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Interactive comment on "Beaded streams of Arctic permafrost landscapes" *by* C. D. Arp et al.

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Response to BGD-11-C4441-2014

Summary of Comments on Beaded streams of Arctic permafrost landscapes

Comment from Referee:

General Comments: This is an important paper in that very little research has ever been published on beaded streams in the arctic region and yet these streams are one of the more common stream types in this environment. Until this paper we have known very little about their distribution and characteristics. The authors have done a nice job utilizing three nested spatial frameworks – from pan-arctic to regional to watershed – to explore the nature of beaded streams. The analyses are straightforward and the report narrative is reasonably clear, with appropriate figures and tables. There

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are, however, a number of revisions that need to be made before this manuscript is published, largely for clarity. These changes are detailed on the following specific comments with finer-scale editorial suggestions in the accompanying annotated PDF. One of the most important changes that needs to be made is to explain more clearly how the pan-arctic survey of Google Earth images actually proceeded. It seems, but is not clear, that a complete, manual survey of the entire pan-arctic region was done. This seems like a monumental undertaking and if that is what was done, kudos. But some other systematic sub-sampling survey was done, this was not adequately explained in the methods and should be. The scale at which the scanning survey was done should be identified. Was a consistent scale used throughout? If not, why not? The justification for the use of RWT needs to be clarified. RWT is not conservative it is degrades or sorbs onto OM and that needs to be more clearly stated. It would be helpful in the discussion about the stability of beaded stream structure to place this in the context of their stability relative to stability of other hydrogeomorphic features in permafrost-dominated landscapes. What is the turnover time of a thaw lake or a river meander? In the context of these other features, are beads more or less stable? I have noted a few suggestions to improve several figures. The manuscript should be thoroughly proofed. There were numerous grammatical errors, several of which have been noted in the annotated PDF. In addition I've offered several editorial suggestions.

Author's Response: We appreciate the reviewer's recognition of the value of this work in light of the abundance of these ecosystems in the arctic relative to published studies. We agree that there was need for improved clarity and numerous grammatical errors throughout and we appreciate the reviewer's attention in pointing these out. The three points raised here that need to be addressed 1) GE survey, 2) RWT tracer tests, and 3) comparison with stability of other thermokarst landforms are all important ones that we have taken care to address.

Author's Changes to the Manuscript:

1 – GE Survey: This survey was in fact a pan-arctic survey of all areas in the contin-

uous permafrost zone, but only areas with high resolution imagery, which particularly in Canada and Russia limited the actually area surveyed greatly. We explained this originally, but not in a quantitative sense and this was misleading. Because of this, we have added a table documenting these differences in areas surveyed and used these data to make estimates of the total number of stream networks and corresponding drainage densities. Additionally we have eliminated the pan-arctic map, because this could be very misleading if not taken in the context of available imagery (we wanted to add polygons to this map showing these areas, but the very patchy nature of this imagery made it very hard to see). Instead we added three images from Google Earth showing examples of beaded networks from different countries from high resolution imagery.

Text in methods now reads (P3, L64-83): The Circum-Arctic survey utilized imagery available in GE to identify channels with beaded morphology. This analysis focused on the continuous permafrost zone north of 66o latitude. We utilized the historical image browser function in GE to access the highest resolution imagery (< 5-m) possible for a given region. This analysis focused on portions of Alaska (U.S.A.), Siberia (Russia), and northern Canada totaling approximately 4.5 million km2. We found that most channels with beaded morphology could be identified when scanning images at 1:6,000 when the imagery was had a resolution of 5-m or finer and was mostly snow-free. The availability of high resolution, snow-free imagery in Alaska was quite good, covering 80% of the continuous permafrost zone surveyed. In Russia and Canada, the availability of such imagery was much lower, 11% and 9%, respectively, as of 2013 (Table 1). For this survey, zoomed in on each channel with beaded morphology for closer inspection and verification and marked its course at the furthest downstream point on the network of beaded channels. Surface elevation, latitude, and classes of permafrost ground ice were attributed to each point using thematic datasets for panarctic (Brown et al., 1998) and Alaska-focused permafrost and ground ice distribution (Jorgenson et al., 2008) and surface elevation. In order to compare among regions with differing extents of sufficient imagery, we extrapolated the number of surveyed streams based

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on the proportion of high resolution imagery available to estimate the total number of beaded stream networks in the Circum-Arctic continuous permafrost zone (Table 1). We additionally estimated drainage density of beaded channels based on assuming an average network length of 10 km, which results in only a broad regional average and definitely varies considerable on finer scales.

