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Biogeosciences Discussions

Interactive comment on "Submarine groundwater discharge to a small estuary estimated from radon and salinity measurements and a box model" *by* J. Crusius et al.

J. Crusius et al.

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Response to Reviewer #3.

1. The methods section seems a bit weak and should include the modeling effort with more explanation than is included in the appendix. I am not sure why the authors chose to put the box model equations in an appendix when they are critical to understanding the results. Response: The model description is beefed up and included in the main body of the text.

2. In addition, this section only describes work completed in June and July, 2004. The results and discussion describes some data from data collected in August, potentially using a different approach. Response: The August S data are included. There were no radon data from then.

3. The hydrologeologic background of the site is too brief! The authors refer to two other references (1 is in review and the other is a USGS report, not easily accessible) for the information. I do not find this adequate, this should be improved. Some information about the annual rainfall, hydraulic gradients, tidal fluctuations, etc. would go along way to helping the reader. In fact, there should be more information that supports the assumption that GW only discharges during near low tide. This assumption (see below) may or may not be valid, not enough information provided for the reader to decide. Response: The discussion of the hydrologic background has been expanded. There are no measurements of the hydraulic gradient near the pond, hence we cannot use such information to test the tidal height dependence of the discharge. We only have the seepage meter info. This new model version assumes discharge at all tidal heights. This model version assumes discharge at all tidal heights. The channel is now treated using 5 boxes. As discussed in the text, this eliminates the model requirement that discharge to the channel occur only at low tide. In a nutshell, with 5 boxes the advection of pond and Nauset water through each channel box remains the same but the gw inputs to each box are 20% of what they were in the original version. This means that flow has to get REALLY low in order for the gw discharge to manifest itself in the channel box near the pond. Thus, even with discharge occuring at all tidal heights, the really low S is only seen at low tide. You do see a gradually decreasing S during outflow in the model, as is often observed.

4. I would like to know if water is recharging the underlying sediments during high tide or is the hydraulic gradient sufficient to support groundwater discharge throughout the tidal cycle. If there is recharge, this needs to taken into account in the radon budget. Granted the definition of GW used would include this temporary storage of tidal water (I disagree with this definition of GW, it is too broad in my opinion). This tidal water would have a different Rn and salinity signature upon discharge than the groundwater measured in the direct push piezometers. There should be some discussion of this and some additional data justifying the assumptions. Response: We do not explicitly include the possibility that there is recharge in the radon budget. However, we discuss

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the possibility that discharge of high-S, low-Rn groundwater, in addition to the observed discharge of freshened gw, cannot be discounted.

5. The model seems to have several limitations. The authors acknowledge most, if not all, of the assumptions involved in the mass balance. However, there is little effort devoted to evaluating the errors associated with the assumptions. The authors do justify many of the assumptions, but I think there needs to be some additional work completed on S5 establishing a real error rather than an arbitrary 50% (primarily associated with the uncertainty of the GW radon). Certainlyt some estimates of error can be assessed from other parameters in the model and the assumptions made. Response: There's an error analysis now.

6. I am also not certain that all the assumptions used are valid. For instance, measurements of radon are only made in the channel and it is assumed that the outflow of radon from the pond is equal to the radon inflow from groundwater. However, there must be some corrections made for decay in the Pond, loss by atmospheric evasion, etc. I am not convinced that it is as simple as Gw in = Out. This is an oversimplification, even with the short residence time of the water. I am also curious about the accuracy of the residence time (1.5 days) since the system is slightly stratified. Does the tidal water mix with the deeper water of the Pond? If not, how does that effect the model? Response: Decay and gas exchange were incorporated into the model from the beginning (this was actually discussed in the first version). There was never an assumption that gw-in = gw-out. I hope this is clearer now.

