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

Anonymous Referee #2

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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 paper 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 discussed. Depth specific data from the soil cores (bulk density, %C, %N, thaw depth) should be summarized and explicitly laid out in tables so that the reader

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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 80% in the polygon centers, the 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.

II. SPECIFIC COMMENTS Title/abstract: âĂć Not all the soils surveyed were from polygonal tundra so this title may be misleading: 2 of the 11 cores were from a floodplain 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.

Introduction âĂć 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 availability across systems. âĂć 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 increased shrub cover (increased evapotranspiration, increased shading) but the impact of these cooling mechanisms is strongest during the growing season rather than during the winter. âĂć 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. âĂć 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 inorganic 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

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clear about what you did and did not measure.

Materials and Methods âĂć Site 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. âĂć 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 comparing the sites other characteristics. Which is coldest? Deepest thaw? âĂć Page 4, Line 23-28: This description is lacking considering the information provided about the other two. âĂć 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 designates center. âĂć 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 inorganic 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. âĂć 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. âĂć 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 biogeochemi-

cal 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. âĂć 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 differentiated 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. âĂć Line 26: Please include a table of %C, %N, bulk density, water/ice content of soil cores in a table. âĂć 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? âĂć Page 6, Line 21-22: How does “phase” come into play with this measurement? What instrument are you referring to? âĂć Page 7, Line 1-14: Does this model take into account subsidence? Given the VERY high ice content (>80%) and high porosity in the polygon centers it seems that 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. âĂć Line 21-22: What does “controlling active layer dynamics” refer to? âĂć Line 23: Is this the control conditions in Figure 3? âĂć Line 33: Or is this the control conditions in Figure 3? âĂć Page 8, Line 3-11: Please include statistical comparisons of ridge versus center soil properties (%C, BD, water content, etc) as well as site level comparisons. Comparing

the numbers scaled with model output is not helpful unless we know how different the data going into the calculation was. Results âĂć 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. âĂć 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. âĂć Line 13: Doesn't 100% porosity mean it is an ice lens? Please clarify. âĂć Line 21-22: Is this number from a TDR probe? I do not think that these work in frozen soils. âĂć Page 11, Line 13-25: Please address here the potential for subsidence considering the high ice content in some of your cores. âĂć Line 26-31: Again, you did not measure the rate of release of N from soils. Choose different expression for the increase in the extractable inorganic N pool that is attributed to a thickening active layer. Discussion âĂć 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 expect successive thaw events of these deep (formerly permafrost) soils to impact the extractable inorganic N pool? âĂć Line 22-23: How might these limitations of the thaw model impact your results? âĂć 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. âĂć 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. âĂć 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. âĂć Please discuss the large increase in extractable inorganic N from the floodplain samples! This is really interesting!

III. TECHNICAL CORRECTIONS Abstract âĂć Use of the term "freeze-locked" seems inappropriate, consider changing here and on page 2 line 4 Introduction âĂć Page 2, Line 6-7: This sentence seems to duplicate the previous sentence; clarify or remove âĂć Page 2, Line 11: change "also" to "only" âĂć Page 2, Line 25: "Approaches" seems to be incorrect word to use here. Consider "attempts" or "ongoing efforts" Materials and Methods âĂć 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. Results âĂć 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 Discussion: âĂć Page 13, Line 13-16: This first sentence is hard to follow. Second sentence is confusing: warmer than what?

Figures & Tables âĂć 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. âĂć 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.

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