2 – RWT Tracer Tests: This is correct that RWT is not truly conservative and we originally neglected this in the text, which now reads (P8, L215-230): Rhodamine WT (RWT), a pink fluorescent dye, was used as a water tracer because it can be detected at low concentrations and only small quantities are required to reach target concentrations, which is an important practical consideration for remote field sites. RWT has low biological reactivity, yet does sorb to organic matter and begins photodegrading after several days of sunlight exposure at low concentrations (Vasudevan et al., 2001). Thus, RWT is not truly conservative, however is widely use to characterize channel hydraulics and transient storage processes, including previous work in Arctic beaded streams (Zarnetske et al. 2007). Based on targeted downstream peak concentrations of 30 ppb, we made pulse additions of RWT at reach heads and monitored concentration at the reach bottom using a YSI 6600-V2 water quality sonde with a RWT probe. This experiment typically lasted a day or longer to account for all tracer moving through the system. RWT tracer data were then fit with the model One-dimensional Transport model with In-channel Storage and Parameterization (OTIS-P) to estimate advective channel area (A), storage zone area (AS), dispersion (D), and the storage exchange coefficient (α) (Runkel, 2000). Percent RWT recovery averaged 81% with an average sorption coefficient (λ) of 1 $\times$ 10-5 used to account for this loss downstream.

3 – Comparison with other Thermokarst Landforms: This is a very good idea and we've added comparison with thermokarst lakes and alluvial rivers (P16, L458-464): For comparison to other thermokarst landforms, thermokarst lakes in this region also progressively expand their lake basins, 0.10 m/yr on average (Jorgenson and Shur 2007), but can drain catastrophically if a shoreline expands beyond a lower gradient or

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is breached by another lake or migrating river (Grosse et al. 2013). Alluvial channels on the ACP are considered highly dynamic often with very high rates of bank erosion due to interactions with permafrost such that major changes in channel course can occur over short time periods (Scott 1978).

However, besides this information we aren't aware of any quantitative data on TK landform turnover rate that could be incorporated (and no references were provided by the reviewer).

Comment from Referee:

Specific Comments Page: 5 I presume that only a subset of the total arctic area was survey. How were scenes selected for quantification? Were they randomly selected? What % of the area was sampled? Were all analyzed at the same scale?

Author Response: As detailed above we did actually conduct a full pan-arctic survey, but only where high res imagery existed.

Author's Changes to the Manuscript: see above changes with respect to GE survey.

Comment from Referee: Page: 7 If these are relevant they should be identified. "...as described below in the next section"?

Author's Changes to the Manuscript: Agreed and this was added (P4, L124).

Comment from Referee: Page: 8 Of a total drainage length of what for each watershed?

Comment from Referee: Page: 11 Vague. What measurements? All of this is true, but phrase does not make sense. If RWT photodegrades it can't be conservative. In addition, the primary complaint about RWT is the it does stick somewhat to organic matter. Thus, beads would be one environment in which this could be a particular problem. The authors could put bounds on whether this is a large problem or not by summing the mass flux of RWT to identify how much of the tracer that was added

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upstream was recovered downstream. It is important quantify this because a loss of tracer due to sorption or photodegration will appear as permanent loss of tracer (and water) from the system.

Author's Changes to the Manuscript: see changes above with respect to this concern.

Comment from Referee: Page: 12 But how were these "found"? Was the entire pan-arctic region searched quantitatively? Or was a subsampling regime used? If sub-sampling, how? The method of searching matters with respect to how best to extrapolate to the pan-arctic. If you look until you find beads, count them, and then extrapolate to the entire region, this could vastly over estimate the coverage compared to random sampling of the region to identify how frequently beaded streams arise in that landscape type. What method was used to search?

