Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-562-AC2, 2021

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Hydrologic regimes drive nutrient export behavior in human impacted watersheds" *by* Galen Gorski and Margaret A. Zimmer

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Received and published: 20 January 2021

REVIEW #2 In this study, the authors examine nitrate c-Q relationships based on high-frequency data across 5 agricultural watersheds in Iowa. They separate their data into baseflow and stormflow by applying a set of objective criteria, although they describe that some subjective decisions are necessary to finalize the data separation process. The authors focus their analysis on seasonal patterns of variation. Intensity of artificial drainage is an important explanatory variable across the watersheds, as concluded by many past studies. Overall, this is a good study that provides some new insights to nitrate behavior in agricultural catchments, but mainly reinforces conclusions from

previous investigations.

Response: While we agree that some of the manuscript's main conclusions are supported by other studies, to our knowledge there have been relatively few studies systematically examining event scale nutrient export dynamics throughout several watersheds across a broad spatial area. Existing studies have mainly focused on detailed event-based studies in a single watershed or several small watersheds or large-scale nutrient export dynamics in which the hydrograph is partitioned in a more generic way (eg. flow percentile). We have added language in the introduction and the abstract to highlight this novelty.

The paper is well written, the methods technically sound, and the authors demonstrate good awareness of previous related papers. The limitations of a c-Q study such as this is that it's not always clear to what extent the static vs. dynamic patterns are driven by c vs. Q. In other words, much of the reason why baseflow is more chemodynamic may be that Q varies less, so that any variation in c is amplified. So, it would be helpful for the authors to comment on the relative c vs. Q roles regarding their interpretation of seasonal differences, and inter-watershed comparisons, especially relative to comparing stormflow to baseflow. I am not necessarily questioning their broad interpretations of the drivers of c-Q patterns, just asking for some additional insight as to whether c or Q are driving some of the differences described in the paper.

Response: Thank you for this observation. We have added text in section 3.4 that addresses the difference between c and Q variability during baseflow and stormflow. We have also added an additional supplemental table (attached). The table shows calculations of the coefficient of variation (CV) for c and Q during baseflow and stormflow periods. Individual values of CVQ and CVc show that baseflow c-Q chemodynamic behavior is driven by both a decrease in Q variation and an increase in c variation compared to stormflow periods. The ratio of CVc:CVQ is higher during baseflow, consistent with variable sourcing of nitrate. During stormflow periods, CVc:CVQ values are lower, indicating little change in c relative to Q. These patterns are more pronounced for the watersheds with the least amount of drainage infrastructure (UPN and MJF) than for those with more drainage. This is also consistent with our explanation for the c-Q behavior. Additionally, there is considerable overlap in c and Q values between stormflow and baseflow as shown by Figure S3. Given that baseflow and stormflow c-Q patterns differ, this suggests that partitioning of the hydrograph by events may sample different hydrologic regimes with similar discharges.

Beyond this criticism, the study provides limited insight to inter-annual variation. Studies of stream nitrate in the agricultural Midwest have highlighted strong year-to-year variation in c-Q patterns. For example, Jones et al., 2017 (cited by the authors) and Davis et al., 2014 (JEQ, 43: 1494-1503) provide examples of the strong role of dry periods followed by re-wetting. The authors do discuss relative wet-dry conditions on a seasonal basis, but the study provides little perspective on inter-annual patterns and the role these may have played in the study results. At least some quantitative insight would be helpful especially at it may have affected the baseflow vs. stormflow conclusions.

Response: We agree that year-to-year variability has been shown to have a significant effect on nutrient mobilization in these systems, however due to the length of the in-situ nitrate measurement record, this dataset cannot yet address inter-annual variability. We have added some language in section 3.5 addressing how inter-annual variability may impact the trends that we see.

Below are some specifics comments and criticisms referred to line number, and these range from minor editorial suggestions to more substantive comments:

Title – should change “nutrient” to “nitrate” in title since nitrate is the only nutrient that was analyzed

Response: We agree, and we have changed the title

Line 48 – suggest adding “excess” before applied

Response: The word has been added

Line 194 – rather than refer to Student’s t-test whenever paired comparison results are discussed, would be better to state this approach in the methods section and then just describe whether results are significant or not given criteria described in methods.

Response: The results of the t-tests have been removed from the manuscript because they added little value as noted by Reviewer #1.

Line 205 – in the paragraph that begins on this line, there is discussion of correlations, but no reference to quantitative values. It would be best to define how correlations were determinedâĂŤ-Pearson Product Moment or another approach? Also, should provide p value to reinforce terms such as “well correlated”. Was this done just visually, or were tests performed and significance determined?

Response: Quantitative values have been added to this paragraph. Language has been added to the methods section to explain how we calculated correlation, and a graphical symbol for significance has been added to Figure S4 (bold indicates $p < 0.01$).

Line 291 – the authors mention the possibility of biofouling affecting nitrate concentrations during one baseflow period. Was this based on evidence from a technician that serviced the site or was it based on anomalous values or rapid unexplained shifts? Would be good to provide basis for this statement. And this does raise the question as to whether biofouling may have affected other periods of observation.

Response: The basis for this statement is a rapid decrease in [NO3] and consistent values well below the rest of the record($\leq$ 0.1 percentile). This does not appear to occur, at least for an extended period of time, throughout the rest of the records or in the other watersheds, which is why we have not identified it elsewhere. We have added language to justify our conjecture.

Line 323 – higher export than what? Comparative here is incomplete.

Response: We have added language to make the comparison more clear

Line 375 – change “that” to “than”

Response: We have changed the word

Figure 3 – the color contrasts in the figure panels are not as strong as those shown in the color key. For example, the blue and green patterns for fall and winter show poor contrast in the figure. This is also true for spring in 3a and summer in 3b.

Response: We have updated the figure to make the color contrast more apparent to the reader.

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	Stormflow			Baseflow		
Watershed	CV_c[a]	CV_Q	CV_c/CV_Q	CV_c	CV_Q	CV_c/CV_Q
UPN	0.412	1.058	0.390	0.506	0.603	0.840
USC	0.321	0.914	0.352	0.325	0.868	0.374
MRF	0.369	0.842	0.438	0.370	0.558	0.664
MJF	0.337	0.870	0.387	0.382	0.744	0.513
DVM	0.401	0.886	0.452	0.444	0.785	0.565

[a] Coefficient of variation = standard deviation/mean

Fig. 1.