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Biogeosciences
Discussions

Interactive comment on "Permafrost thaw and release of inorganic nitrogen from polygonal tundra soils in eastern Siberia" *by* Fabian Beermann et al.

Fabian Beermann et al.

fabian.beermann@gmail.com

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Anonymous Referee 2 Received and published: 18 May 2016 I. GENERAL COMMENTS The research presented by Beerman et al. is novel and very interesting: permafrost literature lacks deep soil N cycling dynamics and this pa- per therefore represents a valuable contribution to the field. The research presented here has carefully been executed and has important ecological consequences for high latitude ecosystems given the fact that plant growth in these ecosystems is primarily limited by N availability. Their finding that the extractable inorganic N pool increases with depth confirms work by others in the field (Keuper et al 2012 , Wild et al 2015). There are, however, portions of this paper that should be more fully developed and carefully dis-

cussed. Depth specific data from the soil cores (bulk density, thaw depth) should be summarized and explicitly laid out in tables so that the reader can compare samples from this site to others in the literature. Site conditions should also be laid out and compared explicitly using metrics that are common and available at all three of the study sites. Overall, I feel that this would allow the authors to lay out and discuss the source of differences in the extractable inorganic N pool that the observed between sites as well as between polygon ridges and centers. Based on the methods they describe, variation between the samples and sites could be due to 1) differences in extractable N pools with depth or 2) differences in expected thaw but these two sources of variation are currently very hard for the reader to tease apart and not compared in the manuscript. I would also like this paper to lay out more clearly how subsidence are included in their calculations or if their calculations would be different if the sites experience subsidence. With ice content of over 80the structure of the soil profile is sure to change and it was not at all clear that the authors have taken this into account. Additionally, the authors repeatedly refer to the release of N from these permafrost soil profiles which I feel is an inaccurate portrayal of the data at hand: they measured the extractable inorganic N pool and scale it the expected increase in the active layer depth but they did not measure a flux of N from a given depth or soil. The flux they are referring to is on a landscape scale rather than on a soil sample scale and these two should be carefully differentiated.

The reviewer has spent a considerable amount of time on this review. We highly appreciate this effort which helped us to improve our manuscript and to better focus on the special characteristics of the studied polygonal tundra ecosystem. We added tables to present depth specific data from the soil cores as well as site conditions and also carried out statistical analyses for the soil chemistry and also for the expected differences in thaw. Throughout the whole manuscript we replaced the term "release" by the term "liberation" (or thaw induced liberation of previously frozen nitrogen) to avoid confusion and also changed the title of the manuscript to thus changed the title to "Permafrost thaw and liberation of inorganic nitrogen in eastern Siberia". Furthermore, we explicitly *stated that we did not take subsidence into account as reliable parametrizations do not exist. However, we do not expect a collapse of the soil profiles upon thaw. The soil profiles with very high ice contents and high porosity consist of frozen peat and - as we do not expect the polygonal tundra to dry out - the peaty soils in the polygonal centers will keep their structure when the soil thaws. Below we respond on a point-by-point basis on every comment. We distinguished the review from our response by using italics for our response. Line numbers correspond to pages in the paper.*

II. SPECIFIC COMMENTS Title/abstract:

Not all the soils surveyed were from polygonal tundra so this title may be misleading: 2 of the 11 cores were from a flood plain that lacked patterned ground (section 2.2). This seems an important distinction, especially given the fact that the upper range of the inorganic N released by these thawing profiles is attributable to the floodplain cores (Table 2). If the floodplain cores are removed and only true polygonal tundra samples are considered the range changes from 8-81 mgN/m2 (highlighted in the abstract) to 8-41 mgN/m2. The authors also do not make any comparison between N release from polygon ridges versus polygon centers which seems to be a missed opportunity given the study system.

