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Comment on egusphere-2022-431

Anonymous Referee #2

Referee comment on "Droughts can reduce the nitrogen retention capacity of catchments" by Carolin Winter et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-431-RC2, 2022

Referee report on: Droughts can reduce the nitrogen retention capacity of catchments

By Carolin Winter, Tam V. Nguyen, Andreas Musolff, Stefanie R. Lutz, Michael Rode, Rohini Kumar, Jan H. Fleckenstein

I agree with the first referee that this is a nicely written manuscript with clear hypotheses and a good concept to analyse altered N dynamics under drought conditions within a mesoscale catchment. In particular, I like the strategy to combine a data-driven and model-based analysis. Although I am no native speaker, I think that language issues are largely settled, which is why a reviewer can concentrate on the most important issues: science and novelty behind the presented analysis. So I congratulate all authors for this work.

This said, I still have some major points that I think should be addressed prior to publication in egushere.

1. Information about N-Inputs

You state that exported N loads have generally decreased (e.g. L 268). What about the input? I propose to present the entire N-balance for the different subcatchments instead of only four components in Table 2. Moreover, a diagram would be more intuitive than a table.

2. Stability of catchment properties

This is a general drawback: If you compare N-retention–discharge relations during the recent drought to longterm values, you assume that catchment properties stayed the same. Can you really do this? There might be landuse changes (forest diebacks after storm events, or due to preceding droughts, e.g. 2003) and there is for sure a rising trend in temperatures. This trend per se alters the N-cycle, prolongs vegetation periods and presumably intensifies processes. I would like to see this point in the discussion section.

3. Scenario of forest dieback

This point was already raised by referee #1: Omit the simplistic scenario of forest dieback. You only simulate reduce N uptakes but there are surely more effects on the N cycle here: e.g. mineralisation of dead organic material, altered soil characteristics, etc.

4. Increased N-mineralization during droughts

You speculate that mineralization during droughts might increase, based on a single study that found increased rates of depolymerisation during droughts in a montane grassland in Austria. I would be more careful when transferring these findings to the forested Selke catchment. E.g. depolymerisation in montane grasslands might principally be temperature dependent. In forest soils, I would rather argue that mineralization is hampered by soil moisture deficits during droughts and subsequently reduced microbial activity. Your data only shows increased mineralization during the entire dry-wet cycle which could be due to onset of strong mineralization during re-wetting. Then mineralization could be more intense, because you have an organic N-pool accumulated during the drought.

5. Groundwater data to illustrate longer term N-effects

You hypothesize that in the upper Selke short TTs lead to visible effects of N-retention rather than in the low Selke, where longer TTs might cause longer term effects. This is a logic outcome of your transit time model. I think this could nicely be proved by groundwater data. Do you have well or spring data that could be used to support your hypothesis here? Recent papers have looked on groundwater nitrate responses after droughts and e.g. found instantaneous or delayed reactions depending on aquifer types.

6. Shape of the Retention-Discharge-Relationship

I disagree with the shape of the function between retention capacity and log-scaled discharge, as presented in Figure 4 and in the conceptual framework in Figure 5. A linear relationship is not meaningful here, maximum retention is 1 (at zero discharge), and your figure implies higher values. The data of HD shows this shape shape quite nicely. So put in the upper limit and an asymptotical approach of the function to this value when it comes to very low Q.

Minor issues (line by line):

L99: Check throughout the manuscript if you introduce abbreviations, I did not find this for TTs

L103 (Fig. 1): Colourscale for discharge anomaly could be clearer

L128: you speak about multi-year drought, although you only analysed the first two years of this event. I agree that the drought lasted longer, in some areas this event is somehow present up to now. But in your analysis this is a two-year event..

L204: these numbers refer to the subcatchments? This is not clear..

L292: I understand that more details of the mHM-SAS-model are given in the supporting material. But still I would like to have a couple of sentences also in the main document that explain what a/b ratios and SAS functions are and why they are indicative of water age.

L297: a median of a median, really?

L303: you present a conceptual drawback of the model: it cannot handle TTs that are larger than the simulation period. Nevertheless you model fits are quite nice on the entire time series from the very begin of grab sampling. Does this mean that your model is right for the wrong reasons and that you overcome structural deficiencies by calibration? I think this should be a point for discussion.

L339: I think the post-drought nitrate pulses in soils do not only propagate through catchments and find their way to rivers, but that runoff generation processes are altered, too. This has been documented before, i.e. a change of HOF/SOF to more SSF in a forest catchment after drought (https://doi.org/10.1016/j.jhydrol.2012.07.010).

L351, L398: You claim that upper Selke is “very low in nitrate” during droughts. What is “very low”? I think in forest catchments a value of 1 mg/l Nitrate-N is not exceptional. Also here you should compare with other studies in forest streams.

L467: N uptake by denitrification?