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Biogeosciences Discussions

Interactive comment on "Modelling the effect of boundary scavenging on Thorium and Protactinium profiles in the ocean" *by* M. Roy-Barman

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I have the advantage of having read the comments of the first referee. I agree with his comments.

Despite their limitations, models such as the one described by Roy-Barman are valuable as comparisons against results from GCMs. While GCMs have more complete physical dynamics, the latest GCM models that include scavenging of Th and Pa still lack realistic representations of particle composition, particle size distributions, particle dynamics, and continental sources of particles at ocean margins. There will be a need for simple models with analytical solutions at least until GCMs are more fully

developed, and perhaps beyond.

I have no major criticisms of this paper, so I will simply present my comments in the sequence of where they appear in the manuscript.

p. 7855 line 7: change "significantly" to "significant amounts of"

p. 7856, Section 2.1: The simple 2-box model neglects ventilation of deep layers by lateral exchange with polar regions. The implicit assumption that exchange with polar regions is negligible should be stated explicitly. It is likely that this assumption is valid for the North Pacific Ocean, but it would be worthwhile here to mention that deepwater ventilation is neglected, and discuss the possible magnitude of errors that might be introduced by neglecting it.

p. 7857, line 7: replace the comma with "and"

p. 7858, line 11: insert "vertical" into …represents the VERTICAL particle…

p. 7859, Section 2.4: There are a number of assumptions implicit in this model that are not strictly true. These assumptions should be mentioned and justified by discussing the errors that are likely to be introduced. For example: a) It is assumed in Eqn 3 that Km is not a function of depth. This is almost certainly not true, as degradation and dissolution of biogenic particles causes the abundance and composition of particles to vary with depth. b) It is assumed that the concentrations of dissolved Th are zero at the sea surface (C=0 at z=0). Observations show that this is not strictly true, due in part to vertical mixing, which is particularly rapid in the mixed layer. It is reasonable to make this assumption in order to keep the analytical solutions manageable, but the assumption should be stated and the magnitude of offset from measured profiles induced by this assumption should be discussed. c) It is assumed that vertical mixing has a negligible impact on vertical concentration profiles. If upward diffusion of radionuclides served as a significant source at any depth, then one would expect vertical mixing to create curvature in the vertical concentration profiles. The assumed negligibility of vertical diffusion should be stated. If possible, place limits on the significance of vertical mixing, although I am not certain if meaningful limits can be established. d) The deep box of the interior ocean is not well mixed (i.e., concentrations are not homogeneous throughout the box in the real ocean). Roy-Barman mentions this point later in the paper. I suggest that it be included here, along with a list of other significant assumptions that are implicit in the model.

In each case, I believe that the assumptions are legitimate. However, I believe that they should be stated in the paper and discussed briefly.

p. 7861, line 13: change “of” to “off”.

p. 7861, line 23: Change “that” to “than”

p. 7861, lines 20-25: Is the curvature in the radionuclide profiles due to the particle flux effect, described here, greater than curvature that would be introduced by including vertical mixing in the model, especially considering that vertical mixing coefficients vary with depth?

p. 7863, line 4: Change “follow” to “follows”

p. 7864, line 4 and Table 1: Here the text refers to literature data in Table 1, but Table 1 contains no literature citations. See comment below on Table 1.

p. 7864, lines 6-7: The relative magnitude of K(Pa) compared to K(Th) was described by Anderson et al., 1983, cited in this paper, before any of the papers by Nozaki.

p. 7865, line 14: change “profiles is” to “profiles are”

p. 7867, line 8: Change “larger” to “smaller”

p. 7867, line 18: Change “than” to “as”

p. 7868, line 7: Change “constrains” to “constraints”

p. 7868, line 13: Change “shelves” to “shelf”

p. 7868, Section 3.2.1: Nowhere in this section is Figure 3a cited.

p. 7869, line 7: Change “latter” to “later”

p. 7870, line 11: Change “to” to “too”

p. 7881, line 14: Capitalize Arctic Ocean

p. 7873, line 2: Change “model” to “models”

p. 7874, line 2: Change “enhance” to “enhanced”

p. 7874, line 12: Change “constrains” to “constraints”

p. 7874, line 28: Broecker, 2008, does not discuss the ballast effect.

Table 1: Clarify this table by adding footnotes to indicate which parameter values are from the literature (and cite the relevant sources) and which are derived in this paper by fitting the models to data.

Figure 2, Caption: In line 2, when referring to panels c to e, it should be panels d to e.

Interactive comment on Biogeosciences Discuss., 6, 7853, 2009.