We agree that the title of the manuscript is misleading and thus changed the title to "Permafrost thaw and liberation of inorganic nitrogen in eastern Siberia" to include all of the different investigated sites. A comparison between the polygon ridges, centers and floodplains is included in the results and the discussion section

Introduction

Page 2, Line 15: Not all the references listed here are not appropriate for establishing N limitation of tundra plant growth. Mack et al 2004 is a long term look at fertilizer impacts on ecosystem C balance but the release of N limitation on plant growth at Toolik site was characterized by earlier papers: many listed and synthesized in Shaver et al. 1992 (Bioscience). Reich et al 2006 is also a confusing reference to cite here

since that paper looks at interactions between CO2 fertilization and N avail- ability across systems.

Following the suggestions we changed the selection of the references

Page 2, Line 19-20: The statement here that permafrost thaw would be REDUCED by increased snow cover/ insulation is contradictory to the literature cited. Myers-Smith et al 2011 as well as many snow fence experiments found that snow insulates soil from cold winter air temperatures but this results in warmer soils, not cooler ones. Myers-Smith does cite some mechanisms for cooling with in- creased shrub cover (increased evapotranspiration, increased shading) but the impact of these cooling mechanisms is strongest during the growing season rather than during the winter.

We excluded the statement that permafrost thaw would be reduced by insulation and restricted the sentence to the finding by Blok et al. (2010) that permafrost thaw in summer can be reduced by shading of the shrubs.

Page 2, Line 28-30: Again, references for this statement do not appear to be well chosen. The statement that tundra plants can take up inorganic N forms is not something addressed in work by Harms and Jones (2012) or Wild et al (2013). Harms and Jones looked at hydrological transport of inorganic N following permafrost thaw and Wild et al looked exclusively at N transformations in soil profiles with no plants present. Please read literature carefully and cite primary sources accurately. Chapin et al 1993 (Nature) would be a much stronger choice here and one where uptake of organic N by a tundra plant was actually quantified.

Following your suggestions we changed the selection of the references

Page 3, Line 12: The word mobilization implies release of N from an inert state: I find that this incorrectly suggests that the flux of N from soil organic matter was measured. The extractable inorganic N pool was measured for the entire soil profile and this was scaled to an increasing depth of thaw. Though this is a "rate", the observed rate of increase in the extractable inor- ganic N pool is solely attributable to the increasing thaw depth, NOT an increase in the release of N from a given gram of soil organic matter. The true release of N from soil organic matter would also have to include the DON component, which as Wild et al. found is a large proportion of the dissolved N in the soil solution and was not measured in this study. The inorganic N component is interesting in its own right but you must be clear about what you did and did not measure.

We totally agree with the referee that the word “mobilization” (and also “release”) do not appropriately characterize the findings of our study. Thus, we consequently replaced these words throughout the manuscript by the term “liberation of frozen nitrogen” or just “liberation”, respectively.

Materials and Methods

ÌĘSite descriptions should reference Figure 1 somewhere. Also, please lay out what the active layer depth is at each site. I see unfrozen peat in the supplemental figures but it is not clear whether this is the entire active layer or just a measurement from the sampling date. The CAVM vegetation characterizations used are helpful but please specify the dominant species at each site. What are the shrubs present at each site? What are the sedges at Lena and Kytalyk? This will allow your reader to assess how similar or different you sites are. It is a bit hard to do this currently

Figure 1 is referred to on page 3, line 25. Active layer depths are included in Tab 2. The description of the vegetation at all three sites were improved to allow a better comparability of the sites. Unfortunately, there is no long-term measurement of the active layer depths for all three sites so that we only can present data for the thawing depth in the respective sampling time (August 2011 and August 2012)a

Page 4: Please calculate mean annual air temperatures so that they are from the same period of time. Lena River Delta MAT is from 2002-2013, Kytalyk site is from

2001-2011 and Kolyma River Delta site has no dates specified for the MAT provided. Make it so that these numbers can be compared! Similarly, please provide similar metrics for the monthly temperatures from each site. These climate variables would be best summarized in a table and save the written site description for compar- ing the sites other characteristics. Which is coldest? Deepest thaw?

The meteorological are now calculated from the same time period (MAT: 2001-2011; Precipitation (2001 – 2003). These data are presented in Tab.1 and the site descriptions were improved.

