

General Reply

In reviewing the comments I am reminded of the story about a bagpipe contestant at Scottish festival who, upon what asking the judge what he thought of his execution, received the reply: 'I am all in favour of it'. The good news among the comments is that they will greatly help in revising the paper and that one referee finds the results to be interesting. That, plus the fact that one email respondent found the paper to be fascinating, even inspiring, persuades me to further develop the work, subject to the advice of the editor. In so doing, however, it is clear that the major overhaul requested by Referee #1 is absolutely necessary.

I will begin by stating the question in terms of comparative runoff delay from two differently defined types of supraglacial microbasin: 1) one for which shape and size are predetermined by the investigator; 2) another for which shape and size are discovered by exploration of the glacier surface. This is also the point at which to include the thoughts expressed in response 12 to Referee #1. In so doing, reference to the second basin type as being 'loosely defined' seems colloquial, so reference to precisely and imprecisely defined microbasins would be better.

As to where to obtain data for a precisely defined microbasin, there are data from my doctoral study (Figure 1, below) which were collected but never published because I was more interested in boundary layer work. Shown here is one of only two periods that extend beyond part of a day, but the interesting point is that a reasonable delay of EBflow seems to be achieved by setting runoff delay in equation (3) of the paper to one hour. In revision, I will explore what my other data sets have to offer in this regard, and incorporate them as part of a comparative study of precisely and imprecisely defined basins on the same glacier, where energy exchange techniques are similar, as are some aspects of the runoff measurement procedure (Figure 2, below).

Specific replies to Referee comments follow, comments for which I express my appreciation.

Replies to Referee #1

1. 'The goals/objectives are unclear...' Points raised here are addressed in my general reply.
2. 'The abstract outlines...' The research question is whether a microbasin that is loosely defined (having no pre-determined shape) as the area apparently covered by a supraglacial stream network, produces a different runoff response than a microbasin that is defined as a precisely known area of predetermined shape. The abstract will be rewritten accordingly and the implication made in the last sentence will be removed from the paper because it is clear to me now that I was too impressed by runoff delay and not sufficiently concerned about diurnal runoff range. Subsequent work with a basin model and historical outflow data suggest that days of lag, not hours, are needed to produce realistic outcomes. The findings may have implications for diurnal variations in subglacial water pressures, but no more than that.
3. 'The study site...' I agree that there needs to be better background development, including the fact that Derikx's work was conducted here.
4. 'The field methods...' Yes, they are simplistic, which is why I use the term 'loosely defined'. Given the limitations of a hand held GPS, the idea here was to capture the shape of the basin, if not a precise definition of its area. This was done two ways: 1) by following the apparent

microbasin perimeter from stream mouth up one side of the basin and back down again on the other side (the result shown); 2) by locating the main stream and its tributaries within the microbasin, the result of which will be added to the diagram to show the network as well as could be observed in the field. A LiDAR survey was done the year before but the likelihood of finding a match between this and a field inspection is small, given how ice surface conditions change from one year to the next.

5. 'By what criteria...' This is part of a boundary-layer experiment for which the criteria were warm, dry and sunny weather that were expected to be especially good conditions for katabatic wind development. Given that these are also good melt conditions, it seemed appropriate to look at surface runoff as well. A season T, P and K_{in} diagram is an excellent suggestion and readily done from the base camp weather station record.

6. 'What make and model...' Like the psychrometer system stated on p. 1573, this is a custom design from earlier work, its use determined by the fact that it was readily available on short notice. There is no question that the use of floats can be regarded as an exercise in amateur hydrology but, somewhat to my surprise, the exercise did produce a stage-discharge relationship that is comparable in shape to that obtained for a weir box in my 1971 work (Figure 2, below). The different Y-axes for the 1971 and 2008 discharges reflect the V-notch of the weir box.

7. '- provide some coordinates' Some of this information is in Figure 1, but this diagram must be reworked, at least eliminating the picture of the instrument mast (as only the 1 m u, T and T_w were actually used) and expanding the microbasin diagram to show more detail, such as GPS elevations, however approximate they would be. Given the shape of the basin, an exaggeration of the Easting axis also seems advisable. There is no need to correct the fluxes for surface lowering because this is a floating micrometeorological instrument system.

8. 'Theoretical approach...' Yes, this has the appearance of being scattered through the manuscript. This will be gathered together and reorganized around the theme of runoff from precisely and imprecisely defined supraglacial microbasins.

9. 'The optimization techniques...' I am not sure that anything more than RMS minimization is needed for a study of this type because its purpose is essentially exploratory, but I will take another look at Nash and Sutcliffe. I agree on the need to tabulate the h and q_0 adjustments.