7. I believe the stratification of the system needs to be addressed in the manuscript. It is currently not incorporated into the results or discussion at all. There is a statement that a 3 box model was constructed and there was very little difference (15%). However, there is no information about this box model and the parameters used in evaluating the GW. Response: The issue of stratification is better addressed now, I think. The very small difference between inflowing and outflowing S (as discussed in this version) suggests that the waters are pretty well mixed in the pond. I think stratification could

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affect S-balance-based discharge estimates to the pond, but should have a smaller effect on the radon-based estimate. Our modeling incorporating 2 boxes into Salt Pond, and reproducing the slight S gradient, did not lead to signficantly different discharge estimates. The work of Anderson and Stolzenbach (1985) used dye tracers to better understand mixing in the pond. They observed a lot of mixing of incoming water with deep water in the pond.

8. Diffusive flux was ignored and was shown later in the paper to be negligible. Although I agree that it will probably not be an important component, it seems lazy to just leave it out. The sampling program should have incorporated some measurements of Ra or porewater Rn in order to construct a complete box model. The assumption that that the diffusive flux is negligible is based on previous results from the Chesapeake Bay estuarine system. Without some information of the sediments in the Salt Pond, this assumption can not be justified. Are the environments really similar? Maybe they are, but it is not evident by the data presented. Response: We mention other studies where diffusive flux was assumed negligible. This seems to be the case everywhere there's lots of advection.

9. The authors assume that the radon activity of the Nauset Marsh waters is constant. There is no data to justify this constant activity. In fact, the activity in the channel dips below this activity on a few occasions during eth incoming tide (Fig. 2). There is also no mention of the Ra-226 activities in the different water bodies. The authors due state a Ra-226 activity of 1-2 Bq/m3 in the pond, but is this also accurate for the Nauset Marsh water? Again, the activity is low for Ra, but a complete mass balance should include this as a source of Rn due to ingrowth unless excess Rn activities are used. The discussion never really describes how the initial GW discharge value was obtained. Was an arbitrary value chosen until it fit the data? A table of the data used in the initial time step may be helpful. Some information on the time step used in constructing the model should also be included. Response: We still assume Rn in Nauset waters far from the pond are constant. But we create a box for marsh waters waters near the

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channel. This allows high-tide S and Rn in the channel to vary, as observed. This is discussed more in the text. Also, unfortunately, no Ra samples were collected from Nauset Marsh, and only one sample was collected within the pond (2 Bq/m3). More Ra sampling should have been done, but the Rn levels are so high that the lack of Ra data does not affect our Rn interpretations.

10. Inconsistent trends in salinity could not be explained. This suggests a problem in the salinity model that needs to be addressed. I don't think it is adequate to suggest that they remain unclear when the objective of the manuscript is to use the model to evaluate the groundwater component. Response: We do not have an explanation for the inconsistent trends in salinity in the channel. We do have a solid explanation for why S might decrease with outgoing tide (see point 9 for reviewer 1). As mentioned in the text, we took hourly salinity profiles within the channel during a 12-hour period. Only during one of these did we see any stratification in the channel, and then only saw a difference of 0.1 psu between surface and deep waters. So we don't think the inconsistent S data are due to stratification. We think it's reasonable to just admit that we don't understand all of the S data at this point. Perhaps someone will sort that out in future work. This work is too long as it is.

11. Rainfall event was ignored. It seems like the rainfall event offers an opportunity to help test and validate the model. It would seem to me that the rainfall event would provide water with very low salinity and Rn. I almost found it amusing that the authors were not willing to make some assumptions on the potential runoff, considering the number of assumptions made with the remainder of the model. Response: We still don't try to interpret the rainfall event. The rainfall measurements were from a site far from this field site, so we don't even know if it's a valid estimate for our site.

12. The flux of N to the study area was calculated. Assumptions of this flux should be included. The authors suggest this is a fairly high flux when compared to other eutrophic sites, Chesapeake Bay. Again, how representative is Chesapeake Bay to Salt Pond. It may be better to compare othe marsh environments. I believe Mandy

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Joye and others were doing some work in some Georgia marsh environments. Also, research from the everglades could be compared, just a few suggestions. Response: We compare our N discharge estimates to other sites on Cape Cod, which are more similar than Chesapeake Bay. We also state the assumptions that go into this estimate.

Almost all of the minor points were addressed, as well.

Interactive comment on Biogeosciences Discussions, 2, 1, 2005.