Author's Changes to the Manuscript: see changes above with respect to this concern.

Comment from Referee: Page: 13 It would be helpful to explain why this is perplexing to the authors, given that the following explanations seem reasonable. Is this a generally accepted statement? If so, an appropriate citation should be included. If not, the statement should probably be qualified; i.e., "may be"

Author's Changes to the Manuscript: We agree that this was poorly worded and have made the following changes to read (P11, L294-299) This lack of channels with beaded morphology on the outer coastal plain is perhaps unexpected, given the ubiquitous presence of ice-wedge polygons in which beaded drainage forms. We have observed however that most channels in this region tend to take a plane bed form without alluvial features, which may relate to very high pore ice content that in addition to wedge-ice makes soils in this regions extremely ice-rich, often exceeding 90% by volume (Brown, 1968).

Comment from Referee: Page: 15 Fig 1a does not really help me understand this relationship. I see no lakes on Fig. 1a.

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Author's Changes to the Manuscript: This is a very good point and we have added lakes to this figure (now figure 2) as well as showing the Fish Creek Watershed separately with lakes also indicated (Figure 3).

Comment from Referee: There appears from Fig 1a to be quite high densities in the southern finger of Fish Creek.

Author Response: The densities in that region are not actually that high, but the channels are very short due to the drainage area shape, so indicating the channels with points was misleading here. Instead we've modified this figure (now Fig. 3) to show the full beaded channel courses so this is clearer.

Comment from Referee: I don't think Fig. 5 really supports this statement.

Author's Changes to the Manuscript: We agree and have removed this reference and also modified the text to read (P14, L383-389): In the Fish Creek Watershed, most channels with small elliptical pools were located in the higher elevation areas associated with eolian sand and loess deposits compared to lower elevation marine sand and silt deposits. Whether this pattern relates to size and form of ice-wedge networks that develop in sandy soils or how eroding sandy soils moderate expansion by infilling pools or interactions with vegetation deserves further consideration. The other channel classes were more evenly distributed throughout the watershed and by surficial geology.

Comment from Referee: Page: 16 "10 per 100 m" according to Fig. 3?

Author's Changes to the Manuscript: This has been corrected and thanks for catching this error.

Comment from Referee: Page: 17 More useful to report ranges as for Q?

Author Response: We've actually decided to eliminate reporting discharge relative to slope and drainage area, because this was done during summer baseflows (as opposed to using a standard discharge such as bankfull). Plus only comparing slope and drainage area is simpler and more in line with other analyses of channel organization (now Fig 7 is one panel instead of two).

Comment from Referee: Page: 18 "scars"?

Author's Changes to the Manuscript: Corrected and agree, these are typically very friendly landforms.

Comment from Referee: Page: 19 "relatively deep and sinuous"? Quantify average values. I agree with the general sense of these conclusions. But a 20% turnover of the beads in 60 years - on a geomorphic time scale - seems significant. In this same area, what would the turnover time be for a river reach; i.e., a full period from point bar to point bar? Is the beaded "transformation" relatively slower, faster, or similar. Also, were the 18% of pools that changed at the smaller, average, or larger size? What percentage of the total pool area is 10.8 m2 and 19.7 m2?

Author's Changes to the Manuscript: This is a good idea and we have added sinuosity here (P16, L438-441): It then flattens greatly to <0.01% over the last 5 km and becomes quite deep (exceeding 5 m in some pools) and very sinuous (2.3) with high, regular banks before its confluence with Fish Creek. We understand the ambiguity in this paragraph and have revised this accordingly with the following clarifying statements (P16, L457-457): Thus our analysis suggests progressive expansion of these thermokarst landforms, yet the channel course appeared entirely unchanged over this period. (P17, L489-492): We suggest that beaded channels may evolve in a similar manner with most pools gradually expanding and some contracting with changing vegetation. Such behavior seems particularly apparent in viewing coalesced beads of some channels (Fig. 4c).

