Review of 'Sediment Pa-231/Th-230 as a recorder of the rate of th Atlantic meridional overturning circulation: Insights from a 2-D model', by Luo et al. submitted to *Ocean Science Discussion*

The authors have developed a simple model including a description of particle scavenging and of the meridional circulation in the Atlantic Ocean to develop understanding of the factors influencing the activity of thorium-230 and protactinium-231 in dissolved and particles forms in the water column as well as the ratio Pa-231/Th-230 in the sediment. The study is motivated by an assumption in some paleoceanographic studies that the ratio Pa-231/Th-230 in Atlantic sediments can be used as an indicator of the rate of the meridional overturning circulation (MOC) in the geologic past. The assumption has been challenged by a series of recent studies, which demonstrated the potential for the ratio Pa-231/Th-230 in Atlantic sediments to be influenced by past changes in the chemical composition of the scavenging particles (more spefically, biogenic opal) (see refs. in the ms.; Gil et al., Paleoceanography, 2009). Whereas the chemical composition of particles is known to have a major influence on the removal of both metals from the water column, such influence is often dismissed in paleoceanographic work if no evidence is found in the sediment for downcore changes in the flux of biogenic opal to the seafloor. The dismissal can never be fully justified, however, since biogenic opal reaching the seafloor may not be preserved in the sediment owing to dissolution. The present study is an attempt to provide new elements in the current debate by an exploration of the role of particle composition and circulation on the distributions of Th-230 and Pa-231 in the Atlantic Ocean.

Overall, I think this manuscript (ms. hereafter) should be a useful contribution to the current debate. In spite of the model limitations (see below), the calculations reported in the ms. provide evidence that the relationship (if any!) between the sediment Pa-231/Th-230 ratio at a particular location and the strength of the MOC in the Atlantic Ocean can be very complex. In my opinion, one of the most important findings of this study is in the statement on p. 2772: 'Clearly, it is impossible to constrain the history of changes in the AMOC from the evolution of Pa-231/Th-230 at one site, as was attempted by McManus et al. (2004)'. The finding is important, as the Pa-231/Th-230 record generated by these authors is often taken as the best evidence for changes in the MOC over the last deglaciation. Another strength of the ms. resides in the impressive set of 'new' measurements of Th-230 and Pa-231 for the Atlantic Ocean (Table 2). These measurements constitute a significant fraction of the current database for this basin. I think they were generated in the laboratory of R. François, who is a co-author of the ms. Whereas he made these data available to the community prior to their publication, it will be a very good thing to see them listed in a Table in a peer-reviewed journal, so his contribution could be properly acknowledged. The ms. is also generally well written.

Although I am generally enthusiastic about the ms., I think the authors should address several comments before their work be published. Major and minor comments are listed below.

Major Comments

1) In contrast to what is stated in the ms., the model being used is not a 'circulation model'. The velocity field which is used to carry Pa-231 and Th-230 is entirely prescribed (mostly from hydrographic data collected along a few transoceanic sections). Accordingly, this velocity field only satisfies (hopefully!) volume conservation, i.e., the net volume transport through each box of the model vanishes. It does not (very probably not) satisfy the dynamical laws for the ocean circulation, even not dynamical approximations of first order, such as geostrophy and hydrostasy. The approach adopted by the authors to constrain the velocity field is thus identical to that for box models, whereby the volume fluxes across the faces of the boxes are imposed, the only formal constraint being that the sum of the fluxes through each box should be zero. I would thus recommend that the authors remove the phrase 'circulation model' from their ms. (in the Abstract and everywhere else).

Unfortunately, the issue is more than semantic. The fact that the velocity field is not dynamically grounded imposes a serious limitation to what can be done with the present model. In a way, the velocity field of the model is merely a plausible kinematical description of the modern circulation based on a few hydrographic sections. However, calculations are reported in the ms., where the 'geometry' of the circulation is changed but not the rate of overturning or vice versa (sections 7.1.3 and 7.1.4). Whether the velocity field in each case is dynamically feasible is unknown since this field is not forced to satisfy the equations of motion (again, it merely satisfies volume conservation). The situation contrasts with numerical experiments of Pa-231 and Th-230 with models that are based on these equations, be they zonally averaged models (Marchal et al., Paleoceanography, 2000), three-dimensional models based on simplified dynamics (Henderson et al., DSR, 1999; Siddall et al., EPSL, 2005), or three-dimensional models based on the primitive equations (Dutay et al., EPSL, 2009). In all these studies, the simulations of Pa-231 and Th-230 derive from a velocity field which is known to be consistent with the dynamics assumed in the models (whether the dynamics is accurate is another issue). I think the authors should (i) provide more details about how the velocity field is actually constrained (e.g., no word is given about the vertical velocities which are typically not estimated from hydrographic sections); and (ii) acknowledge the various limitations of their model, in particular its 'circulation' component.

