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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.

Anonymous Referee #3

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The topic of groundwater discharge to coastal waters is of global significance. This manuscript uses two independent tracers (salinity and radon) to evaluate the groundwater contribution to a small pond and channel that connects it to an open estuary. This article uses a box model/mass balance technique to evaluate the groundwater contribution. This approach has been used in previous research and in areas on Cape Cod. In this respect, this research is limited in its contribution. However, as the authors point out, the research provides additional evidence that these tracers offer a reliable method for quantifying groundwater into coastal water bodies. Although I do believe this article has significant merit and should eventually be published, I have several issues that I feel should be addressed in a final revision. I believe that this article is of widespread interest and is acceptable for publication with major revisions.

GENERAL COMMENTS In general, I felt the paper was well written. However, there are a few sections that need to be tightened up grammatically (run on sentences, redundant statements, e.g. pg 5 line 9-10 and line 18-19; pg 9 line 7-10; pg 11 line 17, pg. 13 line 15 (too list a few). 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. 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.

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. 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.

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

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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.

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? 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.

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.

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

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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.

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.

I am not sure I agree with the assumption that there could be no radon or salt storage in the Salt Pond. A few more sentences regarding this may clarify it, but I was not convinced. How would storage affect the model?

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.

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 Joye and others were doing some work in some Georgia marsh environments. Also, research from the everglades could be compared, just a few suggestions.

I have also included several minor editorial comments for your consideration:

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INTRO The 3rd method would probably be better represented as tracers; radio-tracers are just one group used. Salinity is mentioned in the following sentence.

Smith et al., 2003 not in reference list.

METHODS Pg 6 line 23: S and T measured in a variety of locations and times, this is TOO vague! Are you referring to the Salt Pond or the channel. If the Salt Pond, is the data presented anywhere?

I don't see any data of the vertical profiles of S, T, and DO from the salt pond. May be useful.

Very vague description of radium analysisĚwhere samples ashed prior to counting, how was the system calibrated, maybe there should be a reference to another method.

Seepage meters were only used in waters less than 1 m??? That seems a bit too limited considering the area and depth of this relatively small system. In depths much lower than 1 m, it would seem that the bags would be floating on the surfaceĚthat would probably result in poor results. Was that ever the case?

What is the surface area of the Salt Pond at high tide and is this change in aerial coverage incorporated into the model?

No discussion of August sampling.

I assume Nitrite is considered negligible? Was it measured in any samples to verify?

RESULTS The radon data was simply shifted by 20 minutes. What was the integration time of the Rn measurement? That is probably an important fact when making the shift. I don't think it is justified simply to get it to line up with the salinity results! Have other researchers that have utilized the RAD7 made a similar shift in the Rn data?

DISCUSSION I am sure that the other author (Dulaiova in Burnett and Dulaiova, 2003) would appreciate some credit, Pg 10 line 3 and 4.

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Is the Salt Pond valume also allowed to vary with tidal height? The change in volume of the Salt Pond is on the order of 30%, if I have my figures correct. That is a significant change in volume and would certainly have an impact on Pond Rn activities.

If currents do influence gas exchange, than the assumption that they do not change things in this setting is probably an oversimplification. The volume in the Salt Pond increases by 30% in ˜6 hours and this occurs through a narrow channel. My guess is that there are some significant tidal currents that probably need to be accounted.

If al of this groundwater enters the system during low tide, as suggested, it would seem that one could simply do a mass balance of the water to come up with a groundwater component, was this attempted? Wouldn't there also be a change in current velocity between ebb and flood tide?

The authors discuss the difference in radon activity between fresh and saline groundwater. They suggest that radon may have been removed from the saline water by advection, not sure I understand this. What about mixing between end members?

Little discussion of seepage meter results and how it could be incorporated into the box model.

N flux assumes no change in N within the sedimentsĚthis should be stated clearly.

FIGURES Figure 1 is a nice figure for the region, but it would be nice to have a close up of the study site that shows the different sampling sites. Also, coordinate in figure caption are mixed up.

Fig. 2, 5, 6, 7, and 8 are difficult to readĚthe graphs are too small.

Fig. 10, the mean saline value seems higher than the data suggests. This may just be a function of some low saline values with higher Rn that is not obvious in the figure. It may help to have the saline values as a different symbol.

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