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Hydrology and Earth System Sciences
Discussions

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

**Anonymous Referee #1**

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In this paper, Gorski and Zimmer examine how examine nitrate dynamics in a series of nested basins in agricultural regions of Iowa. Specifically, they focus on the concentration-discharge (c-Q) relationships of nitrate in these rivers, using these relationships to make inferences about the source areas of nitrate delivered to the stream and the amount of in- and near-stream uptake that may modulate these c-Q relationships. In doing so, they divide the periods of analyses into stormflow and baseflow periods, predicated on recent work that shows that different c-Q relationships obtain in many rivers at high vs. low discharge. They find that stormflow periods were largely chemostatic (circum-zero c-Q slopes) while baseflow periods shifted between being chemodynamic and chemostatic depending on the season. They also found that in-



creased ag infrastructure drove a more chemostatic basflow response. They make several interesting and plausible hypotheses as to why this might be the case.

This is a good manuscript can be published with minor modifications. The paper provides an interesting case study of nitrate c-Q dynamics in a region that is very important for the Mississippi N budget. It is well-written and demonstrates a strong command of the extant literature. I would make the following general suggestions for improving an already-good manuscript.

- There are a LOT of comparisons made in this paper, with baseflow vs stormflow, seasons, and different metrics of land use all considered. All these comparisons, at some point, make it difficult to extract the main message from each section. I would encourage some revision to make these main points clearer.

- The authors put a lot of stock in which variable (e.g. drainage density) was the most correlated with a particular c-Q metric, and they provide some interesting hypotheses/speculation about the ultimate causes of these correlations. They sometimes neglect other variables that were nearly as well correlated as the one they were focusing on, however, and they neglect to offer alternative hypotheses that might be as plausible. Given that there are only 5 watersheds in the study, there is likely a large degree of chance that determines which variables are most correlated with c-Q dynamics, especially if there is a high degree of correlation between predictors, which I imagine there is. I think they would do well to acknowledge some of these alternative hypotheses. But...

- With the exception of drainage density, the amount of variability in LULC, both total and within set buffers, is very low, and I would urge the authors to consider whether it is plausible that such small differences might plausibly drive the differences in c-Q patterns that they observed. I don't know one way or the other, but I think it's something to consider cerefully.

Line comments:

52 My opinion: this puts too much stock in the ability of c-Q relationships to identify source areas. I think that c-Q relationships can help develop hypotheses about sources, but attributing a NO3 flux to a source area by c-Q relationship alone is a very tenous thread.

110 spelling "Glycine max"

115 It is my understanding that this area was drained in the late 1800s-early 1900s. https://www.desmoinesregister.com/story/opinion/columnists/2015/03/01/story-pioneer-iowa-wetland-farmland/24212331/

132 Is this volume-weighted mean concentration or just streight mean?

135 spelling "Land Cover"

143 Why this absolute slope criterion rather than relative (either % flow change or criterion based on area-normalized Q?) Wont absolute criterion mean more storm detection in large rivers? 143 I know USGS uses cfs, but please metric-ize discharge measurements, especially since you also use L volumes in the paper. Also 1e-4cfs/s is a nonintuitive quantity (is that like a tablespoon?) and mismatched to timescale of the data. can you put in mˆ3dˆ-2 units instead?

194 I would remove these statistical tests for a number of reasons: 1) It's inappropriate to model count data using a t-test because a t distribution is continuous 2) you're treating watersheds as independent observations in the t-test, but there is an obvious correlation structure given that the watersheds are nested. 3) I don't think the statustics are really necessary just to say "the count of spring and summer events and their precip totals were similar."

200 "+/- 3.07" Please state once what this variability estimate is (sd? I assume not SEM)

209 "groundwater flow paths with longer residence times, and more streambed-water interaction," The relevance of these factors may not be immediately apparent to a



reader.

205-215 - Suggest reworking this paragraph a bit. The central thrust of the argument isnt entirely clear.

221 - Another good cite for conceptual framework considering this idea: Wollheim et al. 2018 Biogeochemistry

279-294 - This might suggest that nonlinear power law fitting, instead of a linear fit in log-log space, might be a more robust approach to dealing with low Q anomalies, because in these periods, absolute residuals are small but log-residuals are huge.

304 - mˆ2 unit should be kmˆ2

344 - I'm not sure I buy this explanation... given that you have multiple other predictors that are just sliiiightly less correlated than 100-m buffer crop % I think it's a stretch to focus on just one explanation. Could also be driven by drainage density, for instance.

Supplement:

Figure S1: it looks like there is some linear interpolation going on over dates where there's no Q data e.g. MJF Dec 2017-Feb 2018. Please show gaps in Q data instead.

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