

Review comments of the manuscript tc-2021-166 by McCaully, Arendt et al. titled: High Temporal and Spatial Nitrate Variability on an Alaskan Hillslope Dominated by Alder Shrubs.

General comments

This study presents a comprehensive dataset which illustrates how substrate source (alder litter) and spatial connectivity in a sloping permafrost landscape may be larger control on NO_3^- presence in uplands than soil moisture content, how increased NO_3^- related to N fixer presence may be mobile in the landscape, and how redox conditions related to soil moisture and topography impacts the spatial extent of this mobility. This is a valuable contribution which underlines the importance of considering topography and N fixers as plant functional type in predictions of future plant N availability and potential N_2O emissions.

However, this is a complicated dataset, and the study could benefit from a more coherent storyline, where the different datasets are presented not only in sequence, but are used together to tell a common story and the reader understands why the methods were chosen. This is done nicely in the abstract, but lacks in the discussion, where the $\delta^{18}\text{O}$ results and the $\delta^{15}\text{N}$ and NO_3^- concentration results are, I suspect, not intended to be two separate stories, but they appear as such at the moment.

A few more specific section comments:

Introduction: The language could use an overhaul, mainly a condensation of the text, where some points are repeated and some sentences/sections come out of context (see specific comments below).

Materials and methods: The sample design is very comprehensive and complicated and as such benefits from a detailed description. However, the information could be more closely related to Figure 1b and 1c for clarity and condensed. The description of isotopic calculations is clear and useful. There is a lack of a quantitative estimate of precipitation (now currently addressed simply as “Precipitation events”) from e.g. a micrometeorological station, as the precipitation downslope movement of NO_3^- is such a central part of the results.

Discussion: The storyline of the discussion is not clear and appears more as a list of results related to literature than a use of results to illuminate your research questions. As an example, the discussion of $\delta^{18}\text{O}$ related to precipitation events (line 328-339) comes a bit disconnected from the NO_3^- -story, but I suspect there is a point related to N transport, which needs to be clarified. The discussion needs to be restructured and condensed to tell the study story based on the results.

Because of the large revisions needed in the communications of the results, I recommend that the manuscript can be reconsidered after major revisions.

Specific comments with line numbering

Line 35-37: Which links and why is it important? Give one or two examples for a more engaging story.

Line 51-56: This second half of the paragraph seems a bit out of place, because the text introduces alder effects on soil chemistry above and continues below. Consider moving it and even skipping line 51-52 or replacing the sentence in line 39-40 as they say much the same.

Line 68: Alternatively “situated in a hillslope landscape”

Line 91-91: I don’t understand the function of this sentence in relation to the next sentences.

Line 160: A sentence on how $\delta^{18}\text{O}$ from H_2O (soil solution) in your NO_3^- is derived would be useful here.

186-190: Iron, sulfate and Manganese enter the story a bit abruptly here. If they have a function in the study design (as it is later clear that they have), please add a sentence earlier when explaining the study scope and strategy, adding the function of measuring those parameters.

Line 269: You define all the other pools, but SON is not defined (Soil Organic Nitrogen, I assume)?

Line 284: This is an interesting finding from this study

Line 290-91: This statement, referring to Boshers et al. (2019) should be explained further. While the equation 1 is nicely explained previously, the argumentation for choosing this method should be discussed in relation to alternatives. You mention that “both possibilities” (line 291-292) are shown in figure 5. By this, I take that you mean the H_2O -derived only and the Eq. 1 determined predictions (?), however, I see only one interval of predictions in figure 5. The text and the link needs a better explanation.

Lines 328-339: This section is interesting and coherent in its argumentation, but its place in the story of the manuscript is not clear. The point may be that there is a connection to the NO_3^- transport and –source, however, this link needs to be clearer for this section to be relevant to the overall story.

Line 375-385: This section is a good example of clear, well-written communication/discussion of the results. !

Technical comments with line numbering:

Line 19: The parentheses around NO_3^- concentrations are not necessary and should either be removed or the sentence restructured

Line 32: Consider using “near-surface hydrologic conditions” in order to exclude e.g. subpermafrost groundwater

Line 181: a comma is likely missing between “bags” and “frozen”.

Line 184: Soil temperature at which depth?

Line 185: Introduce DO as Dissolved Oxygen before abbreviating

Line 200-201: Back up this statement with a reference?

Line 214: The beginning of this sentence should be reformulated – for once, the comma seems misplaced before “2017”

Figure 4: the lower part of the figure is cut off by the caption