Page 4, Line 23-28: This description is lacking considering the information provided about the other two.

The description for the third site was improved in comparison to the other two

Page 5, Lines 3-8: If you are going to use these core labels throughout the paper you need to explain them more clearly. Introduce the site abbreviations when you describe the sites and then here show that an R designates ridge and C desig- nates center.

Following your suggestions, we introduce the site abbreviations in the site description and explain the core labels in detail at the beginning of the soil sampling section as follows: “All soil cores were labelled by their specific study site (LEN, IND, KOL), a consecutive number and, in the polygonal tundra, either by their origin from a polygon ridge (-R) or a polygon center (-C).”

Line 7-8: Why are the floodplain samples included in a paper titled “Polygonal tundra?” If there is a reason you must fully justify it and discuss at length. Right now these samples double the range of your estimate for the extractable inor- ganic N pool but the fact that they are not from polygonal tundra is not mentioned. This is a missed opportunity for discussion and site comparison.

as explained in detail our response to the general comments, we changed the title of our manuscript to include the floodplain soils more coherently into this paper

Line 11: How are you defining plant available ammonium and nitrate? This a salt extract and therefore might be larger than the plant available pool. Also, plants in this system can take up organic N so this extractable inorganic N pool is only part of the total plant available pool.

We removed the term "plant-available" throughout the manuscript to make clear that we measured only the contents of extractable ammonium and nitrate in the samples.

Line 21-25: Please clarify this method: what is sensitivity of these cuvettes/spec for each of the analyses performed? Please also cite other environmental biogeochemical literature where this method has been used: looking at the Hatch-Lange website the applications for their products seem to be geared towards monitoring water quality rather than scientific research. You also do not mention how NH4/NO3 was measured for the Lena R. Delta samples

Though originally meant for monitoring water quality, these cuvette tests were the only possibility to measure inorganic nitrogen at these sites directly after sampling and without transporting the samples from Russia to Germany. To ensure their applicability for scientific research, we tested the comparability of these cuvette tests to common photometrical measuring methods prior to the expeditions using a broad range of organic and mineral soil samples. Following your suggestions, we included the sensitivity of these tests. The measuring methods for the samples from The Lena River Delta are mentioned on Page 5, Line 26-27.

Line 24: Potential annual release of N is a confusing term to use: you did not measure a release of N from soil organic matter, you measured and increase in the size of the

active layer. Please re-write this section or define a term that you use throughout the paper. I see that the extractable inorganic N pool of the active layer increases with thaw depth, but this needs to be clearly dif ferentiated from the release of N from a given soil at a given depth. This would be a great thing to look at in a follow-up paper but was not addressed here and a casual reader may get an inaccurate impression regarding what was measured! I mentioned this in my comments on the introduction but it is an issue throughout the paper.

as already explained above, we changed the terms release and mobilization throughout the paper to the term "liberation"

Line 26: Please include a table of

we included bulk density and water content into the table 3 (formerly Tab. 1). Statistical comparisons were conducted only for the soil cores from the Kolyma River Delta as there were no replicates for the two other sites. Statistical comparisons of all soil properties between the active layer and the frozen ground as well as statistical comparisons between the soil cores of the polygon ridges, the polygon centers and the floodplains were conducted and are presented in table 3 as well.

Page 6: I am not sure how Lines 8-30 tie in with your other methods. I assume these are environmental variables used in the model but you do not say this. Please also justify observing soil temperatures in the ridge only: how would you expect the center to differ? What about the floodplain cores?

