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Interactive comment on "Near-surface permeability in a supraglacial drainage basin on the Llewellyn glacier, Juneau Ice Field, British Columbia" *by* L. Karlstrom et al.

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The three reviews to our manuscript (by J. Walder, M. Pelto and an anonymous referee) provide a number of constructive criticisms and suggestions for improving clarity. All reviewers agreed that our methods for estimating permeability and the time variation of near-surface hydraulic processes are novel and may be applicable to other sites. Our short duration field study, still a few weeks and great expense, was intended to develop and test these methods. The success of initial tests presented in this manuscript will be useful in developing more extensive future studies (for example, a season long instrument deployment or an integration of our field measurements with remote sensing

data) that will build on our existing work. However, our measurements and modeling results presented here stand alone, and if correct have implications for observations and modeling of glacial hydrology in larger scale systems. We thus feel that our work (suitably revised in light of reviewer comments) is timely and contributes significantly to the general understanding of time varying near-surface glacier hydrology.

There are three common themes to the reviews. 1) The spatial characterization of the drainage basin and channels, and description of the study period in the context of annual variability at the Llewellyn glacier, should be more thorough. 2) Methods and modeling details are not clearly presented, and some model assumptions require better justification. 3) There are a number of relevant uncited references that provide more context for our study.

In response to reviewer comments we have restructured the manuscript, omitting extraneous material that does not contribute to our primary results. The revised manuscript is significantly more focused and clear. Below we present a point-by-point response to all individual referee comments, along with detail of our manuscript revisions. We have re-ordered referee comments according to similar themed comments (each referee separated for clarity). Comments are in bold font, response in regular font, figure and equation references are with respect to the original submission.

J. Walder comments:

You state that you "observed" some supraglacial streams. Did you map the channel network? Did you do any structural mapping of the glacier?

Page 5284, line 22: "hummocky glacier surface topography in the upper parts of the drainage basin that reflects competition between localized erosion by streams and large scale surface lowering." This is a truism. Also, the figure presented is inadequate for giving the reader any sense of what the supraglacial drainage NETWORK looked like.

Page 5286, line 25: Sorry, but "[c]oarse survey of the study supraglacial drainage basin", and a photograph, do not give the reader any confidence that you have adequately characterized the supraglacial drainage network.

We acknowledge that the description of our study site could be improved. We have split the map in figure 1 into a separate figure, and added a new figure to clearly show the drainage basin and individual streams of our study area as well as features that are relevant to our characterization of the basin. We indicate the take off point for drainage basin survey transect profiles plotted in figure 2, and the stream in which we placed the DTS instrument. Detailed mapping of the network would require documentation of the temporal reconfigurations of the network, and a longer study time to see multiple cycles of channel formation/abandonment. We have shown that such reconfigurations to the network do occur on a several-day timescale (figure 2.c and new figure panel), itself a novel observation that can be expanded on in a future study. Our five transects moving upstream in the drainage basin, while not a substitute for a detailed survey, nonetheless convey the quantitative information from a detailed survey that is of most relevance to our analysis, namely, channel spacing, width/depth of channels, and a measure of their persistence in time.

Page 5287, line 9: "The underlying ice is relatively fracture free, so streams do not exhibit structural control." This is not exactly a substitute for a structural map of the glacier. The streams may well not be structurally controlled, but what you have presented does not demonstrate that.

We did not conduct a detailed structural map of the glacier because fractures in this marginal zone are uncommon and their spacing was many times that of streams or of typical meander wavelengths. However, we acknowledge that the statement in question is not well justified without such a map. We have reworded to state rather that meander widths/wavelengths (which are measured) are consistent with the scaling exhibited by other supraglacial streams (a scaling valid for streams without significant structural control).

Page 5284, line 15: "Atlin, BC" is telegraphic. Write out "British Columbia, Canada".

Fixed.

Page 5284, line 18: "a layer of weathered, partially melted surface ice." Not sure what this is supposed to communicate. You were at the ELA, so exposed ice was necessarily experiencing melting and "weathering" (and what exactly is meant by "weathering"?).

