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Discussions

Interactive comment on "Seasonal watershed-scale influences on nitrogen concentrations across the Upper Mississippi River Basin" *by* Michael L. Wine et al.

Anonymous Referee #1

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General Comments

One of the general conclusions of this manuscript, that TN concentrations in the UMRB tend to decline dramatically from spring to summer has been reported for nitrate-N in many individual watersheds in the region. In rivers draining agricultural watersheds, nitrate is frequently the dominant source of N at high flow in the spring. In summer and fall, nitrate concentrations typically decline because of less drainage from cropland and more in-stream denitrification. Thus the observed decline in TN is not surprising, although I am not aware of a publication that has demonstrated this for TN over a large region, or related the pattern to climate and watershed characteristics as in this

manuscript. The conclusion that an increase in wetland area and/or a reduction in cropland area would reduce N concentrations is also not controversial. I am not familiar with some of the statistical methods used, so I can't fully evaluate how appropriately they were applied or interpreted. But I have some concern about independence of observations over time at individual sites and between upstream and downstream sites.

The major weakness of the manuscript includes lack of attention to sources of N other than fertilizer, namely animal manure, point sources and mineralization of soil organic N. There is also a lack of attention to relevant literature on nitrate concentration dynamics in UMRB rivers. These weaknesses seem to contribute to some misunderstanding and misinterpretation of some of their results discussed below. Not including temperature as a predictor variable may also be a weakness, given the role of temperature in many N processes and given the large latitudinal differences in temperatures across the UMRB.

I found the paper somewhat difficult to follow in places, in part due to my unfamiliarity with some of the methods used, but also due to what seemed to be irrelevant and unnecessary commentary.

Specific Comments For some examples of studies that have shown large seasonal swings in nitrate-N concentrations that are consistent with the seasonal variation in TN concentration presented in the manuscript: see Lucey and Goolsby (1993), David et al. (1997), Mitchell et al.(2000), Keefer et al. (2010). In the pre-fertilizer era, Palmer (1903) commented on a diminution of nitrate concentrations in the Kankakee River in the summer months, which he attributed to uptake by aquatic vegetation and reduced drainage from agricultural fields. More recent studies identify denitrification occurring in stream, river, lake and reservoir sediments as being important factors during warm, low flow periods (see Royer et al., 2004; David et al., 2006; Alexander et al. 2009).

Lines 371-3 and Figure 8b.: The discussion and the following sentence in the Figure caption are incorrect: "Inverse relationships [between TN concentration and log of discharge] are observed in watersheds in which 50

The relationship illustrated in Figure 8a is very similar to what I have seen for nitrate in many rivers draining tile drained watersheds in Illinois and Iowa: low concentrations at low flow, increase with the log of flow, up to a moderately high discharge, above which there may be a flattening or a decrease of concentrations at very high flow, probably due to depletion of source N and/or increased surface runoff diluting high nitrate water from tile drains. High flow generally mobilizes more sediment and particulate N, which is likely to render the relationship with TN more linear than with nitrate-N. I wonder how their general results might be different if they conducted their modeling analysis only on the majority of watersheds with positive relationships between TN concentration and log of flow. This would likely exclude the watersheds with significant point source inputs and focus on the predominantly agricultural watersheds.

Figure 8c, illustrates no relationship between TN concentration and flow and is from the Rock River at Afton, which seems to be a mixed use watershed, with considerable agriculture, natural areas, lakes and urban areas. Lakes, of course, tend to act as an N sink, like wetlands. When data from individual rivers is presented, I think some identifying information would be helpful.

They cite Cao et al 2018 on timing of N fertilizer application, but these estimates are highly speculative, based in part of University recommendations, which are not necessarily adopted. Good data on fertilizer application timing is very limited. Actual fertilizer timing is likely to vary by location and year (see Gentry et al. 2014 for one example). In addition to fertilizer, animal manures are applied, and mineralization of soil organic N increases as soils warm up in the spring, which are not discussed in this paper. The manuscript frequently attributes high river TN concentrations to recent fertilizer applications, which may be a factor in some settings, but N concentration at any time is likely to be from a variety of sources and ages. The highest N concentrations typically occur in a wet spring following a drought during the previous growing season that depressed corn yields and N fertilizer uptake (see Loecke et al. 2017). Consequently, much of the

elevated N in such a spring is not necessarily from current year fertilization, but may be from the previous year as well as mineralization of soil organic N (Gentry et al. 2009).

On their counterfactual modeling: it would be informative to specify the number of hectares or the percentage of cropland converted to wetlands or other land uses. The conversion appears to be rather extensive and if so, they are extrapolating well beyond the data used to develop the model, resulting in highly uncertain projections. Furthermore, wetland denitrification is influenced by temperature, and that is not considered in their model. Fortunately, the manuscript does not devote much attention to the quantitative model predictions, but to the extent that it does, perhaps a few words about extrapolation and uncertainty are in order.

Interpreting their results for impact on N loads is difficult because concentration reductions do not directly translate to load reductions(Royer et al. 2006). It would be difficult to estimate loads at all the sites in the dataset, with some of the sites having as few as 2 samples per year on average. The manuscript seems unnecessarily long, in part, because a considerable amount of irrelevant, and sometimes incorrect, background is presented in the introductory and methods paragraphs. The analysis focuses on total N, but much of the literature review discusses “nutrients” (N and P) rather than focusing on N. On line 94, they state that the Mississippi River is the longest river in the US, which is incorrect and irrelevant.

On lines 95-6 they state that the UMRB is the largest contributor of “residual” N to the Gulf of Mexico. I am not sure what is meant by “residual”. UMRB typically has higher N yields than other parts of the MRB, but the Ohio River typically carries a higher load. URMB loads are less than half of the overall loads to the Gulf of Mexico, so ranking URMB as the highest depends on how other portions of the MRB are divided up.

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