Answer: In order to compare analytical N data from different sites we choose cores and data available at all three sites. The most comprehensible sample and monitoring sets come from polygon ridges while comparable information from polygon centers from all three sites is fragmentary. Temperature data from floodplain bore holes were not measured at all. The available data from all three sites (polygon ridges) are visualized in the supplementary Figure 2 but cover different observation periods (JUL-11-JUL-12 *in the Indigirka Lowland, JUL-11-JUL-12 in the Lena Delta and JUL-12-JUL-13 in the Kolyma Lowland) during which the core drilling took place. Long-term monitoring (or at least additional data sets) are cited in section 2.5 on p6 ln11-14 as follows: "The longest available record of ground temperatures and active layer depth for Samoylov Island in the Lena River Delta between 1998 and 2011 is given in Boike et al. (2013). Comparable data for the Kytalyk study site in the Indigirka Lowlands between 2008 and 2009 is summarized in Parmentier et al. (2011) and between 2009 and 2011 in Iwahana et al. (2014)." The available monitoring data sets are however limited and cover for example only soil moisture observations during summer time at sites in the Kolyma and Indigirka lowlands (see Figure S2). Therefore, the monitoring data were not numerically put into the model, but provided real background information to test the model's performance.*

Page 6, Line 21-22: How does "phase" come into play with this measurement? What instrument are you refer- ring to?

Soils can be partially saturated (with air and water present), or be fully saturated (no air content) or be totally dry (no water content). In a saturated soil or a dry soil, the three-phase system (air, water, soil) thus reduces to two phases only (air, soil or water, soil). The frozen soil is a porous medium composed of a solid (soil) matrix and an interconnected pore space filled with water, air and ice.

Page 7, Line 1-14: Does this model take into account subsidence? Given the VERY high ice content (>80that the structure of the soil profile will collapse with thaw. If the entire profile subsides, does the model give you the depth of thaw from the "new" soil surface or the one that you measured? Are your calculations for the extractable inorganic N pool dependent on the assumption that material in the soil cores will be at the same depth before and after thaw? This point is critical for the accuracy of your approach.

Our model does not take subsidence into account as reliable parametrizations do not exist. However, we do not expect a collapse of the soil profiles upon thaw. The soil profiles with very high ice contents and high porosity consist of frozen peat and - as we do not expect the polygonal tundra to dry out - the peaty soils in the polygonal centers will keep their structure when the soil thaws. There would only be a substraction of the soil profile due to the phase change of the water of 9% at maximum. Considering that the calculation of the liberated N is based on the frozen ground, subsidence is a minor issue we didn′t take into account. We specified this in the methods on page 7, line 20-22

Line 21-22:

What does "controlling active layer dynamics" refer to?

and

Line 23: Is this the control conditions in Figure 3

and

Line 33: Or is this the control conditions in Figure 3?

the control conditions of figure 3 are described by the sentence in Line 33. In order to avoid confusion the Sentence in Line 23 has been rewritten to "The following period from 1989 to 2012 was used to assess the accuracy of the modeled annual active layer dynamics". See page 7, Line 30-31 and Page 8, Line 7-8

Page 8, Line 3-11: Please include statistical comparisons of ridge versus center soil properties (the numbers scaled with model output is not helpful unless we know how different the data going into the calculation was.

We included statistical comparison between the polygon ridges, centers and the *floodplain for the soil cores from the Kolyma River Delta in table 3 (formerly table 1). However, due to missing replicates for the sites in the Lena River Delta and the Indigirka Lowlands we are not able to present comparison between the three sites.*

Results

Page 9, Line 18-20: Please discuss this result at length in your discussion as well as when you frame your results. These samples have the highest extractable inorganic N pool and are the only samples that are NOT from polygonal tundra.

We extended this part of the Result section and furthermore, discussed this result in detail in the discussion section. See also our response to your comments to the discussion section

Page 10, Line 5-31: It would be easier to read if you re-wrote this section so that the sites were easily compared to one another. How did seasonal patterns and soil thermal dynamics at each site differ? Talk about them relative to one another. Right now you are making the reader do the site comparisons and that is a lot of work, especially given the disparate information available for each site. Please also take care to differentiate this section from the site description written in your methods.