2) The distributions of Pa-231 and Th-230 simulated by the model are compared to measurements of Pa-231 and Th-230 in the Atlantic Ocean. Whereas the comparison is instructive, it should be quantified using statistics such as root mean square differences. Note that reporting such statistics for model-data differences would enrich, not weaken, the ms. In particular, it would avoid the subjectivity that is often involved in statements of 'agreement' of a model with data, it would allow one to determine the extent to which a simple model as the one employed here could replicate observations of Pa-231 and Th-230, and it would provide a benchmark for future quantitative comparisons of these observations with models.

3) In spite of the model limitations, I think the authors should attempt to identify the implications of their calculations for the ongoing program GEOTRACES, which aims at a

better understanding of the distribution and behavior of trace elements and their isotopes, such as Pa-231 and Th-230. Could the authors make explicit recommendations about the sampling strategy for this program? Which are regions which should be densely sampled if an accurate depiction of the large-scale meridional distribution of Pa-231 and Th-230 in the Atlantic Ocean is to be provided? Which horizontal spacing should be used between stations? Similarly, what should be the vertical spacing needed to properly represent the vertical distributions of Pa-231 and Th-230?

4) Finally, I think the ms. would benefit from a discussion of what is new here compared to earlier model studies of Pa-231 and Th-230 in the ocean. Significant efforts have been invested over the last decade in modeling Pa-231 and Th-230 in the ocean (Henderson et al., DSR, 1999; Marchal et al., Paleoceanography, 2000; Siddall et al., EPSL, 2005; Siddall et al., Paleoceanography, 2008: Dutay et al., G-cubed, 2009), however, only one of these studies is quoted in the ms. I think the authors should summarize the insights from these earlier studies and put their own results in perspective.

Minor Comments

1) Page 2759, equation (2): write 'ΔFlux/Δz'

2) Page 2760, line 2: write 'Two Sv of AABW are added ...'

3) Page 2760, line 22: write '... constant to a good approximation ...'

4) Page 2762, lines 21–22: It is suggested that the ocean circulation is the only possible contributor to the observed departures of Pa-231 and Th-230 from linear vertical distributions. Could enhanced scavenging near the bottom, where relatively high concentrations of suspended particles are observed (Brewer et al., EPSL, 1976) or inferred from optical measurements (Biscaye and Eittreim, Marine Geology, 1977) also contribute?

5) Page 2763, 2nd paragraph: Here is an attempt to rationalize why Pa-231 and Th-230 apparently increase southward in the deep Atlantic. Interestingly, the scenario is implying an effect of water mass composition: the amount of Pa-231 and Th-230 that desorb from sinking particles would depend on the amount of Pa-231 and Th-230 in the deep, southward flowing water. Thus, the ratio of Pa-231/Th-230 in the sediment would be influenced by the composition of the deep water – not only by how fast the water moves. The scenario seems to contradict an earlier claim in the ms. (p. 2757) that the ratio is a 'kinematic tracer' and so would depart from more conventional tracers in paleoceanography, such as C-13/C-12 and Cd/Ca of benthic foraminifera, which are typically regarded as water mass tracers. Could the authors clarify the apparent contradiction?

6) Page 2763, line 28: write '... The gradual relaxation has been described ...' (whether the relaxation is an accurate parameterization is unknown).

7) Page 2768, section 5.4: write '... Deep Western Boundary Current ...'. Last sentence: the concentrations of suspended particles seem to be particulary large near the western

boundary (Brewer et al., EPSL, 1976; Biscaye and Eittreim, Marine Geology, 1977), suggesting that enhanced particle scavenging near the western boundary could actually also play a role in the lower concentrations.

8) Page 2770, paragraph 1: The formula for the 'equilibrium' sediment Pa-231/Th-230 should be given.

9) Page 2772, section 7.1.1: It is stated that the sediment Pa-231/Th-230 'integrates the lateral export of Pa-213 over the entire water column' but that the integration should be 'weighted (not weighed as written) by the depth-dependent residence time of Pa-231 with respect to scavenging'. Could this be tested with the model?

10) Table 3: Whether these values come from observations or the model should be specified.

11) Figures 14 and 15: Are the units for lateral velocity correct? What do the red symbols mean?

12) Figure 17: Are the units for lateral velocity correct?