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Interactive comment on “Sediment-water column fluxes of carbon, oxygen and nutrients in Bedford Basin, Nova Scotia, inferred from ^{224}Ra measurements” by W. J. Burt et al.

Anonymous Referee #2

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General Comments: Overall, I like this article and think it merits publication. The data presented is a nice set of measurements, and the article addresses an interesting topic, as it evaluates remineralization rates from plankton debris, based on making a mass balance for water column nutrient budgets. This approach avoids some of the complications of direct flux measurements and core incubations, although it has some complications of its own, due to non-steady state behavior. The article is clearly written, well illustrated, and should draw significant interest.

I have a few concerns about the details of the approach. Major ones follow: 1. As the authors demonstrate, the system shows non-steady state behavior. The data pre-

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sented follows a rapid flushing of the basin. With its short half-life, the ^{224}Ra tracer will return to a steady state distribution in a couple of weeks, and the repeated profiles indicate that it does. However, the time for nutrient and O_2 profiles to relax to steady state gradients is considerably longer. Relaxation time can be roughly estimated by the ratio of depth below sill (squared) to the eddy diffusivity (about $35^2/K = 50$ days for $K=2 \text{ cm}^2/\text{s}$). Thus, the increase in fluxes calculated represent relaxation toward what may be a steady state flux (although flux could be changing with time). Consequently, the calculation they present for carbon oxidation rate from the change in the water column composition, may be closer to the accurate flux than the rate they find as the average of their sampling times. They could easily solve the non-steady state problem by doing a numerical simulation (perhaps a multi-box diffusion model) to see if they can reproduce the time dependence of the observed profiles. In constructing this model, they might also evaluate whether the flux should largely come from the bottom, or if they need a water column source/sink term. I suspect that their interpretation that most of the flux comes from the bottom is correct, but this could be demonstrated more convincingly. 2. The authors have chosen a 1-D model because it is easily applied. However, they need to justify this (on page 9207 perhaps). They might do so through scaling arguments of horizontal eddy diffusivity (see Okubo, DSR, 1971) and basin dimension. Or they could assume that horizontal eddy diffusivity is fast and include a source term for ^{224}Ra in the water column (and they will also have to introduce an area factor in the diffusivity term). Or, they could use the numerical model they should construct as noted above in comment 1, with real basin geometry for each layer. They might consult an interesting paper by Colman and Armstrong (L&O 32, p577, 1987) and references therein.

Technical Comments: Other suggestions of lesser concern follow: 1. (p.9292, line26) While sediment compaction, or macrofaunal irrigation MIGHT play some role in introducing the Ra isotopes to the overlying water, the short scale distance for this isotope (probably $<1\text{cm}$, depending on adsorption coefficient) means that molecular diffusion will very likely dominate input. Thus, pore fluid is not "discharged". In my opinion, I would not refer to this as "SGD" (p.9203, line 23), as seems to be implied (but not actu-

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ally stated). 2. (p.9209, lines 1-3) Adding a bit more info about the fitting routine would be helpful. Was this done with a standard software package (Kaleidagraph, Matlab?), and was the equation first transformed into a log format (this can make a significant difference, and use of the general curve fit routine of Kaleidagraph is a great way to use the exponential form directly. Weighting by analytical uncertainty (as they did) is also likely to be an important factor. 3. In Table 1, there are a few more significant figures than needed. Also, I do not understand why the uncertainty for 3 Nov QRa is so large in comparison to those for K and Ao (also p. 9211, line 28). If the equation $A = A_o \cdot \exp(-az)$ was fit to the data, the fractional uncertainty for K is twice that for the parameter a (so a has uncertainty of about 30%). The fractional uncertainty in flux (calculated as $a \cdot A_o$) should then be about 40% when calculated by error propagation. Also, it might be nice to add a mean \pm sig in this table (excluding the outlier). 4. In looking at the data in Table 1, I do not see a significant time trend for QRa or K. I would suggest just using an average value for K, as 224Ra should have relaxed to near steady state. It would also be nice to know what the buoyancy gradient is for this basin, over the depth range sampled. This would make the data more readily compared to other settings (Sarmiento et al, 1976 for example), rather than the rather wide range noted for off the UK. 5. (p.9210, line 3) I am not familiar with the VINDTA, but it sounds like this technique is for coulometric TCO₂. How did you obtain Alkalinity? pH measurement? If so, presumably this was done before HgCl₂ step, as HgCl₂ affects alkalinity (and thus pH). Some clarification here would be nice. 6. (p. 9212, lines 7-11) Not sure the comparison of Ra flux in atoms/m² sec is quite equivalent to the Rn flux of Berelson et al. While the methodology is somewhat similar, as the authors note, the absolute values will depend on sediment characteristics (these were not described here, although a note about this would be useful).. 7. (p. 9213, line 19) "remineralization" would be a bit less confusing than "return flow". 8. A more precise way to get the O₂:CO₂:N:P stoichiometry would be to plot each of these parameters vs. O₂, rather than comparing the slopes of the calculated flux vs. time. That will remove the systematic variations due to uncertainty in eddy diffusivity for each profile set. It might also reveal whether there are changes

in the remineralization stoichiometry as the basin becomes less oxygenated, and denitrification in sediments becomes more likely. 9. It is of interest that the flushing of the basin is due to input of warmer (but more saline) water apparently as an annual event. I gather that it gradually becomes colder and fresher as the winter passes. A few comments about this would be nice to include in the discussion. 10. (p. 9215, line 16-18) It is not apparent from Table 1 that the flux of 224Ra changed at all with time, due to input of any sediment that is richer in 228-Th, so I would delete this comment. 11. (most of p. 9216). The nutrient stoichiometry will not reveal whether the reactions are taking place in the water column or in the sediments unless the denitrification is sufficiently large to make a big impact. This does not seem to be the case. This discussion needs to be modified a bit. It is also interesting to see the Alkalinity profiles. I do not know if the precision is good enough, but there appears to be a negative flux. In fact, oxic respiration converts particulate ammonium to nitric acid and should cause a slight decrease in alkalinity (about 0.15 moles per mole of TCO₂). In most settings, carbonate dissolution masks this, but maybe not here. Other reactions in sediments (oxidation of metals, sulfide) may remove alkalinity, but this is balanced by the alkalinity produced in forming them, so only the nitrogen reactions or carbonate reactions should play a role. On page 9217, a comment is made about possible sulfidic character of the sediments. Is there any info about sediment characteristics here? Any profiles of Corg with accumulation rates that could be used to calculate a DIC flux for comparison? 12. (p. 9218, line 9) Some rewording here would make more sense. "modest allochthonous inputs" do not "sustain" the oxic conditions, although they play a role in the O₂ balance. Ultimately, the oxic conditions are sustained by the replacement of basin water through episodic inflow and diffusive transport, at rates exceeding the oxygen demand from metabolizable POM through allochthonous and overlying water inputs. 13. Their suggestions about temporal variations in Corg input seem like an excellent way to explain the mismatch between the annual trap flux previously measured and the ~3x higher flux they will calculate with a non-steady state model. Or perhaps the trap measurements miss nepheloid transport during flushing.

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