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Interactive comment on "Controls on spatial and temporal patterns of soil nitrogen availability in a High Arctic wetland" *by* Jacqueline K. Y. Hung et al.

Anonymous Referee #2

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Hung et al. examined spatial heterogeneity in soil nutrient pools, effects of prospective abiotic drivers of nutrient availability, and relationships of nutrients and soil moisture with carbon balance of a High Arctic ecosystem. The study identifies landscape positions and times within the growing season that support strong links between nutrients and productivity. Empirical studies such as this have potential to reveal relationships between source-sink dynamics of carbon and spatial and temporal variation in soil moisture that have previously been unrecognized. This study quantifies correlational relationships among carbon fluxes, nutrient availability, and abiotic attributes of soils and extends these correlations to assess mechanistic relationships. Greater caution and scrutiny must be applied to interpreting correlational relationships to consider al-

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ternative explanations that do not include direct causal links between the measured attributes. For example, coherent temporal patterns between nitrogen pools and productivity might result if both are responding to a shared driver, and might not reflect a direct effect of nitrogen on plant production. The manuscript's context is broad relative to the limited spatial and temporal extent of data collection. There is value in such focused studies, as they can reveal key patterns that might affect processes at larger scales (e.g., regional C balance), but the patterns revealed by the current analyses and their potential implications tend to get lost among discussions of tangential processes not directly addressed by the data in-hand (e.g., phosphorus, N transformations). Finally, there is a missed opportunity to compare patterns in soil nutrients with nitrogen dynamics at the watershed scale, for which there are long-term observations at this site.

Specific comments

Abstract Line 15: Suggest replacing "highly" with "strongly" here and throughout when referring to correlations

Line 15: "dry tracks" and "wet tracks" not yet defined. The correlates of nitrate reflected in the R2 values are unclear.

Introduction The Introduction is long relative to the study's objectives and to other papers published in this journal. Hone in on documented factors that influence nutrient availability and potential links between dynamics of nutrients and carbon fluxes, and pare away ideas that do not directly inform the present analyses.

p. 2, line 30: delete one instance of Arctic

p. 2, lines 31-32: Increased specificity needed here with respect toe "projected increases." Does this refer to CO2 flux?

p. 3, lines 4-5: This text is identical to the text of Commane et al. Commane et al. is not included in the Literature Cited section.

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p. 3, lines 17-20: How are “high Arctic” and “wetlands” defined here? Many study sites cited as such are not classified by the original authors as wetlands or geographically within the high Arctc (e.g., alpine tundra in the Alaska range)

p. 4, line 5: define CBAWO

p. 4, line 28: revise for grammar

p. 6, line 14: Please describe the spacing of the points on the sampling grid.

p. 7, line 30: Please report limits of quantitation and how samples below these limits were handled.

p. 8, lines 16-17: Ecosystem respiration includes heterotrophic respiration, and therefore NEE-ER does not yield GPP. See Chapin et al. (2006) for consensus definitions of carbon cycling terms. Chapin, F. S., Woodwell, G. M., Randerson, J. T., Rastetter, E. B., Lovett, G. M., Baldocchi, D. D., et al. (2006). Reconciling carbon-cycle concepts, terminology, and methods. Ecosystems, 9(7), 1041–1050.

Methods p. 8, line 18: Regression and Pearson correlation analyses are duplicative and only one should be reported. If the coefficients are of interest and linear associations are expected, use regression.

Results p. 9, line 12: I am not certain of the interpretation of the epsilon terms reported here, but I believe they are associated with the deviation from the sphericity assumption of the rmANOVA. Typically those values are used to correct the final P-value. It is unclear whether corrected P-values are reported.

p. 9, line 15: I recommend leaving out the within/between subjects language in favor of more straightforward reporting of the ecological pattern captured by each term.

p. 12, line 19: These regression statistics would be easier to interpret if reported on the corresponding panels of figures 5 & 6. However, the regressions should be performed as multiple regressions to avoid inflating the chance of false positives. Further,

collinearity among predictors should be addressed.

p. 13, line 8: Referring to a table or figure, rather than a p-value to support the result would provide clarity.

p. 13, lines 30-32: Many correlational relationships are described here, and it would be fruitful to speculate about multiple potential causal associations. For example, seasonal patterns in these abiotic attributes might co-occur with the light regime and therefore NPP, resulting in less labile substrate to fuel ER, but with no direct effects of moisture, temperature, and active layer on ER.

p. 14, line 10: Has it been established that soils at the study site are anoxic?

p. 14, lines 17-20: There are some potentially interesting ideas about the drivers of N dynamics listed here, but the effectiveness of this discussion would be improved if the logic linking each of the factors was fully spelled out.

p. 14, lines 26-30: Here is another example of interpreting correlations as causal relationships. It is plausible that another factor, likely seasonality, drove both NPP and nitrate availability, rather than nitrate influencing GPP directly. Further, increased availability of nitrate in soils could occur due to lack of plant uptake of N.

p. 14-15, lines 33-2: The parallelism between the present and Toolik Lake result is not quite clear. How are patterns in light reflected in the present dataset?

p. 15, lines 19-20: Several papers from the Cape Bounty study have addressed nitrate dynamics from a catchment perspective. It seems relevant to place the present results into the existing context for the site.

p. 16, lines 10-28: Discussion of steps in the N cycle and nutrients (P) not addressed by the present dataset (available nitrate and ammonium) is beyond the scope of this study and detracts from its take-home messages.

Fig. 4: It would be helpful if the symbol colors or sizes were proportional to the resin

N content. Shapes could be used to represent wet/dry tundra. I don't think spatial interpolation is appropriate here because the area between the two sets of points is unsampled, and therefore error in the estimates varies greatly across the study area.

Tables 6-7: Interpretation of the B1, B2 identifiers is unclear.

Fig. 5: These plots require labels with units on both axes.

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