Comment from Referee: This definition of a gulch (which is not a technical term?) should appear earlier in the manuscript and then would not have to be reported here. The sentence is awkward as is. Author's Changes to the Manuscript: We think readers generally understand what "gulch" implies, but have added clarification in methods as

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well as referred to this area as the riparian zone instead (P6, L170-172): The channel gulch / riparian corridor was also delineated for both periods based primarily on the darker (greener) signature of taller sedges, willows, and dwarf birch and moister understory bryophyte communities.

Comment from Referee: Page: 20 Confusing. Is "medium" intended to be a size designation here. The transition from "sand" to "sediment" is not helpful. Sand is a sediment. Is the transition from an "organic poor sand to an organic rich silt" (or peat)?

Author's Changes to the Manuscript: This section was not well written and we have gone thorough it to clarify all such issues (P17, L496-517): Analyzing the stratigraphy and geochronology of sediments in a large pool of Crea Creek may attests to the timing of stream channel formation and the depositional environment since initiation. A fibrous organic-rich layer with abundant terrestrial plant material separated the transition from organic-poor medium-grained sand to organic-rich silty sediment that is the uppermost unitâĂŤwe interpreted this layer as basal sediments that were dated to 9.0 ($\pm$40), and 13.6 ($\pm$215) ka cal years BP (Fig. 10). The terrestrial macrofossils (shrub twigs) in this fibrous unit and the two dates that span 4 ka suggest this layer may have been a terrestrial soil that persisted for millennia on top of eolian or alluvial sand deposits, but predated the initiation of the beaded stream pool. Alternatively, this layer may represent the depositional environment of an early stage of the beaded stream pool where terrestrial vegetation was overhanging and being deposited, and adjacent soils were being eroded by ice wedge degradation and supplying a range of reworked material with different 14C ages to be deposited onto this fibrous layer. Regardless, we interpret the 9.0 ka moss macrofossil sampled from the upper portion of the fibrous layer to be a conservative upper limit age on the initiation of the beaded stream pool. At this time, we do not know whether the lower limit of this age estimate is near the 9.0 ka time period, or represents the late Holocene. The large age-gap from 9.0 ka at 42 cm to $\sim$0.7 ka at 22 cm suggests that either a water-level lowering event caused a hiatus of sedimentation through much of the Holocene, or that high flow events or other

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processes eroded the sediment deposits representing most of the Holocene (Fig. 10). However, there was no preserved wetland or terrestrial soil layer interrupting the gyttja unit, which would have accompanied a water-lowering event. The Crea beaded stream pool we examined appears to have had episodic sedimentation during the Holocene that is periodically eroded by either high flow events, or ice scouring.

Page: 30 Personally, I would switch the order of these last two paragraphs to more closely parallel the structure of the paper.

Author's Changes to the Manuscript: It took us some though, but in the end agree this it is better reversed and have done just this (P26-27).

Comment from Referee: Figures Page: 44 The finer lines and shading in this figure are hard to see. Need to be bolder.

Author's Changes to the Manuscript: Thanks for noticing this and have improved quality of this and several other figures.

Comment from Referee: Page: 50 It would be helpful to put the discharge below each date. This should be italicized and not bolded. Otherwise it could be interpreted as a missing panel C.

Author's Response: We ended up decided that this figure (now Fig. 14) was too complicated and only show data from mid-July and compare it now to an alluvial channel with similar discharge (all experimental data including Q are given in Table 3).

Comment from Referee: Page: 51 Perhaps better to refer to the middle bead as having "cap" ice rather than "floating" ice. My guess is that the ice is pretty firmly attached to the edges and not really floating freely, as this suggests. Author's Response: We get this, but floating ice refers to the presence of water under the ice and we think most readers familiar with Arctic systems recognize this and more so than cap ice. Plus we discuss floating ice in the text and if this is confusing to some readers, we think this will provide an explanation.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C6693/2014/bgd-11-C6693-2014-supplement.pdf

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