Reworded to state that weathering means loss of ice mass through partial melting or sublimation. We have added a reference (Muller and Keeler, 1969) that provides an example of similar glacial surfaces in other settings.

Page 5285, line 2: "Stream profiles were conducted"? By the way, you provide nothing in the way of a defined coordinate system.

Page 5287, line 12: "choosing a reach with slope of 0.05 and of typical size for the area." How is one supposed to know what is "typical size" if you have not mapped the drainage network?

Page 5287, line 22: I don't really understand what your coordinates theta and d are supposed to be. The distance d is measured from what datum, say? As for the wavelet transform, I admit to ignorance as to what this might be, but I wonder whether other readers might be equally in the dark, so I suggest a brief explanation of what it is that the transform does and what the transform's utility is.

We have removed the section on wavelet analysis of a stream profile as it is tangential to the main story. We have summarized some of the more relevant results in a statement or two within the results section, but feel that the manuscript focus benefits from this omission.

Page 5285, line 7: I'm wondering how you went from presumably measured sur-

face velocity to a depth-averaged velocity.

Added a sentence explaining our velocity measurement technique. We used an Acoustic Doppler Velocimeter for velocity measurements, and the standard hydrologic measurement taken at 0.6 times the depth as the average velocity in the stream.

Page 5286, line 7: Are the isotopic data used anywhere in the present manuscript? If not, there's no point in presenting this material.

Agreed. We now omit this data to improve the manuscript focus.

Section 3.1: The use of the perched water table analysis is clever, but you need to makeit clear to the reader, who may not be familiar with the Dupuit-Forchheimer approximation, exactly what you're doing here. And you need to actually estimate sublimation and evaporation rates and show that they're negligible compared to infiltration, instead of assuming that.

We have added a sentence describing our use of curve fitting to the water table profile as a means of estimating permeability. With regard to the last comment, we agree that estimating evaporation/sublimation is useful for justifying our approach. We use average relative humidity (78%), temperature (7.8 C) and wind speed (6.8 m/s – note we have corrected a typo in this reported value in the manuscript) over our observation period to estimate a Latent Heat flux of -67.7 W/m^2. This corresponds to evaporation/sublimation rates of $\sim$ 1 mm/hr, which is less than 1% of our inferred daytime melting rate. We have included this estimate in the text.

Page 5286, line 13: Were these water-level measurements made along just a single transect perpendicular to the local stream thalweg? Or did you make measurements at some regularly spaced distance along the stream?

We made other point measurements at different locations along the same stream that confirm the water table was present along the study reach. However, this was the only transect taken. A more complete documentation of the water table and its time

variation would be quite interesting and we intend to dedicate a future trip to such measurements (but would require a different approach to documenting water levels or more people to help with the monitoring). However, more account of the hummocky surface topography would be required for such a study, and we feel this is outside the scope of our work here.

Page 5288, line 1: I'm confused. How is it that your analysis of the along-flow properties of a single stream can possibly tell us anything about the properties of the stream network? I could well be misinterpreting what you've done, of course, but some clarification is in order.

We are here extrapolating the observation that dominant wavelengths observed in our detailed analysis of a single channel profile are consistent with meander scaling of other streams without major structural influence to the rest of the network. We feel that this is justified, given that the stream profile chosen is in not extraordinary for the basin (now hopefully more clear to verify in the added figure). However, this section has been removed to improve focus.

Page 5288, line 10: I fail to see how anything discussed up to this point allows you to draw this conclusion. You've said nothing about melting rates.

Melting of the glacier surface is of similar magnitude to local incision by streams here, as evidenced by the high number of abandoned channels in the basin and the rapid reorganization of drainages that we observe. However, we have omitted this section in the revised manuscript for clarity.

Why assume the channels are separated by 2 m? Again, you have not presented any data about drainage system morphology to support this claim.

We pick a drainage divide (half the channel spacing) that is on the small side but none the less consistent with spacing values reported in figure 2.a.

Page 5288, lines 27-28: unclear if you are relating observations here or just an

assumption for the mathematical model.

This is an observation. Changed for clarity.

By the way, the scenario considered here and represented by equation 1 is, I believe, well known in the literature on irrigation. You could probably consult that literature and just cite solutions.

