

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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GENERAL REMARKS The submitted paper by Karlstrom et al. presents interesting approaches to estimating the near-surface hydraulic permeability of glacier ice. These approaches are likely to be applicable more broadly and for that reason need to be clearly explained. Unfortunately the authors do not achieve the necessary clarity throughout. For example, the assumptions embedded in mathematical models are not always clearly explained. There are some other odd oversights: for example, the authors claim certain average properties for the supraglacial drainage system, but present no maps or photographs of the glacier surface to give the reader a sense of the actual

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drainage system. Elsewhere they provide extraneous, distracting details about methods or measurements but never make use of those details. I believe the paper would be greatly improved by some rigorous editing. The authors should carefully consider the review by Peltó, who raises issues quite different than I do. As a caveat, I wish to state that I have not been actively involved in glacier-hydrology research for at least a decade and am admittedly not familiar with many recent developments in the subject.

SPECIFIC REMARKS 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 15: “Atlin, BC” is telegraphic. Write out “British Columbia, Canada”. 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”?). 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 5285, line 2: “Stream profiles were conducted”? By the way, you provide nothing in the way of a defined coordinate system.

Page 5285, line 7: I’m wondering how you went from presumably measured surface velocity to a depth-averaged velocity.

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

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?

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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.

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.

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 θ 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.

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.

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.

Section 3.1: The use of the perched water table analysis is clever, but you need to make it 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.

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

Interactive comment on The Cryosphere Discuss., 7, 5281, 2013.

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