henderson et al., 1999). Other than that, the comparison with Pa/Th data is mostly qualitative. The authors acknowledge that they refrained from making more data/model comparisons because of the crudeness of the model (Page 8 Line 41 (P8L41)). However, it is still important to show those comparisons. Readers may gain information about the fidelity (or lack thereof) of the model to the modern observations, including regions where the model performs well and regions where it does not. This information will help make the audience more informed, and therefore increase the impact of the paper. For example, the paper mentions the compilations of sedimentary Pa/Th by Lippold et al. (2016) and Ng et al. (2018) (P9L6). How does the model compare with them graphically? How do the particulate and dissolved Pa and Th results compare with GEOTRACES observations? These could be addressed in a few brief passages.

Additionally, hypotheses are offered for why  $\delta^{13}\text{C}$  response leads Pa/Th (possibly biology and/or air-sea exchange slows down  $\delta^{13}\text{C}$  response), yet given the setup of the model, it would be a missed opportunity not to conduct a more detailed diagnosis of

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modeled causes for the lead-lag relationships among the various tracers. If the reasons can be pinned down, the paper can make a more robust conclusion, even if it is model-dependent. Is it possible to plot the biological changes before and after a hosing experiment? How about changes in the air-sea exchange rate? Depending on the results of those plots, the paper can then present a fuller picture of the changes during a hosing experiment.

Additional smaller points for consideration:

In P1L25 This is confusing. Should it read “without an a priori guess”?

In P1L33-34 These are not global changes and should be more narrowly defined, possibly as regional or even local.

In P1L36, it should be “see Lynch-Stieglitz (2017) for a review”.

In P1L40,  $\epsilon\text{Nd}$ , Cd/Ca, sortable silt are also valuable proxies to reconstruct circulation and water mass and they are worth mentioning.

In P2L25, this should be 12C, although in truth it is both, with a lower 13C/13C.

In P4L17, Equation 2, the second minus sign is different from the first. The multiplication dot is positioned as a punctuation would.

In P4L34, Equation 4, the “d” in “Kd” should be subscript.

In P5L14, it should be “compiled in Dutay et al. (2009)”.

In P6L13, this is a long and potentially confusing sentence, yet a valuable one for its description of how the proxies were evaluated. It would help to have a comma after “identify”, which might make it clearer that the identification is of simulation periods exceeding a defined length, for each proxy.

In P8L3-5, this is a bold statement that just does not ring true. Ten thousand years for equilibration of the carbon isotope signal in the Atlantic ocean, and a thousand for

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Pa/Th? This is in a basin where the residence time of the deep waters is a few hundred years today, and maybe a thousand years more in the past. Unless I misunderstand the point here, something is not right.

In P8L26, when listing the reasons that the millennial scale climate changes are not analogous to the hosing experiments, it would be useful to also point out that the location of the freshwater hosing in the model (the Nordic Seas) could also be different from events that originate primarily from the Laurentide ice sheet on North America, mostly likely including the millennial Dansgaard-Oeschger and Heinrich events.

P9L10, substitute “If” with “While”.

P9L32, modeled  $\delta^{13}\text{C}$  results are compared with another model’s results, yet this section is named “Comparison to proxy data”. Maybe update section title to “Comparison to proxy and modeled data.”

In Figure 2, the labels (e.g. A. Single response) are unnecessarily far from the plots. The caption should define the dotted black lines (which I assume is the 2 sigma variation of the control phase). Also in the caption, the dotted red vertical line is the response time and the dotted red horizontal line is the proxy response. The caption states it the other way.

In Figure 3, I think I’m missing something here. Why are there overlaps between the data coverage of single and dual response plots? Shouldn’t the two be mutually exclusive?

Lastly, a citation in your references has the wrong publication year. The citation for “Luo, Y., Francois, R. and Allen, S. E.: Sediment 231Pa/230Th as a recorder of the rate of the Atlantic meridional overturning circulation: insights from a 2-D model., Ocean Science Discussions, 6(4), 2755–2829, 2009.” should instead be Luo, Y., Francois, R., and Allen, S. E.: Sediment 231Pa/230Th as a recorder of the rate of the Atlantic meridional overturning circulation: insights from a 2-D model, Ocean Sci., 6, 381-400,

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<https://doi.org/10.5194/os-6-381-2010>, 2010.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-45>, 2019.

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