We agree that this scenario has other applications in hydrology. However, we feel that readers of The Cryosphere may not all have a similar background, and thus it is useful to restate the problem. We have added a statement that similar flows governed by similar equations occur in soils, with an example reference.

Equation 3: define h sub 0 and explain how you went from the Dupuit equation to this linearized form

Done.

Page 5290, line 8: porosity was estimated? Or assumed?

Assumed, based on values reported in the literature. Because porosity appears in the model equation alongside melt rate N(t), for which there much greater uncertainty (in maximum magnitude as well as fluctuation in time), we consider our estimate reasonable for the present study.

Page 5291, line 6: dimensions given for diffusivity are wrong. But more broadly, I don't follow the discussion here. Why is a thermal diffusion time scale correspond- ing to stream DEPTH relevant? I would have thought stream temperature reflects an advective/diffusive balance in the streamwise sense.

Fixed typo in diffusivity dimension. It is correct that this estimate could be evaluated in a more rigorous fashion. We can calculate a Peclet number for the channel: cross-stream gradients in temperature will be greater than downstream gradients for a D = 1 cm depth channel being heated by the sun. Assume downstream scale length

of L = 1-5 m (typical dimension of hummocky terrain high in drainage network) and velocity scale of V = 1-10 mm/s. Then a Peclet number is Pe = V $^*D^2/(L^*\kappa)$, with κ = 1.7^*10^{-7} m^2/s the thermal diffusivity. Pe = 0.1-6. The fact that Pe <= O(1) justifies the consideration of diffusion through the stream depth for heating of water. We now present this argument in the text.

Page 5291 bottom and page 5292 top: I don't follow the argument here. Please present the mathematical model that you are working with to get temperature.

We do not model water temperature. Instead, we rely on the scaling arguments above, which demonstrate that a characteristic stream water heating time is much less than the diurnal solar heating period. If stream water starts at 0 C upon entering the stream, then the observed water temperature variability likely comes from solar heating during the day.

Page 5292 lines 2-3: What is the reference to "pore pressure diffusion timescale of supraglacial channels" supposed to mean?

Misuse of terminology. We mean that the lag likely corresponds to a porous flow transport timescale, from a perched aquifer into the channel.

Anonymous referee comments:

1) There is a wealth of data (or at least methods) described in this paper, however, I found that in places this lacks clarity. For example, the section on stream sinuosity, while interesting, might appear somewhat tangential to some, particularly as the novel elements in the paper appear to focus on ice permeability – for which the hard-won data relating to discharge, water temperature and near-surface ice water table heights are highly appropriate. The brief insert relating to the isotopic composition of the meltwater becomes masked in a bracketed note in the results section, and appears no further. The methods for recording discharge are developed in the text, but other than singular values for peak

discharge, there is seems to be a lack of indication as to whether field observations did support diurnal variations, and whether changes in discharge relate to changes in isotopic composition or water temperatures. It felt as though there were a number of relationships that were left under-explored or perhaps lacking in explanation. A clearer focus in the paper might be beneficial.

2) Relating to this, the robustness of the methods and data could be better described. The time-frame of study, the number of measurements taken, the uncertainty in data described (not just constituent components) remain unclear in places. The manner by which discharge was estimated remains rather unclear; the method for monitoring ablation would seem to be unreliable; some of the data is not well explained. The authors present a diurnal cycle, but it is not clear if this is an average of the 4-day period noted, or if this is a single 24-hr cycle. To help readers better understand the field-methods used, a little more in the way of detail would be helpful.

Comments 1 and 2 here are related. As this referee notes, a primary strength of our manuscript is method development. We hope that, rather than convincing readers that we have completely characterized the study site (we haven't, and could not with the limited budget and time of our pilot study), we present and validate data collection and modeling techniques that can be utilized by future studies of our own and others. For example, here we present the first use of a Distributed Temperature Sensor in a supraglacial stream, and a novel application of perched aquifer hydraulics to estimate field scale permeability.