10. 'Section 4.1 -' This begins with the final measurement period because it is longest and containing as it does, a transition from warm to cold summer conditions, it seemed to be an interesting way to begin. In retrospect it may be better to delete Figure 2 and refer to Table 1 alone, where all the days are listed.

11. 'Section 4.3 -' I agree on the need to relocate the Hannah and Gurnell calculations, as well as to tabulate the results.

12. 'Finally in ln 5...' and 'Finally in the conclusion...' Agreed, the idea of basin shape is really a discussion item and the role of weathering crust development is not well articulated. I have

always wondered why weathering crust development doesn't produce more supraglacial runoff delay than it apparently does according to runoff box experiments. Perhaps it is because we haven't been describing our runoff areas correctly.

13. 'While I'm...' Points raised here are addressed in my general reply.

Replies to Referee #2

1. 'P1570: The abstract...' Points raised here are addressed in my general reply and response 2 to Referee #1, but I agree that it must be rewritten.

2. 'Introduction: referencing...' Yes, this needs to be expanded, with particular focus on supraglacial drainage and some attention to subglacial water pressure. In addition to Haut Glacier d'Arolla, work by de Hooke and Kohler on Störglaciaren may be relevant, as well as some of Clarke's work on the Trapridge. An email response from a reader of this discussion paper who appears to be quite interested in the results has also referred me to Larson's work on the Burroughs Glacier.

3. 'In general, a better...' As noted in response 2 to Referee #1, I did overextend the implications of my results in the closing sentence of the abstract, thus inviting remarks from both referees for the need to comment more extensively on the subglacial drainage system. As stated above, I will withdraw the implication and focus the literature review accordingly.

4. 'P1571, line 15-20...' This is shown in Section 2.1 and Figure 1, but I accept the point about stating the scale in the Introduction.

5. 'P1571, line 25+...' This is addressed in response 4 to Referee #1 comments. As stated above, an exact LiDAR match is likely not achievable though I will check with Chris Hopkinson to see what he thinks.

6. 'P1572, line 10...' Perhaps placing the measurement periods in the context of the base camp weather record, as stated in response 5 to Referee #1, is a solution here, as well as better reference to Table 1 and an expanded version of Figure 3. It is already clear to me that if this paper is to go ahead, substantial reorganization is required and that this is among the points to consider in revision.

7. 'P1572, lines 13-21...' This is addressed in response 6 to Referee #1, where it is acknowledged that the procedure is problematic. The key point for me is the timing of peak stage, which is the most robust part of the procedure.

8. 'P1573, lines 13-15...' This is among the long term working practices that we sometimes lose touch with regarding an original source, as is the case for me, so I will have to refresh.. A recent reference to half-hourly averaging appears in Thomas Foken's textbook on Micrometeorology, in relation to the eddy correlation procedure. Nevertheless, it is probably advisable to eliminate the second phrase of this sentence.

9. 'P1574, line 4...' Yes, should be Oerlemans.
10. 'P1574, line 25 – P1574, line 6...' I hope that this conveys my concerns as well, especially if the findings are taken to be more than exploratory in nature. In retrospect, it appears that I have conveyed more than an exploratory significance to the work, so that must be removed in revision.
11. 'P1575, line 17...' The optimization is applied for the duration of each melt measurement period, thus allowing the possibility of some day to day variation in base flow if more than one day is involved. In the five-day sequence of Figure 3, only the first three days are used for optimization because the SR50 responds to snow thickness after that. The result is base flow near zero for the snow cover days, which seems to be a realistic outcome. The other measurement periods do not show such low temperatures as seen in Figure 3. For example, air temperature is no lower than 5 °C during the August 13-16 period.
12. 'P1576, lines 5-9...' I agree, certainly with respect to the final two days. They do not play a much of a role other than to show the effect of a late summer snow cover, so I may consider removing them.
13. 'P1576, line 25...' The importance of katabatic wind is taken to be a given here, rather than a conclusion, so the statement is meant to convey that the observations are consistent with katabatic wind theory.
14. 'P1577, lines 5-11...' Yes, it is probably best to restrict this to 'base flow near zero for the snow cover days' in the sense stated in response 11 above and leave it at that.
15. 'P1577, line 21...' As stated in response 7 above, the timing of peak discharge is the most robust part of this procedure. In response 3 to Referee #1 and my general reply I refer to precisely defined runoff area data of my own, which are half-hourly, showing shorter time to peak discharge. Only two of them cover more than a 24 hour period, but a comparison with what is presented here and a better outline of procedure may suffice to make the case.
16. 'P1578, lines 3-13...' Yes, the role of the weathering crust needs to be properly articulated along the lines stated in response 12 to Referee #1.
17. 'P. 1579, line 21-22...' Withdrawn; besides, it also diverts attention from my point about microbasin shape.
18. 'P. 1579, lines 23-25...' The paper will be refocused on the theme of short term supraglacial runoff, possibly noting implications for diurnal subglacial water pressure variations, thus removing reference to longer term response.
19. 'P. 1580, lines 1-9...' The elongate nature of supraglacial drainages, at least on Peyto Glacier, was made clear to me by following stream channels up-glacier from moulin openings, as were intermittent cross-over to other elongate drainages. The main point of presenting this paper is to persuade others to investigate further, using a better approach than I describe here.

