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Biogeosciences Discussions

Interactive comment on "Resuspension and estuarine nutrient cycling: insights from the Neuse River Estuary" *by* D. R. Corbett

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Detailed Response to Comments

I found the reviewers' comments thorough and insightful. The reviewers primarily suggested minor grammatical and organizational changes and some clarification on several points. I agreed with most of the suggestions, have addressed them below, and could easily incorporate them into a revised manuscript. I have included a detailed list of responses to the reviewers. I believe I have addressed all of the points raised in the reviews and is suitable for publication in Continental Biogeosciences.

Response to Referee # 1 – Richard Peterson

Generally, the reviewer found the manuscript of good quality and ready for publication.

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There were several minor comments that I have addressed below:

1) The reviewer is correct that in many systems the ration of NH_4/NO_3 can provide some insight into the redox state. However, the NO_3+NO_2 concentrations in the porewaters were never greater than 1% of the NH_4 concentrations with little to no discernable pattern in the ratio...ultimately (and not surprisingly) the porewaters are reducing everywhere and throughout the year. This type of analysis may be more useful in the bottom waters, but I think the reader can see it based on figure 4. Although the ratio is not presented, it is evident that NH_4 dominates throughout the estuary, with more equal concentrations toward the mouth. 2) The observed amount of sediment resuspended at each site was based on total inventories of ^7Be (throughout the entire core that ^7Be was present), as well as independent SSC data collected during the same time period (see Giffin and Corbett, 2003). However, in order to calculate the depth to which the sediments were disturbed, we used an average bulk density of the top 5 cm. In addition, the average grain size of the top 5 cm was used when evaluating the critical erosion velocity in order to assess the annual occurrence of resuspension events based on winds and waves.

The reviewer does ask an important question regarding the potential variability of ^7Be in sediments. As this manuscript was focused on the nutrient delivery associated with resuspension, we spent little time on the processes of resuspension. This was pointed out by the other reviewers as well and it is clear we need to add some discussion on our calculations related to resuspension rather than simply referring to our previously published work. I have included an excerpt here from the Giffin and Corbett (2003) paper that addresses this possible variability in ^7Be:

“It has been noted that seasonal variation in ^7Be in the sediment inventories can result from differences in precipitation throughout the year (Canuel et al., 1990). Precipitation (Fig. 4) was collected at New Bern, NC, adjacent to the Neuse River Estuary. River discharge for the Neuse is also presented to show the relationship with precipitation. The daily precipitation data show a fairly uniform distribution throughout the year of the

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study with the notable exception of the low precipitation of the October to November timeframe. Comparison of this data to the 7Be total inventory (Table 2) shows no observable decline in 7Be during the late fall when precipitation was at its lowest. The reasons that no decline in 7Be was observed were most probably due to the half-life of 53.3 days being longer than this short dry spell and that dry deposition, as small as it may be, continued through this period. Further, a major source of 7Be to the estuary are sediments delivered through basin-wide runoff. The area of the basin is relatively large compared to that of the estuary, reducing the effects of short-term rainfall variations."

3) I agree with the reviewer completely…I think the benthic/pelagic coupling in this system is critical to N cycling. The manuscript does suggest the same, stating:

"… in some organic rich eutrophic estuaries, such as the Chesapeake, NH+4 is recycled directly back to the water column from the sediments without the occurrence of denitrification (Kemp et al.,1990). This increase in NH+4 recycling can be a consequence of greater mineralization of organic matter (Tuominen et al., 1999). It is also believed that anoxia in these estuarie may also reduce the effectiveness of denitrification (Nixon et al., 1996). Therefore, since internal loading can be as substantial as external riverine sources, these internal sources need to be considered in nutrient policy formulation."

With the possibility of deceased denitrification rates within the estuary (e.g., Chesapeake Bay), the internal recycling becomes much more important to supplying the water column with N.

4) As with any contour plot, there will be interpolated data between the actual data points. A time series plot such as this is not uncommon in order to try and provide time series data along a transect. I believe this is the best possible approach to presenting the data and could add a sentence in the caption cautioning the reader to not over interpret contours between sampling locations or time. I have attempted a similar plot

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that uses different size points or types of symbols at each location (no contours), but interpreting is much more difficult for the reader.

The reviewer had several minor technical comments. These are easy to correct and will be done in the revised manuscript. Specifically, we will add precipitation to Figure 3, typos will be corrected, and we will change the x-axis label in Figure 4. Regarding the flux calculation, we assumed a 24 hour period in order to make the comparison to the diffusive fluxes presented. The fact is, that resuspension event likely occurred over the course of just a couple hours. I have taken a more conservative approach here and spread that over 24 hours so I could make a direct comparison with diffusion.

Response to Referee # 2 As with Reviewer #1, all of these comments are fairly minor and easy to correct. In fact, most of the major comments seem to be associated with the reviewers own interest in the subject (wanting additional analysis beyond the scope of the manuscript)...I am happy that the manuscript was thought provoking and raises additional questions that could be addressed in subsequent work.