Overall, we agree that clearer focus would benefit the manuscript. We have removed discussion of the isotopic data completely. The discussion on sinuousity has been largely removed with the exception of a few main results that seem relevant to our primary focus. We have added several sentences discussion about our discharge measurement techniques, the time frame and uncertainties in the methods section.

3) The "weathering crust" of glacier ice surfaces has been described as well as its close links to synoptic conditions (see Muller and Keeler, 1969, J Glaciology). Although the authors touch upon this, utilising this older paper may provide a useful context, particularly given the importance antecedent conditions will have on the discrete measurements made. Similarly, there are sources of information on temperate or weathered ice permeability, and near surface water tables or hydraulics, which do not seem to appear here, even as comparison – in particular, to name a few, Thomas Schneider's work on firn on Storglaciaren, a range of Scott Munro's work based at Peyto Glacier, and Gorow Wakahama's work in the 1960s on ice permeability. Reference should be made to these pioneering studies which may now have increased value given the increased need to improve understanding of surface ice permeability and hydraulic conductivity.

We agree that better reference to the literature on ice and near-surface glacial permeability could be made, and now make reference to the relevant papers mentioned above as well as others that are relevant. We note that field scale permeabilities inferred by Schneider are consistent within an order of magnitude with ours (although our permeability values are larger, but we note that our study site is weathered ice and not firn, per se).

M. Pelto comments:

Karlstrom et al (2013) utilize some innovative techniques to assess near surface glacier ice permeability, water table level and supraglacial stream temperature. These methods applied over a significant period of field work and placed in the larger weather and glacier surface condition context could be important. At this point the four days of field work do not provide a robust enough data set. Further this data set is not providea meaningful context. I encourage the authors to employ these methods again for a longer period, and to utilize both local weather records and satellite imagery to aid in setting the context. Below are larger general points to address.

We agree that 4 days of data do not constitute a robust dataset for documenting the evolution in space and time of hydraulic geometries and properties. However, as outlined at the beginning of this response, the primary purpose of our manuscript is method development. In that respect we have shown the data collection and modeling methods to be successful, that the results consistent with other similar studies of field scale permeability, and the implications for large scale hydrologic modeling of supraglacial hydrology are potentially important. Thus our study should serve as both a motivation for longer term, more thorough study of the Llewellyn hydrologic system, and an application of our techniques to other glaciers and ice sheets.

1) What days encompassed the field season? What are the weather conditions for this period. I assume the field work occurred in the first few days of August, 2010. If so this is an exceptional period of weather, particularly August 4, 2010. Atlin, BC daily weather records note the average daily maximum in August is 17.4 C, with an all-time maximum of 30 C. From August 1-4 daily maximum temperatures were 22 C, 25 C, 26 C and 28 C respectively, the latter just two degrees short of the August historic maximum at this long term station. The daily minimums were also well above average. This sets the stage for an unusual level of ablation in the study area. This was observed on Taku Glacier as well with ablation being higher in August 2010 than any other period observed during the last decade.

We now make reference to the unusual nature of this study period (although our study on the Llewellyn was August 4-7), and reference the similarity to the Taku glacier.

2) Supraglacial streams in this region begin to develop until after snowcover is lost and tend to become increasingly incised. In the specific area of the field study satellite imagery indicates snowcover still exists on July 8, nearly snow free conditions by July 19, and on August 1 no patches of snowcover in the vicinity. Utilization of satellite imagery can readily identify the duration of bare surface conditions before the field season began. The surface ice character will

change with the progression of the melt season, leading to changes in permeability.

This season-scale view of time varying supraglacial hydrology in our study area is fascinating, and we believe that a season long study that includes remote sensing data as well as instrumental deployment and repeat surveys would be very fruitful. Our pilot study provides good motivation for such an effort. We now note that hydraulic properties as well as drainage organization vary significantly over the melt season, and that our study period likely coincides with an early stage of channel development.

3) The field location is on the glacier edge, has fewer crevasses, lower albedo and higher slopes than the adjacent main area of the glacier. This needs to be better characterized. McGrath et al. (2010) provide an excellent Figure 1 showing their basin of study and the stream network. The field site in this study is not near the annual ELA which typically is at least 5 km and 300 m upglacier, as was the case in 2010.

