Here the authors provide a revised version of the manuscript (MS). The new version has overcome some points of criticism from the last round of reviews. However, there is still more work needed in order to increase consistency, readability and traceability of the manuscript, in my opinion.

General comments:

The readability of the MS could be improved by proofreading by a native-speaker. There are several passages in the text which are hard to understand, simply because grammar issues sometimes obscure the logic and line of argumentation behind the words.

I appreciate that the implementation of $^{231}Pa/^{230}Th$ into CESM and the provision of the source code is a reasonable step forward. Still I would have welcomed if the examined scenarios and parameter sets would have been more realistic in a sense that they can be actually used for testing (pale)oceanographic hypotheses (see comments below).

Specific comments:

Line 17: "p-coupled" and "p-fixed" are not generally known terms. They should not be used without explicit definition. Please consider rephrasing, e.g.: In addition to the fully coupled implementation of the scavenging behaviour of ^{231}Pa and ^{230}Th with the active marine ecosystem module (p-coupled), another form of ^{231}Pa and ^{230}Th scavenging have also been implemented with prescribed particle flux fields of the present climate (p-fixed).

Line 96: Please explain in more detail: how can the effects of circulation on $^{231}Pa/^{230}Th$ be separated from the effects of particle fluxes simply by using two different non-confirmed particle schemes?

Table 2: Please add p-fixed or p-coupled to the scenarios respectively.

Table 3: Is there a reason for the iterating and non-iterating grey layers? Some references appear twice.

Line 216: I do not agree the parameter set used by (Siddall et al., 2005) is a reasonable choice, only because "[…] the control experiment in Siddall et al., (2005) is able to simulate major features of 231Pa and 230Th distributions […]". Choosing the parameter set more carefully and based on more recent approaches may help yielding more realistic simulations. (Rempfer, Stocker, Joos, Lippold, & Jaccard, 2017) listed different experimental studies suggesting a more balanced choice on K values. With the upper limit K value used for opal by (Siddall et al., 2005) the particle effect are inevitably overestimated.

Line 230 and Fig. 9: a freshwater input of 1 Sv for 1.2 ka is way too high in order to simulate any past fresh-water flux (Carlson & Clark, 2012). If the authors want to show that AMOC and $^{231}Pa/^{230}Th$ are a function of fresh-water flux then their study is presented approx. 20 years too late. But if they want to improve our knowledge on the reaction of $^{231}Pa/^{230}Th$ on realistic fresh-water fluxes of the past, they should lower the fresh-water input. I think the authors miss an opportunity here.

They also miss an opportunity by not-implementing bottom-scavenging. There are new GEOTRACES data out, which suggest non-negligible effects from nepheloid layers on $^{231}Pa/^{230}Th$. I would expect that (at least for p-fixed) this would be very laborious.

Lien 242: I don't understand this sentence at all. There is a reference to statistical values in Fig. 4a which are not there.

Figure 5: I cannot follow the statistics provided here. The yellow points in 5a hardly lead to a slope close to 1.

Line 259: I think I cannot accept that the dissolved fractions are simulated so utterly bad, simply "[…] because boundary scavenging and sediment resuspensions are not included in our model […]". I suggest first that the authors re-examine the observational data. Which of the outliers (e.g. Fig. 5c) are reliable values with reasonable errors? Because what can we learn from a parameter set and model which is not able to reconstruct the magnitude of the particulate fraction. If this was already a problem in the studies by (Dutay, Lacan, Roy-Barman, & Bopp, 2009) and (Siddall et al., 2005), why not recalibrate the model? How did (Rempfer et al., 2017) cope with this problem?

Line 281: "The sediment $^{231}Pa/^{230}Th$ in CTRL is overall consistent with observations […]". Wouldn't it be interesting to go into more detail here? Where are they consistent? Which basin, which water depth? Is margin distance an issue? By carving out which region is worse represented than others a lot could be learned about and from the model. E.g. Southern Ocean: because opal fluxes are so high $^{231}Pa/^{230}Th$ can vary a lot (much more than in the Atlantic). Simulating correct absolute values is almost impossible because opal flux varies on very small spatial scales, which cannot be captured by any model. Thus, the quality of the model run assessed by observations from this area will inevitably lead to bad agreement.

Line296: Where is the statement given here shown/demonstrated? Figure?

Line 303, Fig.6: I cannot follow the argumentation here. It would be necessary to increase the scale on Fig. 6and b in order to better resolve the high values. At the moment any variations are hidden within the red colour. The finding, that K influences dissolved fractions but not particulate fractions needs much more explanation. The simplification with reference to Eq. 3 and 7 does not help much.

Table 2: More realistic values for EXP1 and 2 would be appreciated in order to derive helpful insights from the model runs.

Line 329: This statement should be proved statistically (like Fig. 5).

Line 360: In the following paragraph the effects of opal on $^{231}Pa/^{230}Th$ is discussed. However, the model generates opal fluxes not in agreement with reality. In the response to the reviewer the authors claim that the large scale global opal production is reflected well in the model (e.g. high in SO). I agree. They also claim that the question, why the models produces a "fake-bloom" of opal production in the Western North Atlantic, is beyond the scope of this study. I may accept this (but then one may questioning the validity of the model approach), however in this case the paragraph following line 360 needs to be written more carefully and with a clear statement, that opal is not well represented on smaller spatial scales. Same with line 409.

Line 419: Of course studies on AMOC reconstructions need to cross check opal fluxes, but this sentence spreads a way too negative message when based on unrealistic opal fluxes and hence I do not agree. Please rephrase.

Fig 9: the difference between coupled and fixed are partly so big, that I wonder how both methods did agree so well before. Differences in the range of Δ^{231}Pa/^{230}Th>0.1 (e.g. 9d) are not increasing my confidence in the model. Observations are much more constrained. Again I plead for applying realistic model parameters only. Further, I could not find information on water depth and longitude of the values shown in Fig. 9 diagrams, which are essential for the interpretation.

Line 424: Why is there a decrease of ^{231}Pa/^{230}Th above 2 km only? To my understanding and as stated in line 442 the decrease affects all of the NADW seized water depths.

Line 460: Yes, the parameters are somewhere in the range of the right magnitude, but not more. It would be great if this study would help to represent ^{231}Pa/^{230}Th in a realistic model, not only somewhere in the range of a factor of 25.

Fig10b: site locations are not visible.

Fig12c: Please explain the change of direction of ^{231}Pa/^{230}Th with depth at about 4000m for ON

Carlson, A., & Clark, P. 2012. Ice sheet sources of sea level rise and freshwater discharge during the last deglaciation. ***Reviews of Geophysics***, 50: RG4007.

Dutay, J., Lacan, F., Roy-Barman, M., & Bopp, L. 2009. Influence of particle size and type on ^{231}Pa and ^{230}Th simulation with a global coupled biogeochemical-ocean general circulation model: A first approach. ***Geochemistry Geophysics Geosystems***, 10(1).

Rempfer, J., Stocker, T. F., Joos, F., Lippold, J., & Jaccard, S. L. 2017. New insights into cycling of 231Pa and 230Th in the Atlantic Ocean. ***Earth and Planetary Science Letters***, 468: 27-37.

Siddall, M., Henderson, G., Edwards, N., Frank, M., Müller, S., Stocker, T., & Joos, F. 2005. ^{231}Pa/^{230}Th fractionation by ocean transport, biogenic particle flux and particle type. ***Earth and Planetary Science Letters***, 237: 135-155.