20. 'Table 2...' These were all 24 hour periods, as stated in Table 1, and should be so stated here.

21. 'Figure 1...' Yes, this figure must be reworked along the lines stated in response 7 to Referee #1.

22. 'Figs. 2 & 3...' One aspect of the revision may be the deletion of Figure 2, as considered in response 10 to Referee #1. I'll consult with the editor on the use of colour.

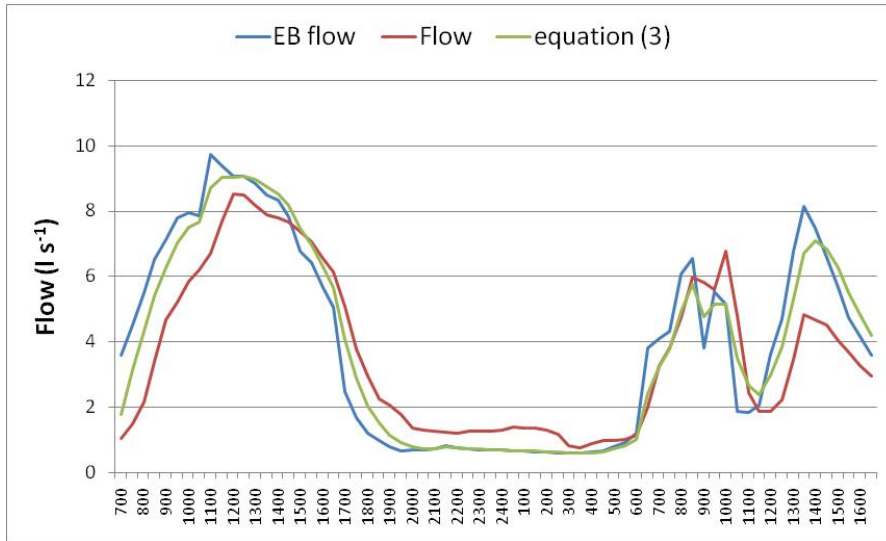


Figure 1. Half-hourly results from 1971 field work, showing Flow measurements, an approximate work up of energy balance flow estimates (EB flow) and the result of applying a one hour flow delay to EB flow, using equation (3) from the paper.

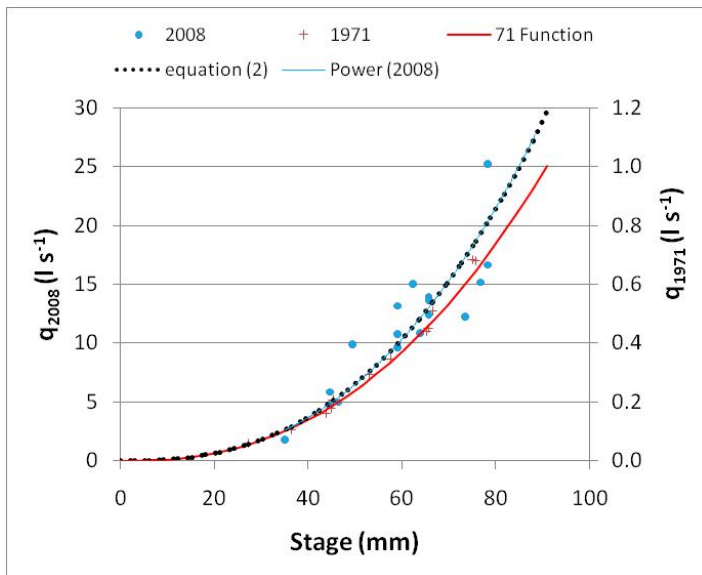


Figure 2. Stage-discharge curves for 1971, using a V-notch weir and 2008, for an open channel.