We have included a new figure that better characterizes the study area and drainage basin (see response to similar comments by J. Walder), and have revised our discussion of the ELA. We now focus rather on our site's proximity to the Transient Snow Line (observed upon travel to the Llewellyn to be 1-2 km up glacier and 75-100 m above our site), for which we also make reference to other Juneau Icefield study sites.

5284-21: Field observations are too spatially and temporally limited to assess how ephemeral the streams, in fact most field work on the icefield indicates streams typically are longer lasting. I had the chance to work with Marston (1983) during this fieldwork for the referenced paper. This included some work in the same area of the Llewellyn Glacier that was too brief to include in that paper. Marston (1983; Figure 7) notes that downcutting of the supraglacial streams exceeds lateral expansion, which led to channel deepening, increased sinuosity and persistence of the channels. Immature channels have the chance to not be

persistent, but as the melt season continues the channels tend to incise further. This only occurs if downcutting exceeds surface melt rate. The fact that the 2010 short field season likely coincided with exceptional ablation may have led to difficulty in downcutting exceeding surface ablation particularly in an area of lower albedo near the medial moraine.

Because our study here focuses on novel method development, the 2010 field season is sufficient to demonstrate the utility and success of our techniques, which may have applicability to other glacier systems. We leave to a future, longer and more spatially extensive study the task of better characterizing of the seasonal variability in near surface permeability and supraglacial stream networks on the Llewellyn glacier. We disagree that our study period was too short to document the ephemeral nature of small stream channels: our drainage basin survey (and new figure panel documenting an abandoned channel) demonstrates that channel abandonment can occur and was common in the study drainage basin. This observation, combined with high ablation rates that erase the record of channels abandoned for more than one day (ablation rates are 5-10 cm/day – greater than the depth of channels in our study basin), imply that small streams such as we studied are highly ephemeral.

4) Reference should be made to ablation measurements in the region, some of which in fact coincide with the study period. They are not in the same location and do not substitute for field area measurement, but provide necessary context. Mernild et al (2013), Pelto (2011) and Pelto et al (2013) provide overall assessment and more specific 2010 assessment of ablation in the area from August for Taku Glacier and Lemon Creek Glacier.

We now reference other ablation measurements in the region during the study period.

5) Reference needs to be made to the recent paper exploring supraglacial stream development from a modelling approach (Jarosch and Gudmundsson, 2012). How do your observations compare to their model results in Figure 2,3 and 5.

The Jarosch and Gudmundsson (2012) work does not consider the competition between glacier surface lowering and localized incision, thus does not seem to be relevant for the streams we observed. We expect that for systems in which the glacier surface lowering is comparable to the incision rate, streams will not evolve past the stage in figure 3.a of their work, and hence we cannot make a meaningful comparison.

6) Some of the methods are not adequate as described 5285-7: how can a tape measure be used to assess stream depth? 5285-8: Ice screws not an accurate means of ablation assessment. How do you extrapolate from this point measure, realizing that the screw during emplacement disrupts ice locally around screw.

The depth measurement methodology was not well presented. We in fact used the measuring ticks on an Acoustic Doppler Velocimeter (that used for velocity measurements) to measure depth concurrent with our measurements of average stream velocity. This typo has been corrected. We agree that the method used for estimating ablation is rough. However, we acknowledge this significant source of uncertainty and in fact couch our results (e.g., figure 7) in terms of uncertainty in ablation rate. We also note the order of magnitude similarity to other concurrent ablation rate estimates on the Lemon Creek glacier.

5287-14: This requires a velocity of 8 m/s, which is unrealistic. The hydraulic geometry must be assessed for general conclusions can be drawn about evolution of the channels, note for example Table 1 from Kostrzewsk and Zwolinski (1995)

This is a typo – peak discharge should be 0.013 m^3/s which corresponds to the measured velocity of 0.8 m/s. We have referenced a more complete compilation of hydraulic geometry in Knighton (1981), that provides a more thorough dataset with which to compare to ours (although we note that hydraulic geometry of supraglacial streams is in generally very poorly documented due to the likely significant time-evolution that this referee points out in earlier comments).
