

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.

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RC: General comments: Overall, this paper provides a clear method section and presents an extensive analysis of a dataset of available inorganic nitrogen in a high arctic wet sedge meadow. The spatial extent of data collection is welcome and given the importance of arctic wetlands, this study could provide a valuable addition from a different site. There is definitely value in improving understanding the controls on C and N cycling in arctic wetlands and there are potentially interesting data here. The manuscript, however, would benefit from an appropriate title, clearer aims and a greater effort to highlight what novel contribution the study makes. There is a substantial quan-

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tity of introductory text and results, and given that a fairly high proportion of the material confirms existing knowledge (such as the importance of hydrology in determining abiotic processes in wetlands, the importance of fine scale variability in Arctic ecosystems, the multiple environmental drivers of soil nutrient availability), a more concise summary of previous work would leave more room to explore novel findings. In view of the fact that the main issues are mostly to with approach to structure and content, I have provided comments in combination with the minor technical notes.

AC: We appreciate the insightful comments and questions posed by the reviewer here. Please see the attached supplementary for a revised abstract and more concise introduction. The title has also been changed to better reflect the paper's contents.

Specific and technical comments:

RC: Title and abstract - the abstract has a strong focus on N availability and fits well with the title, but a substantial proportion of the results and discussion relate to C exchange. Title and abstract should be modified to better reflect the content of the rest of the paper.

AC: This has been addressed in the attached supplementary. The title now reads "Available nitrogen and environmental controls on carbon exchange in a High Arctic wetland".

RC: The introduction would benefit greatly from a reduction in length, through taking a more focused approach to presenting the background information and rationale specific to this particular study. The current information is a sort of mini-review demonstrating the breadth of understanding of various elements of arctic ecosystem function but the text moves rapidly between wider issues such as global climate change (p3 lines 3-7), and the specifics of the study several times and this makes it challenging to disentangle exactly what the current understanding is and what gaps this study addresses. Specific sections - p2 line 16 to p4 line 6 includes multiple statements where the relevance isn't clear (for example, the lack of long-term studies in the Arctic, when this a short term

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study), and p3 lines 20-30 could be summarized in much less text. The sentence at lines 4-6 is one of the few mentions of the key information underpinning this study and yet it is not stated what the past research is or what it showed.

AC: This has been addressed in the attached supplementary; the introduction has been shortened by a page from the original submission.

RC: P2, line 15 – ‘this study looked at spatial patterns to see how these patterns shift through the growing season’ – I presume what is meant is that spatial variability and temporal variability were investigated (doesn’t make sense).

AC: Sentence removed; this has been addressed in the attached supplementary.

RC: P2, line 15 and elsewhere – the investigation of growing season variability seems overstated – is two sampling periods sufficient to investigate temporal patterns of nutrient availability, even in such a short season?

AC: All mentions of “growing season variability” will be replaced with “intra-seasonal variability”, as we do understand that the use of the former is overstated in given the timeframe of this study, as it didn’t capture the entirety of the growing season. Mentions of the study’s timeframe will also be defined using the exact dates of study.

RC: P2, line 28 “global C changes” is vague – clarify what processes are being referred to here.

AC: Sentence should read “. . .and could contribute significantly to the global C balance.”

RC: P4, line 5 – seems to be the first mention of the CBAWO wetlands, if so, define abbreviation.

AC: CBAWO should be defined as the “Cape Bounty Arctic Watershed Observatory (CBAWO)”.

RC: P4, line 7 – not sure ‘nutrient regime in plant growth’ is the right phraseology for

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what is meant here.

AC: The phrase has been removed.

RC: P4, line 25 – this sentence is unclear – what is meant by ‘help in future predictions of decreasing carbon storage?’

AC: Sentence has been revised to read “Understanding the role N plays in C exchange in Arctic ecosystems will help in predicting the response of the Arctic C cycle to changes in temperature (Chapin et al., 2002; Mack et al., 2004)”.

RC: P5, line 11 – what is a ‘spatial lens’? Please clarify what exactly it is that hasn’t been previously investigated.

AC: Sentence should read “However, the spatial relationships between available N and C exchange have not been assessed.”

Methods

RC: Throughout methods paper refers to early and late season. This implies that the experiment took place either side of the ‘mid’ season, when actually it’s a main growing season experiment that doesn’t include ‘early’ and ‘late’ growing season (as acknowledged by the authors in the second paragraph of section 4.2). Suggest either using real time descriptions or clarifying in the methods section (2.2) where the sampled timed periods fit in the overall growing season.

