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Comment on egusphere-2022-137

Anonymous Referee #2

Referee comment on "Environmental Controls on Observed Spatial Variability of Soil Pore Water Geochemistry in Small Headwater Catchments Underlain with Permafrost" by Nathan Alec Conroy et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-137-RC2, 2022

The topic is interesting and within the scope of the journal. Conroy et al. investigated environmental Controls on observed spatial variability of soil pore water geochemistry in two headwater catchments in the Arctic. They collected samples (several geochemical constituents) from two different catchments in vegetation, soil moisture, and other characteristics. The authors performed PCA and Mann-Whitney U-Test to compare inter- and intra-catchment variability in chemical constituents. They also used PHREEQC to examine mineral solubility and other thermodynamic controls. The m/s includes several speculative statements and does not provide a clear understanding of different controls on spatial variability of soil pore water geochemistry. For example, their overarching hypothesis is that vegetation type and hillslope position are the dominant controls on spatial variability of SPW geochemistry. I want to hypothesize that spatial variability of SPW leads to different vegetation in different catchments. Both hypotheses can be shown to be true based on the analysis presented in this m/s. IMO, the problem is the limitation of their methodology. The PCA and non-parametric correlations cannot inform controls of spatial variability of SPW quantitatively. The authors should include coupled physical modeling to explore dominant controls. I have given more specific examples below. The figure quality is not good. For instance, it is hard to read Figure 4. The other minor point is that the m/s should be improved for style and writing.

Specific comments:

Lines 100-103: This study focuses on two sites with permafrost on the Seward Peninsula of western Alaska, the Teller-27 Catchment and the Kougarok-64 Hillslope (Figure 1). Figure 1 is confusing. The Teller-27 Catchment, henceforth "Teller,"... and The Kougarok-64 Hillslope, henceforth "Kougarok,"...

I do not understand why Teller is located in the figure twice and so far away from each other. Which one is Teller in the study?

2: Line 107: These are identified as TL# (Teller Station #) or KG# (Kougarok Station #) in Figure 2 and Figure 3, ...

I did not find TL and KG anywhere else in the m/s, except in Tables 1 and 2.

3: Line 108: Teller and Kougarok are not paired watersheds. Why did the authors choose them to compare?

4. It will be better to make a table of similarities and dissimilarities between the two watersheds, as described in Section 2.1.

5. Line 216: When the authors investigate a permafrost site, why were modeling exercises performed at 25 °C? Shouldn't they do it for the entire range of temperatures observed there? Or the temperature at which samples were collected?

6. Why was Methane production "turned off"? In the Arctic, several papers (from NGEE itself) have examined methane as GHG variability.

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2009JG001283

https://bg.copernicus.org/articles/10/5139/2013/

There are several other paper on this.

7.4.1 Inter-site Variability: Teller versus Kougarok: The authors found that many constituents were significantly different between

Teller and Kougarok. It is unclear how they deduced vegetation, soil moisture, and redox, weathering, water/soil interactions, hydrological transport, and mineral solubility control the difference between the two sites. However, constituents between the two sites will show significant differences anyway. One could measure these constituents in any catchments elsewhere; two catchments would show significant differences almost every time.

8. Discussion in sections 4.3 to 4.5 is speculative. Comparing observations from two sites and linking several controls using statistical measures like PCA and correlations is not convincing. To come to these conclusions, the authors need to quantify the controls and do physical modeling.

9. For example, in Line 304: it is unclear why Teller Station 2 [SHOULD it not be TL2 as mentioned in Section 2.1] did not exhibit elevated NO3 while Station 7 did, but we suspect that higher

305 seasonal moisture content and greater microbial denitrification at Teller Station 2 likely played a role.

10. Line 313: The lack of a clear correlation between vegetation and soil moisture...

These are speculative statements. Modeling would need to demonstrate the real cause.

11. Section 4.4: Did the authors do thermodynamic modeling to investigate their results? It is not clear how they came up with some conclusions. For example, what was the source of Iron (III) and Mn? They talked about different redox species; what was the mineralogy there?

12. Line 345: weathering, water/soil interactions, and hydrological transport were clear drivers of

hydrogeochemical variability for some solutes?

How is this so clear? I guess the authors need to perform physical modeling to prove these statements. They are mostly true, as the author cited several papers, but that brings the question of novelty. What is new here?