The authors nicely improved the manuscript in this revised version. They have answered most of my comments / questions / suggestions / concerns. I still have a few comments that needs to be answered and a few minor adjustments are needed before publication.

One of my concern is that the justification for choosing a comparison with the simulation(s) made by Siddall et al. (2005), i.e. rather old results, which have since been discussed and improved, does not clearly appear. The study will indeed gain from a justification of this choice that should be given at the beginning of the manuscript, i.e. end of the "Introduction" and/or "Experimental design".

As underlined by the other reviewer, the fact that the model is offline is not correctly emphasized. From my point of view, it is not a weakness but a strength from this model because it makes it indeed easier to manipulate.

Figures with "vertical profile averaged horizontally" (1, 2, 3, 4 and 5): I am not sure I understand this term correctly. An explanation must be provided. Does it mean you choose 1) to make an average of all the dissPa concentrations at a given depth along the entire transect? Or 2) to average the value along a given latitude for the entire basin for each layer?
Then what is the representativeness of this averaged value (orange points) in the Atlantic because: case 1) The north and south Atlantic have very different behaviors with 231Pa being strongly entrained in the AMOC In the north and 231Pa being strongly scavenged by opal-rich particles in the south. Case 2) the west and east basins have different behaviors for diss Pa (diss Th probably to a lesser extend).

L38-41. As you described the conclusions based on the data analyses (lines above), you should also describe the main conclusions of the 2 models you are citing in this subsection.

L45. "sinking particles scavenge 230Th more strongly ". I suggest to replace strongly by efficiently.

L45-47. While the work by Chase et al. is highly cited, there were much earlier studies showing the effect of opal such as Rutgers van der Loeff and Berger (Deep Sea Res. 1, 40, 1993) or Walter et al. (Earth Planet Sci Lett 149, 1997). Some of these studies are cited later in the manuscript but should also appear in the Introduction as this effect has been known for a long time.

L48-51. Citations should be proofread, e.g. Rempfer et al., van Hulten et al., Missiaen et al., 2020a are not 2D ocean models: please carefully check these references and to which model they correspond.

L63-65. While the approach was different from that of the authors' model, the effect of efficiency of scavenging depending on particle concentration has been explored by recent models, e.g. van Hulten et al. (2018); Missiaen et al. (2020a and 2020b), even if the approach was different. It should be mentioned here.

L124. I suggest that, for non-model specialists, you explain the term "convection" as you did in your answer to my initial review. This was a clear and short explanation that could make your paper more accessible to a broader audience.

L165. This part needs clarification.
You write that the benthic nepheloid layer is 50 to 130m above the bottom. Do you mean thickness? i.e. bottom to bottom+50m for 50m?

In addition, in your answer to reviewers, you say that the thickness of the nepheloid layer increases from 5 to 250m. There is a discrepancy with what is written in the manuscript. Please clarify this point.

L250-253. Since you are comparing the results of your simulations to that of other models for the Southern Ocean when discussing particulate Pa and Th (section 3.3) it would be good that have a similar comparison for the dissolved Pa and Th at the end of section 3.2. even if the other models indeed use slightly different approaches to simulate the effect of changing adsorption coefficient in the Southern Ocean (e.g. Rempfer et al., 2017 and Missiaen et al., 2020a).

L301. typo: Missiaen (not Messiaen)

L320. typo: GEOTRACES (not GAOTRACES)

L334-336. "In this transect, the observational data shows a clear signal associated with hydrothermal vents": please explain the underlying mechanism, i.e. how would hydrothermal vents affect both diss. and part. Pa and Th concentrations. A reference is also needed. May be also the earlier findings of this mechanism, e.g. Shimmield and Price, Geochim Cosmochim Acta 52, 1988.

L337. replace "our scope" by "the scope of this study"

L341. "we discuss about": remove "about"

L401. "as a matter of course" replace by "as a matter of fact"

L405. "231Pa transported toward the Southern Ocean is expected to be immediately removed there due to the high opal flux". This is an overstatement. Immediately should be replaced by "quickly" or "very quickly" or "quicker than in the open ocean". Both data and modeling studies show that there is still some Pa exported within the Southern Ocean

L408-410. "This result implies that scavenging of 230Th is not so efficient in the Southern Ocean as previously expected due to the dependence of scavenging efficiency on particle concentration.". In Missiaen et al. (2020a), they simulate the effect of halving of 1) the total particle flux, 2) the POC, 3) the CaCO3 and 4) opal. They show that for Th, it results in increasing the dissolved 230Th concentration in the Southern Ocean in case 1) and further show that the main effect comes from POC and opal. How does it compare with your results on scavenging efficiency and particle concentration? Can you link both results?

L453: also add the ref to Henderson and Anderson for residence time

L498. "(i.e., specifying the smaller $K^{\mathrm{Pa}}{}_{\mathrm{bottom}}$ than Figs. 2c and 2d)" do you actually mean: "(i.e., specifying smaller $K^{\mathrm{Pa}}{}_{\mathrm{bottom}}$ than in Figs. 2c and 2d)"?

L819. "Mangini" not "Mangianini"