AC: Addendums will be made to indicate the specific timing of the sampling periods in the overall growing season.

RC: P6, lines 4 and 5 – it is not clear how the vegetation communities differ between wet and dry tracks (i.e. do sedges and grasses dominate, with lesser elements of *Salix arctica* and herbaceous flowering plants in each of the different types, or does *Salix arctica* dominate in wet tracks?) With the genera (*Carex* and *Eriophorum*), can the authors say whether they’re referring to two or more species (spp.) or one unspecified

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species (sp.)? Also, is there a word (herbaceous?) missing between flowering and 'Eriophorum' (and if so, is this appropriate description as Eriophorum belongs to the Cyperaceae). I think Gramineae is now Poaceae. I appreciate that biotic controls are outside of the scope of this experiment, but there is a surprising lack of mention of the possible role of vegetation in any of the relationships described later, given that different species / plant functional types preferentially use different N sources (e.g. NH₄, NO₃, organic N).

AC: We agree that this needs to be clarified; the initial detail was not as clear as the biotic controls are outside of the experimental scope. Wet tracks were generally dominated by the members of the Cyperaceae family (i.e. Carex and Eriophorum spp.) and mosses (Sphagnum spp.). Dry tracks often had fewer graminoids (Poaceae, as indicated by the reviewer, not Gramineae), with Sphagnum spp. and various lichen genus underneath. Salix arctica was present in both wet and dry tracks, but less so in the dry tracks.

RC: P6, line 13 – alternate? (not alternative)

AC: The sentence should read “A total of 64 sites were established on alternating wet and dry strips sampled using ion exchange resin strips. . .”

RC: P6, line 15 – was? (not is)

AC: The sentence should read “The aim with this sampling design was to. . .”

RC: Results – is there a reason why this is structured in a different order from the methods? Consistency would make for an easier read.

AC: The order of sections will be reorganized in revision for better flow.

RC: P9, lines 3-5 – the comparison of data from this study with 2014 and 2015 comes out of nowhere – what is the importance of the June 2015 temperature to this study?

AC: Mentions of the 2014 and 2015 data to be removed, as they are not critical in this

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study.

RC: P9 line 19, and P10, lines 5-8 and line 15 – where are the different tracks and what general concept does comparisons of difference between them relate to? This issue is common throughout much of the results (the spatial findings in particular) - references to what is happening on specific parts of the site by name rather than in context don't indicate to the reader how the findings of this study can be applied beyond this specific site.

AC: Please see supplementary Figure 1 for a graphic showing the spacing of the points on the sampling grid and location of wet and dry tracks in relation to each other; this will be included in the revised manuscript.

RC: P10, line 13 – incoming GPP?

AC: Sentence should read “As with ER, GPP was greater in the wet tracks than the dry tracks.”

RC: P11, line 11 and P12, line 2 – opening sentences should be in the methods, not results

AC: Both sentences moved to Methods – Data Analysis section.

RC: P11, line 25 – this is a common finding with ion exchange resins in arctic ecosystems – saturation seems improbable when deploying the same resins in more nutrient rich ecosystems records often much higher totals, so could it be that in a longer burial some kind of equilibrium with soil levels is reached?

AC: The idea behind this sampling design of incorporating two different resin deployments was to compare the seasonality of the resins to test the technology in an Arctic setting. While that was not robustly designed in this paper, future study could look at nutrient additions to see if it is indeed an equilibrium with the soil levels that is reached.

RC: P11, line 30 – further investigation of what?

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AC: Further investigation of ion exchange resin saturations levels or ideal burial lengths through experimental manipulations.

Discussion and conclusions

RC: These should be checked to see whether the data provide evidence to support all the assertions made, especially where they relate to processes that were assumed rather than measured - such as mineralization, nitrification and transport.

AC: We have reviewed the section and notes areas requiring clarification; assertions that are not directly backed by the data were taken out (i.e. references to mineralization reworded to reference NH₄ availability, rather than direct mineralization, as that was not measured).

RC: Although there is discussion in context of other studies, the discussion is lacking in implications and does not clearly demonstrate what it is that this study shows that does not simply agree with previous findings (the majority). Where differences are highlighted (e.g. the presence of nitrate) no further thoughts are provided as to why this might be or what this will mean.

AC: The presence of nitrate alone in this system is notable, particularly for an aerobic process to occur in a waterlogged environment like an Arctic wetland. Different plants species utilize nitrate more efficiently than others (Smirnov and Stewart, 1985; Nadelhoffer et al., 1991), hence the implications of this could influence future aboveground biomass composition and promote inter-species competition for nitrate-N.