As stated above and described in section 2.5 environmental variables have been monitored using different equipment at all three study sites at different spatial (depths below surface) and temporal (observation periods) scales. The overall summary for all three study sites is described on p10 ln5-15 as follows: "The soil temperatures and volumetric water content profiles at the three sites show the typical active layer processes in permafrost-affected soils, characterized by: i) large seasonal temperature amplitudes of approximately -25 °C to +10 °C in the uppermost layers, ii) seasonal freeze-thaw as indicated by the decrease/increase of liquid water content, iii) pronounced temperature stabilization during phase change at 0 °C during fall freeze

back ("zero curtain") and during spring, iv) liquid water contents in frozen soils up to 0.1 m3 m-3. The volumetric water contents of the polygon ridges ranged between 0.1-0.6 m3 m-3, whereas the center of the polygon was always saturated with water contents up to 1 m3 m-3. Differences in the water content indicate differences in the texture/porosity (peat with up to nearly 100The following site-specific data descriptions based on supplementary Figure S2 to highlight the environmental conditions of the year during whose summer period the drilling to place and the temporary sites in the Indigirka and Kolyma lowlands were instrumented. Water content and ground temperature are certainly connected to the modelling approach and therefore given in the paper. The presentation of monitoring data is meant to illustrate available data on environmental variables of the year of permafrost probing. To us, the site-to-site comparison seems easily possible using supplementary Figure S2 while writing about different observation periods at different sites of variable measured at different depth due to site heterogeneity and technical issues would not much contribute to the reader's understanding.

Line 13: Doesn't 100% porosity mean it is an ice lens? Please clarify.

There were specific samples containing Sphagnum peat with high volume and very low dry weight. When water saturated, these samples had a porosity of nearly 100% but weren't definitely not an ice lense. We specified this in the text.

Line 21-22: Is this number from a TDR probe? I do not think that these work in frozen soils.

The instrument's precision is based on the method as described above, Time Domain Reflectometry (TDR). This method is based on electro-magnetic pulses propagated through the soil along a transmission line, here a triple-wire probe,and its reflections are recorded. From the traveltime of the signal along the soil probe the soil dielectric *constant e can be calculated. It is a well-established method for the calculation of water content in frozen and non-frozen soils.*

See also: Spaans, E. J. A. and Baker, J. M.: Examining the use of time domain reflectometry for measuring liquid water content in frozen soil, Water Resour. Res., 31, 2917-2925, 1995.

Page 11, Line 13-25: Please address here the potential for subsidence considering the high ice content in some of your cores.

We specified in the methods, that subsidence was excluded from our modeling as reliable parametrizations of this process do not exist. Therefore, if we address potential subsidence of our permafrost cores it would be speculative. See also our comment above.

Line 26-31: Again, you did not measure the rate of release of N from soils. Choose different expres- sion for the increase in the extractable inorganic N pool that is attributed to a thickening active layer.

as mentioned above, the term release have been replaced throughout the manuscript by term liberation

Discussion

Page 12: This discussion needs to be developed more fully. How would you expected immobilization on the part of the microbial community to change with thaw? How might this impact the amount of inorganic N available for plants? Please also discuss the temporal and spatial component at work here: the increase in inorganic N you find is all at depth, and all likely to be released late in the growing season. Where are plant roots compared to this N? Are all these soils going to be saturated and anaerobic upon

thaw and likely to denitrify N? How do you think that the extractable organic N pool differs with depth and with thaw? How do you ex- pect successive thaw events of these deep (formerly permafrost) soils to impact the extractable inorganic N pool?

Following your suggestions we extended the discussion of our results. We added a paragraph (page 15, line 12-20) where we more fully discuss the impact and importance of the liberated N, especially with respect to competition with microbial communities, denitrification of N and also deep-rooting plant species. However, we did not discuss the extractable organic N pool and also successive thaw-events as we do not have any data concerning these issues and so any discussion would be speculative.

Line 22-23: How might these limitations of the thaw model impact your results?

We added following information to the text: "Langer et al. (2013) have demonstrated that uncertainties in soil composition (moisture content) can result in uncertainties in the modeled active layer depth of about +- 15cm whereas uncertainties in snow properties had only a minor impact on the active layer dynamics."

Line 32: I don't think that it is entirely fair to say that the floodplain site is the most "sensitive" to climate change. Nothing about the site other than thaw depth is changing in this calculation, which is only one measure of response to climate change. How much of the increase in the active layer extractable inorganic N pool for each site was driven by deeper thaw versus higher extractable inorganic N per depth? If you laid out the depth specific data more explicitly you could discuss this fully.

