



EGUsphere, referee comment RC1
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Comment on egusphere-2022-431

Anonymous Referee #1

Referee comment on "Droughts can reduce the nitrogen retention capacity of catchments" by Carolin Winter et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-431-RC1>, 2022

This manuscript combines data-driven analyses and modeling approaches to understand how catchment and drought characteristics impact annual N export, transit times, and overall catchment N retention across a set of nested sub-catchments in Germany. The manuscript is very well-written and easy to read, and the authors present their ideas and results in a clear and compelling way. The authors combine of data-driven C-Q analyses with catchment modeling which is a unique approach.

Overall, this paper has the potential to be a strong contribution to the literature, but additional information/considerations are needed. Below are some general suggestions, as well as line by line comments.

General comments:

This paper explores how antecedent drought impacts N export and retention. There has been a fair bit of work on this in the US – in particular Davis et al. 2014 (Journal of Environmental Quality) and Loecke et al. 2017 (Biogeochemistry). The idea of “weather whiplash” may be a useful reference and motivation for this work.

The manuscript relies on a previously calibrated model (mHM-SAS, Nguyen et al 2022) for a fair bit of the analysis. While it is beyond the scope of this manuscript to include/replicate all the calibration information in the original modeling paper, the authors need to provide enough detail for readers of this paper to understand the analysis presented in this paper. For example, SAS functions were not introduced until line 202, and there is no description of what they are or how they are defined, and how SAS functions relate to nitrate generally. The a/b ratio (line 292) was never defined or explained, despite it being a central figure in the paper. It seems important to have at least some content explaining the model/SAS functions (and how they relate to nitrate

cycling) in the introduction, and then substantially more context in the methods, perhaps even results. For example, the authors mention briefly the relatively low NSE efficiency for the lower basin model in the methods section (line 204) but never expand in results or discussion.

One central component of the analysis is the catchment vs soil N retention capacity. The catchment N retention metric is using a simple mass balance approach. However the soil N retention capacity is a simulated parameter from the mHM-SAS model. After digging back into the original Nguyen 2021 manuscript (which appears to be missing from the reference list), it doesn't seem like this parameter has been validated in any way. Were there any measurements of root zone leachate made to validate this particular parameter estimate? At the very least, there is likely uncertainty in this parameter estimate, but this is not propagated into the Nret-soil metrics. There are numerous fates of N in the soil, and this seems like it has the potential to be highly uncertain. It would be helpful if the authors could provide more nuance here, as this is potentially an over-simplification.

Finally, the forest change scenarios seemed like side component of the manuscript that wasn't fully explained, explored, or integrated into the main narrative of the manuscript. The scenarios used were extreme (100% vegetation loss?), and overall it came across as somewhat of a diversion from the main story line which was so focused on drought. I don't think this is a necessary part of the manuscript.

Line by line comments:

Line 100: is the agriculture in the lower catchment irrigated? If so, how would this impact the findings discussed throughout the rest of the manuscript?

Line 115: I think this description could be clarified/simplified. eg - 1 year = 1 drying/wetting cycle. the year starts in May. Calling it a drying-rewetting cycle instead of year is clunky.

Line 160: This is results, not methods.

Line 195: I think this section needs to be expanded to provide more context about the modeling approach here. For example – what are SAS functions? Why is the NSE so low for the lower catchment?

Line 202: have the authors considered whether/how a model calibrated under moderate climate conditions will perform under extreme climate conditions? Might there be specific hydrologic processes that are more sensitive to drought and therefore do not simulate well

during a period with different climate conditions than the calibration period?

Line 220: This sensitivity analysis is good - but do you think that N inputs might systematically have changed during the drought? How might applying the long-term N inputs to the drought period impact your conclusions?

Line 231: instead – word choice?

Line 273: More information on model fits needed (found it in line 209, but it could use a brief description in the results)

Line 292: a/b ratio has not been defined or described; substantially more context is needed here.

Line 295: It is surprising to that the median TTs are dominated by old water in the lower catchment, which is dominated by agriculture. Typically agricultural land use encourages rapid movement of water through the surface/subsurface (via tile drainage, etc), which might lead to younger water fractions. Can you provide more discussion as to why the median TTs are so long here, particularly given the land use? It also would be helpful to have more information in the site description about irrigation practices in the agricultural portion of the watershed, as this might impact TTs and flushing of N.

Line 324: this applies to throughout the manuscript, but the language of upper Selke could be clarified by in parentheses including the site names (SH, MD). It's hard to remember which sites are upper vs lower based on the initials (which is what is labeled on all the figures).

Line 336-337: This is an incomplete sentence

Line 337: Were the concentrations during the drought exceptionally high? They appear to plot on top of the existing historical data, albeit on the high end – but this makes it sound like the concentrations were much higher than what was previously observed.

Line 455: this is a really interesting point! Very cool!

Line 343: Overall this appears to be true, but it also warrants some more nuanced

discussion/context – similarly high concentrations were observed at other times in the historical record. Were those droughts as well?

Line 358: Can you provide more explanation here? Why is the potential for denitrification in groundwater lower in the downstream sub-catchment? Why would the capacity for in-stream retention be greater in this area (if denitrification is reduced)? Why would those two microbial processes respond differently to the same drivers? Reduced flow velocity would like enhance both?

Line 380: this is also a general comment – It seems like there is a disconnect between the upper and lower catchment behavior throughout the manuscript. How much of this might be driven by lower model fit metrics/uncertainty in the model outputs for that spatial scale specifically? Also does the land use change factor in here? How about irrigation in the lower agricultural catchment during the drought?

Figure 1: This figure is very data dense and could potentially be split apart into panel a/b and a separate figure for panel c. Further, the flipped double axis is difficult to read – typically discharge is not shown as a hanging plot (usually this is used for precipitation). One suggestion is to pull the hydrographs for all 3 onto a single panel of this figure. And finally, panel C seems like it should be in the results section – it is showing the calibration results of the model.

Figure 2: The color of the slope lines for panel a-c is confusing, because the color scheme flips (grey line corresponds to orange points, red line corresponds to grey points). Also, the discussion starting around line 335 is drawing attention to differences in these C-Q plots by wet vs dry season. The colors here are differentiated by year, and obscure the point about wet vs dry season (within a year). In contrast, there isn't a discussion of how the C-Q slope changes between 2018 and 2019. So you could consider shifting the colors to show the wet/dry season comparison and not accentuate year. This might fit the discussion better.

Figure 3: suggest labeling y axis in panels d-f in same units used in written text/results section (years, not days).