EGUsphere, referee comment RC2
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Comment on egusphere-2022-717

Anonymous Referee #2

Referee comment on "Stable isotopic evidence for the excess leaching of unprocessed atmospheric nitrate from forested catchments under high nitrogen saturation" by Weitian Ding et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-717-RC2, 2022

This manuscript aims to investigate how forests attenuate atmospheric NO3- deposition. Work was carried out in two forested catchments in Japan, one at a lower elevation that previous work shows receives higher loads of atmospheric NO3- deposition and the second at a higher elevation that receives lower levels of atmospheric NO3- deposition. In each catchment monthly streamwater samples were collected over ~2 years, and isotopic measurements (del 17O) were used to determine the proportion of NO3- export derived from atmospheric deposition. The key finding of the work is that the lower elevation / higher deposition catchment also exports a proportionally higher quantity of atmospheric NO3-. The topic of how, and how much, forests can retain atmospheric NO3- inputs is of scientific interest and relevant to *Biogeosciences.* However, presently this work is a bit short on data (monthly samples from three sites across two streams are combined with previously published annual atmospheric deposition rates) and analysis (primarily focusing on deriving the ratio between atmospheric NO3- export and inputs at the annual scale and determining whether or not each catchment was N saturated or not).

Key comments

- It is difficult to identify a single driver for the differences in the proportion of atmospheric NO3- export between the two sites given that they differ both in terms of the amount of N deposition and their climate (the low deposition site receives significantly less rainfall and is significantly cooler than the high deposition site; L120-121). These also led to differences in vegetation between the two sites (L114-119). Differences in hydrology are not accounted for, but should be (e.g., both surface water – groundwater interactions and slope, both of which could impact N attenuation and the degree of streamwater mixing with microbial NO3- sources). The

fact that FK has lower concentrations of atmospheric NO3- at the upstream site than the downstream does indicate that there is unaccounted for hydrologic mixing (or loss) occurring along the stream, which could significantly bias M/D estimates based on a single sampling point (as in the MY catchment). Given how different the sites are MY does not act as a useful 'control' for FK. Additional data that would enable a functional understanding of how NO3- moves through these two different sites is therefore needed.

- The atmospheric deposition info used to calculate M/D (the crux of the study) were collected over 10 years, but these measurements ended prior to the stream water sampling that is the primary data here. This is a major limitation, given how much atmospheric N deposition can vary month to month and year to year. A robust approach to constrain the uncertainty created by relying on this 'mean' data is required. Information is also needed on the exact location of the atmospheric sample collection relative to the streamwater collection sites (in particular for helping to assess whether there might be differences in atmospheric inputs at sites FK1 v FK2).

Specific comments

L4: The abstract should be revised to start with establishing the 'big picture' issue addressed and aim of the study, rather than jumping straight in to site differences.

L4-6: Here and elsewhere, I suggest referring to the sites by name rather than using acronyms, as this will make it easier to connect this to other work on the sites and more intuitive to follow within the manuscript.

L50: This line suggests that groundwater inputs are greater in humid temperate forests than other biomes, which is as far as I know not true.

L66: Word missing after 'recent'

L93-95: How could the validity of the approach be tested with the collected data? Why is there reason to think that this method wouldn't work in catchments with higher rates of N deposition? A clear hypothesis about how and why catchment retain v export atmospheric NO3- will be important for setting up a stronger discussion section.

L96: Word missing after 'recent'

L105-107: As above, it is not clear how the reliability of the M/D ratio can be evaluated using these methods. What results would show that it's *unreliable*?

L126: How were the boundaries between the FK1 and FK2 catchments determined? Fig. 1 indicates that these sites are both located along the same stream in the same catchment.

L161-163: More information on internal standards needed (number, delta values, etc). Information on calibration for del17O also needed.

L226-229: Were climate conditions (rainfall, stream flow, temperature) significantly different between the years where atmospheric N was measured v the years where stream N was measured?

L234: Is this a reasonable explanation for the two sites? Some geologic / hydrologic information is needed to support this.

L236: Given how important this value is for estimated M/D (L264), it would be illustrative to calculate stream flow based on a range rather than a single average value.

L273-275: Did rainfall differ between the two stream water sampled years? This would be useful information for helping interpret differences in NO3- over time.

L290: Report in more quantitative terms (what is 'little' variation?)

L302-305: Move to Discussion.

L325-329: What is the likely source of the 20% discrepancy? Is this due to differences in method (and if so how / what?) or genuine inter-annual differences in either N inputs or N retention? These points should be expanded on here.

L336-343: The collected data would need to be combined with more detailed meteorological information and/or isotopic modelling in order to determine the source of atmospheric N to the two sites. Consequently this explanation for the differences between the two sites is mostly speculation and does not have much baring on the overall aim of the study (to understand forest N saturation dynamics), so I suggest removing altogether or moving to the site description as part of the explanation for the known difference in N

deposition rates between the two locations.

L349: But how many locations has this been reported for? Given the relatively small dataset shown in Table 3 I wonder how surprising the relatively high M/D ratio is. Is it likely that other sites around the world will have similar (or even higher!) ratios?

L353: What else besides Datm could cause the high concentration of NO3(atm) in the stream water? Alternative explanations (if they exist) should be discussed.

L370-388: Beyond forest N uptake, what could cause catchment retention of N deposition? E.g., retention in soils or groundwater?

L415-418: How does this finding compare to other parts of the world where precipitation is low but N deposition is high (e.g., parts of the southwestern US)?

L421-422: The relationship between precipitation and N losses really cannot be evaluated here given that the stream and precipitation data is decoupled (stream data collected after the precipitation sampling was concluded), and that dynamics are consequently evaluated only at a very broad timescale based on mean average annual precipitation and evapotranspiration for the two sites.

Fig. 1: This indicates that sites FK1 and FK2 are just two points along the same stream, meaning that they represent the same catchment. Some clarification is needed in the Methods and here to describe the hydrologic connection between the two locations and whether they should be considered upstream/downstream or two different sub-catchment (in which case this map should be updated to clearly show the catchments).