

Review of HESS-2010-14

The goals/objectives are unclear, the field methods are suspect and the description of analyses somewhat vague, the results are poorly (and incompletely) presented and segue into a discussion that becomes almost an internal dialogue in which the author pursues various trains of thought to fulfill an unexplained research goal. While the paper topic is of interest, and several theories are outlined in the Discussion that are quite original and of significance to glacier hydrology, these theories are based on poor data and analysis – thus it is difficult to assess their applicability and/or validity. I suggest that the author collect additional datasets in the future (applying more rigorous and repeatable techniques) to support the theories developed within this manuscript..

The abstract outlines the available data – where really it should be outlining the research question and results. This problem continues into the Introduction. Nowhere is it stated that this study pertains to alpine glaciers (this is a key omission as meltwater runoff delays from Arctic glaciers are a function of quite different driving processes), and nowhere is the key purpose of the study/experiment outlined. Thus the background information provided in the Intro seems somewhat random, as there is no context in which to place it. While I did determine that the timing of meltwater delay on/within the glacier was the goal of this study, a key reference on this topic was missing: Neinow et al. 1998.

The study site description neglects all mention of the Peyto Glacier itself, and the significant amount of work the author has completed there in the past. This would have helped provide context for some of the energy balance (EB) measurements described later in the ms, and perhaps could have precluded the need for explanation of these data, as it is stated on p. 1576 that the results of the EB measurements from the study period are the same as results found previously on Peyto. It would also have provided context for the weathering crust theory – as previous studies by this author on Peyto have suggested the importance of the weathering crust for a range of surface processes.

The field methods are somewhat simplistic. “The loosely defined limits of the basin were obtained by instructing field assistants to follow the supraglacial stream network upstream to its apparent limits”. So where are these ‘apparent limits’ and how were they defined? If the author sent a different set of assistants – or if he himself completed the same task – would the same basin be defined each time? Plus the GPS unit used is described as a hand-held unit – which has an accuracy of +/- 5-10m. In the context of this small of a basin, this error could be significant. Figure 1, which is meant to show the basin, shows hardly anything. LiDAR data are available for Peyto and could be used to produce a DEM with a plotted outline of the ‘loosely defined basin’ overlain on it. What about the supraglacial stream network within that basin – was it not mapped and plotted to determine hydraulic gradients and drainage patterns?

“Data were selectively obtained by focusing on fair weather periods”. By what criteria were these periods defined? What was the rationale for only selecting these periods? It would have helped to see a figure that plotted T , P and K_m for the entire season, with the ‘fair weather periods’ labeled to provide a sense of how they compared with the rest of the season.

“Microbasin discharge was measure by installing a stage level recorder.” What make and model of stage recorder? Why was this used and not a transducer? “A stage-discharge relationship was obtained by the velocity-area technique, using floats to estimate velocity”. If a current meter couldn’t be used due to logistical constraints, why not use salt slugs? These are far more accurate than floats, and no mention is made of the errors inherent in calculating Q from surface velocities.

No mention is made of how x-sectional area of the channel was calculated. No explanation of how often velocity was measured with floats to create the rating curve. There will also be significant error from the stage recorder melting into the ice surface while measuring stream level – this problem could have been avoided by using a transducer instead (and would make the calculations of Q described later in the ms more robust).

“:Micrometeorological instruments were installed near the basin outlet” – provide some coordinates, an elevation, some kind of reference to the glacier as a whole. Also would have helped to have a table showing all met instrumentation, height above the ice surface at which it was installed, and accuracy. No mention of how flux data from the 4-level u , T and RH system was corrected for surface lowering throughout the season.

Theoretical approach – this was difficult to evaluate given that the author failed to provide clearly defined goals and objectives. If it's the surface runoff delay that's of importance, the detailed discussion of EB components is somewhat extraneous. While they factor into the production of melt, the focus here is more on the meltwater runoff – for these EB components the author could have just summarized results of his previous work on Peyto. The ms would work better if the theoretical approach started with the discussion of meltwater runoff, and use the EB info only to explain variations in runoff. Thus the beginning of Section 3.2 should really have been in the introduction somewhere, or in the study site section, so the reader knows exactly what we're dealing with and how it may be addressed throughout the ms.

The optimization techniques applied to surface runoff were only vaguely described which makes them somewhat suspect. Why use only RMSE to assess differences between q and q' – why not Nash-Sutcliffe or something similar? This should be explained. “The optimization procedure consisted of first adjusting the measured ablation rate... Then h was adjusted...” – adjusted how? What quantifiable approach was used to maximize the fit between q and q' ? “Finally, q_0 was set to achieve the smallest RMS...” – how was it set? What range of values was assumed appropriate? How physically representative are the resulting values? I would like to see a table that shows the adjustments to h and q_0 required in each time period (out of the 4) to minimize the RMSE (and include the RMSE in that table).

Section 4.1 – why does this start with the final measurement period out of 4? And why start by discussing energy exchange at the surface when the purpose of the ms is to discuss delays in supraglacial runoff? In fact, none of the other measurement periods are plotted, only the fourth is shown.

Section 4.3 – brings in additional calculations from Hannah and Gurnell – these should have been outlined in the Methods. The results from applying this reservoir approach should also be presented in a table for easy comparison with the Linsley et al. approach from p. 1575.

Finally in ln 5 of p. 1579 the author mentions the weathering crust. If this was the focus of the paper, then it should have appeared immediately in the intro. The author could have prefaced it by mentioning his previous work on weathering crusts, and his intent to apply that knowledge to a study of delays in supraglacial transit times.

Finally in the conclusion the author brings up the idea of basin shape as driving response time. This should not appear in the conclusion, and shows that the structure of the paper is fundamentally

flawed. This should have appeared in the discussion – which was a hodge podge mix of results/discussion and internal musings.

While I'm keenly interested in some of the conclusions the author has drawn from this research, and I think they would make a good contribution to glacier hydrology, they are fairly speculative given that the field and analytical techniques are not entirely robust. The ms itself also requires a major overhaul for structure, content and clarity.