We changed this sentence to make clear that the active layer thicknesses at the floodplain sites were most sensitive to increased temperatures. Furthermore, we discussed on page 14, line 24-26 that the high N liberation rates at the floodplain sites are a result of high N contents in the frozen ground and also highest increases in active layer thickness

Include tables and statistics for site conditions and initial thaw depths as well as modeled increases in thaw depth. A table with the depth specific soil properties: bulk density, extracts, ice content, inorganic N extracts would be easier to decipher than the figures provided. I know you don't have the replicates for doing stats on data within a site but perhaps you could compare all ridge to all center samples or all permafrost soils to all active layer soils (Table 1, for example). The data in the supplemental figures is very hard to read and almost impossible to compare visually. Don't bury these data in the supplemental material please! This data is very interesting and the discussion would benefit from having a clearer comparison of the sites as well as polygon centers and ridges.

As explained above, we changed the presentation of the results in this paper. Fig. 2 Now provides an easy overview on the elemental pools in all soil cores. Furthermore, Tab. 1 provides information on the climatology of the different sites whereas Tab. 2 of the shows the characteristics of the different soil cores, including initial thaw depths of the sites. Tab. 3 now provides detailed information on the soil elemental pool, on water contents and bulk density as well as shows statistical comparisons between the active layer and the frozen ground and also between the different locations in the Kolyma River Delta.

Overall, I felt that there was missed opportunity to discuss ridge versus center dynamics. The paper focuses on polygonal tundra and there were samples from both ridges and centers but there was not comparison between theses paired cores. If there is a reason for this please lay it out explicitly.

it Though this paper focuses among others on the polygonal tundra, there were no significant differences between the polygonal ridges and the polygonal centers concerning the main subject of frozen nitrogen stores and thawing induced liberation of this nitrogen. We mentioned these inexistent differences in the discussion on page 13, line 29– 31

Please discuss the large increase in extractable inorganic N from the floodplain samples! This is really interesting!

Following your suggestions and as explained above we have discussed these results on page 14, line 31 – page 15, line6 .

III. TECHNICAL CORRECTIONS

Abstract

Use of the term "freeze-locked" seems inappropriate, consider changing here and on page 2 line 4 Introduction

Changed on both pages

Page 2, Line 6-7: This sentence seems to duplicate the previous sentence; clarify or remove

We removed this sentence

Page 2, Line 11: change "also" to "only"

changed

Page 2, Line 25: "Approaches" seems to be incorrect word to use here. Consider "attempts" or "ongoing efforts" Materials and

We changed the term "approaches" to the term "attempts"

Methods

Page 3, Line 30-31: Condense the description of terraces and floodplain: now it is too long considering you only sampled one site. Include enough information that others working at the site know where your samples are from but not so much that people unfamiliar with the site are distracted by the description.

We shortened the descriptition of the study area in the Lena River Delta

Results

Page 10, Line 26-29: Be consistent with your site names, choose ONE per site and use it consistently throughout the paper (especially if you are going to use abbreviations for the sites in your core names). This happened again at the top of page 11

Changed on both occasions

Discussion:

Page 13, Line 13-16: This first sentence is hard to follow. Second sentence is confusing: warmer than what?

These two sentences were reworked

Figures Tables

Figure 1 site labels should be bigger, they are hard to read. I don't think you ever referred to this figure in the text either but I could be mistaken.

A new version of Figure 1 with bigger site labels is provided within the revised document and is referred to on page 3, line 25

Figure 2: Consider just graphing the averages and error bars. You say that some of the data shown are averages and some are raw which is a bit confusing. I know there is a lot of variability between the sites but I am not convinced that this raw(ish) data is conveying any additional information! I like that you have normalized the cores to the depth of thaw but you should say what the active layer depth at each site was somewhere in the paper.

We highly appreciate the suggestion to show just the average data of all soil cores. A new version of this figure is provided in the revised document. The active layer depth

at each site is presented in the new table 2.

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