RC: Substantial parts of section 4.2 are suggestions for other studies – although this is interesting, it doesn't relate to the sub-heading and it could be summarized in a couple of sentences, rather than providing a heavily referenced rationale.

AC: The original submission of Section 4.2 lacked much of the main findings from this study pertaining to comparison the strength of relationships between environmental variables in predicting carbon flux vs. inorganic N and environmental variables in

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predicting carbon flux. These findings will be included in subsequent revisions of the manuscript. To summarize, as seen in the attached Figure 2, the strength and spread of the relationship to carbon dioxide exchange is tightened when plant-available nitrogen forms are factored in. To date, many studies of Arctic wetlands have not factored in the importance of soil-available nutrients in explaining seasonal variability of carbon flux.

RC: In the conclusions, some of them seem not arise from the results presented (for example, was there a test of the relationship between distance from snowpack on N availability, and is there any evidence that mineralization promotes photosynthetic activity?) and many of them are readily referenced to other older studies that it is not clear what has been found that is not already well known.

AC: References to the snowpack and spatial aspects relating to that will be taken out, as they are not critical to the study.

RC: Figs 5 and 6 – add y-axis labels.

AC: Figures edited to add y-axis labels

RC: Table 7 – title doesn't match table contents

AC: Titles for Table 7 and 9 should read “Bivariate regression R2 coefficients for carbon flux measurements (dependent variable) against nitrogen (independent variable). . .”

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-440/bg-2017-440-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-440>, 2017.

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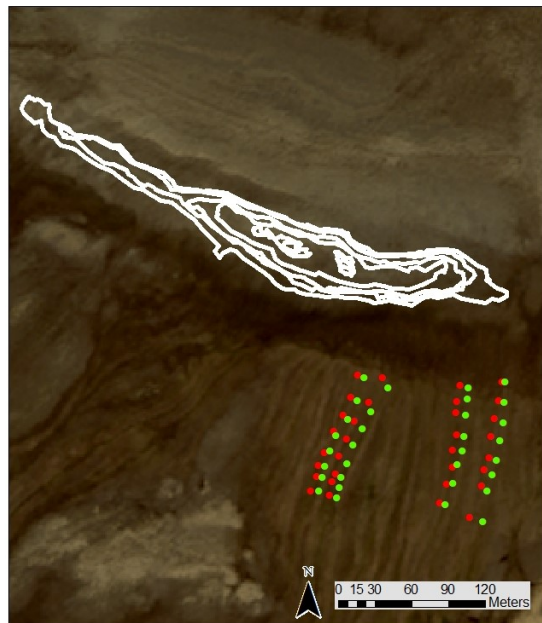


Fig. 1. Natural cover image of the study area with wet (red) and dry (green) plots overlaid

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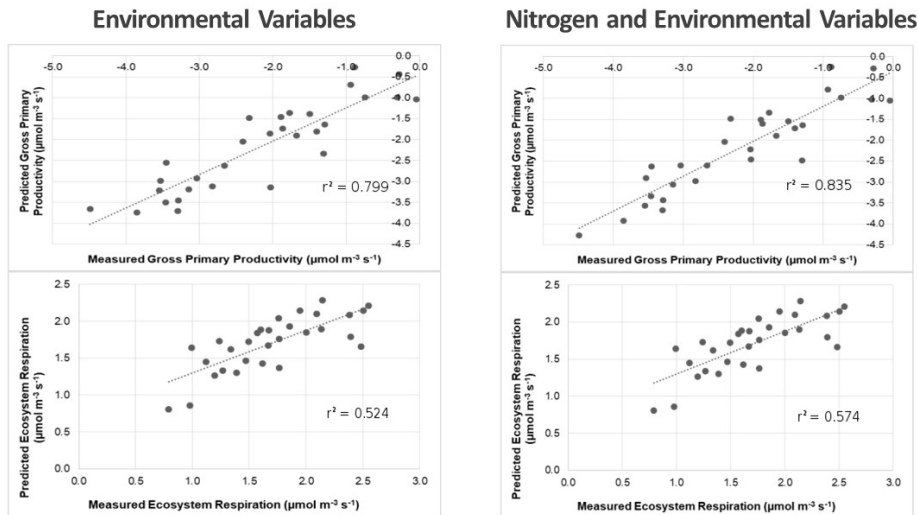


Fig. 2. Multiple regression results using environmental variables and nitrogen in predicting CO₂ exchange

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