1) Although we have salinity for each site during all sampling events, as the reviewer states, it is really beyond the scope of the manuscript and some data (e.g., SO4) is not available.

2) I don't believe bioirrigation could be discounted, especially during periods of increased wave heights. However, I don't believe that discounts the importance of resuspension, only adds an additional internal source for nutrient delivery to the water column beyond diffusion that has not been considered. We do not have data to comment one direction or the other as it relates to hyporheic processes as it was beyond the scope of the project and manuscript.

3) Again, I cannot argue that porewaters are not entrained during periods of increased current speeds and wave activity. Most of the work on this process has been conducted in sandy sediments with ripples along the seabed. Although the process is not limited to sandy sediments, the sediments at these sampling sites in the NRE and PRE consists

almost entirely of muds (<63um) with little macro-topography (my personal observation while diving to collect cores).

4) I am certainly not discounting this process. In fact, it simply provides an additional means of efficiently moving nutrients from the sediments to the overlying water column at timescales much shorter than diffusion alone. This manuscript describes the potential importance of resuspension as one mechanism...certainly there could be more.

Again, all minor comments can easily be addressed in a revised manuscript.

Response to Referee # 3 Like the other reviewers, Reviewer #3 found the manuscript of sound scientific significance and quality and should be published. The reviewer had several minor comments which will be easy to address in a revised version. Specifically,

I think it is clear that a bit more information should be provided on the radio-isotopic methods and the approach to calculate resuspension. Currently I have referred to an earlier published paper. I could easily include something like:

Sediment cores at nine sites in the estuaries were collected at least bimonthly beginning May 2001 for a 12-month period to quantify short-lived nuclides to evaluate sediment advective processes. Samples from all sediment cores were analyzed for 234Th (t1/2 = 24.1 days) and 7Be (t1/2 = 53.3 days) by direct gamma counting. Gamma counting was conducted on one of two low-background, high-efficiency, high-purity Germanium detectors (Coaxial- and Well-type) coupled with a multi-channel analyzer. Sediment 7Be and 234Th inventories were calculated from downcore activities (Canuel et al., 1990) Variations in the inventories of short-lived nuclides between sampling periods provide evidence of no deposition, removal, or new deposition (see Giffin and Corbett, 2003).

The amount of sediment resuspended in a removal episode can be calculated using the radionuclide inventory lost in dpm cm-2 and assuming only the top 5 cm of the sediments are involved in the disturbance, allowing for an estimate of the surface activity (dpm g-1). This calculation gives the amount of sediment resuspended assuming the sediment can be transported. Using a simple relationship developed by Hjulstrom (Pipkin, 1994), where the grain size and current velocity are known, the critical erosion velocity can be evaluated. Grain size analysis showed primarily silts and fine sands at surface with downcore median diameters of 23–155 um in the Neuse and 19–74 um in the Pamlico with no fining sequences either vertically or laterally throughout the estuaries (Giffin and Corbett, 2003). The largest removal in the Neuse in August 2001 at NR-6 involved a 7Be inventory loss of 0.8 dpm cm-2 in the top several centimeters where the 7Be activity measured was 1.8 dpm g-1 or approximately 0.4 g cm-2. Grain size analysis at this site (NR-6) determined that sediments primarily consist of unconsolidated silt and very fine sand. Observed current calculations have shown that velocities of 20 cm s-1 are common, which on a Hjulstrom diagram are sufficient to initiate transport of these sediment sizes. Accounting for the bulk density of the sediment removed indicates that the removal involved the top 2.2 cm of sediment.

The reviewer is correct, the word sensors was used twice in describing the evaluation of resuspension events. The sensors referred to are turbidity sensors and an acoustic Doppler velocity meter. These two sensors provided invaluable data on near bottom current speeds and time-series suspended sediment concentration. This data in combination with meteorological data (wind speeds) and the time-series radio-isotopic inventory variations allowed for extrapolation of resuspension events over a greater period of time in the study area. Although the Giffin and Corbett (2203) paper is referred to, it certainly needs to be clarified in the revised manuscript.

The reviewer did recognize that we did not slice our cores under an N atmosphere. As noted in the methods, sediment samples were transferred quickly into centrifuge tubes with little to no air space and stored on ice. Samples were centrifuged and filtered within 8 hours of collection. Based on the methods, I cannot guarantee that porewaters were not exposed to ambient conditions for some minimal amount of time. However, this exposure to air (O2) would have only influenced the ratio of NH4+ to NO3-+NO2-. As

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indicated in the manuscript and these comments, the porewaters were dominated by NH_4^+ throughout the system, with NO_3^-+NO_2^-representing «1% of N in all porewater samples. Therefore, any change in redox due to a small exposure to air would have no influence on the results and conclusions of this study.

All remaining specific comments will easily be changed/addressed in a revised manuscript.

Interactive comment on Biogeosciences Discuss., 7, 2767, 2010.

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