

Interactive comment on “An investigation of the influence of supraglacial debris on glacier-hydrology” by C. L. Fyffe et al.

Anonymous Referee #2

Received and published: 2 March 2016

General comments:

This paper uses established methods to make inferences about the glacier drainage system under debris-covered ice. Though similar studies have been done in many places and for a long time, I found the choice to investigate a debris-covered glacier worthwhile and the results (though perhaps difficult to precisely interpret due to limited data) interesting. I think the paper merits publication subject to revision.

My general criticisms are detailed below but can be summarized as follows:

A great deal is made of the result that a less efficient drainage system (some combination of supraglacial/englacial/subglacial) is tapped downstream of a more efficient drainage system, and that there is a distinction between the drainage system accessed

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by moulins in clean ice versus portals (of some nature) in debris covered ice. The interpretation assumes that the structure and evolution of the drainage system being probed are those of the englacial/subglacial system. It is unclear to me, however, that whatever happens supraglacially (under the cover of debris) is not playing a role here. The authors state that it is difficult to identify moulins in the debris covered ice, so I wonder how much of the measured dye delay could be occurring supraglacially in the debris covered area. There is probably no practical way to address this except with a few more explanatory words.

A related issue is the simple assumption that the subglacial drainage system must consist of some main arterial axis. This is often true, but I am surprised that no mention is made of the possible modification of subglacial hydraulic structure by the distinct morphology created by the supraglacial debris (both the moraines and the hummocky terrain). On the scale of moraines and other larger features, modification to the ordinary subglacial hydraulic catchment structure can occur (see references to Fischer et al and Shugar et al later). This modification could lead to asymmetric inhibition of drainage across the glacier, or more than one main drainage axis, or a separation of flow caused by the moraines. I suspect the response here will be that the bed DEM is too poor to do any such hydraulic catchment calculations, but some discussion of this possibility and citation of references seem warranted.

Much is made of the inferred temporal evolution of the drainage system and the fact that it does not appear to be 'progressive' or monotonic. It seems to me that the sampling schedule is perhaps aliasing the higher frequency variations in the drainage system, a possibility that is eventually acknowledged. I'd advise a bit more reservation in interpreting the results as contradictory to the standard conceptual model of long-term progressive drainage system development. I doubt any glacier system evolves steadily and monotonically without short-term variations in response to weather. This discussion could be made shorter and the possibilities clarified by annotating Figure 2 with the sampling schedule.

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A bit more detail is warranted in several places in the methodology (see below), and though clear in most places, the text could use a bit of polish. This is a worthwhile study and I hope these comments serve to improve the final paper.

Specific comments (page.line):

5374.8: Consider rephrasing “mid-part of the glacier”. Is it in the central ablation area?

5375.28 “daily amplitude and magnitude” Should “magnitude” be “volume” instead? Not sure what magnitude is precisely and how it differs from amplitude.

5377.16 Is it possible that the stage-discharge relationship was altered during the high flow events that damaged the well? Please provide some detail to reassure the reader that a single rating curve is applicable across two melt seasons. Deposition/excavation of sediment below the water line in such events is known to alter stage-discharge relationships. Was the dilution gauging done in 2010? Over what time period, e.g. a single daily cycle?

Section 3.1.2: - It would help to have some description of the size and nature of these supraglacial streams (e.g. width-depth aspect ratios, tortuosity), not least in order to imagine how the velocity-area measurements were carried out. Please provide additional detail on these measurements. - Does ‘measured by floats’ mean suspending instruments in the stream with a float, or throwing something in the stream that floats?

5379.22 Please define “dye lot”

5379. Section 3.5. Rather than compressing the dye tracing methods into Table 2, I think it would be worth providing the equations in the text used to compute those parameters in Tables 4 and 5 that are not explained elsewhere, i.e. D , Ac , Pr . To be specific, it would be great if the reader didn’t have to look up Seaberg for D and if the Kilpatrick and Cobb equation were written in the text. How was the integral of the dye return curve computed (discretization, etc.)?

5380.10-11 By the time the dye would have reached the moulin, would there have been

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significant dispersion, i.e. is there a significant time lag between the leading and trailing edges of the dye plume?

5381.5-6 I'm afraid Figures 4 and 5 do not help me see what is said here (large catchments bounded by moraine crests). Figure 4 appears almost all purple in my pdf which doesn't help much. Figure 5 is so crowded with lines I cannot make out the catchments of S14 and S15. Some annotation to the figures will be necessary if you want the readers to be able to verify these statements.

5381. Section 4.3. Figure 6 is 2010 and Figure 7 2010 and 2011? Would be helpful to note years in captions or axis labels somehow.

5381. Section 4.3. Tables 4 and 5. Please provide a brief explanation somewhere of what variable causes estimated Pr to exceed 100%.

5381. Section 4.3.1. S3 (Fig 7a) seems a notable exception to the general pattern described here and is probably worth mentioning, along with S6 and S8 (already mentioned).

Figure 7. A more intuitive format for dates, e.g., "09 Aug 2010", would make the interpretation speedier.

Figure 8. It would be easy and useful to differentiate 2010 from 2011 data (by symbol or color).

5383.11-13. Evidence of the statements about the upper glacier behaviour can be found by picking through the table, but it would be nice to have one figure where all the breakthrough curves being discussed were plotted together. Here the text is referencing S10, S12, S13, S14 in June, but only two curves of this group appear in Figure 6a.

5386.6-7 Just curious: how were these streams observed beneath or through snow pack? Sounds of flowing water? Depression in snow surface?

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5386.8-12 These are very qualitative statements and do not seem like they make a strong argument. Perhaps pare this down to one brief statement that includes the reference to Mair and omit the part about favourable spring weather, etc.

5386.19-20. "However, compared to June, the late July return curves S12_300711 and S14_290711 were slower and more dispersed, although they still had singular peaks" How can one be sure this result is not just a consequence of the particular time of sampling? Can the sampling times be indicated in Figure 2? By eyeball, air temperatures could have been relatively low during these two days.

5386.26 In general, progressive development is expected but the system is variable. In looking at air temperature and discharge in Figure 2, sporadic sampling could be aliasing the higher frequency signals that are actually in the record. Perhaps more caution is warranted in making these statements, and a careful look at the sample timing is needed. The explanation given in the first half of page 5387 seems to capture this idea, but then calls into question the earlier statements about the unexpected nature of the dye tracing results. I'm not sure they're so unexpected when sampling in sporadic and the drainage system evolution is not monotonic.

5389.16-17 "A distributed and channelized system probably occurs simultaneously under Miage Glacier, with the distributed system draining to the main channel system." Perhaps, but there does not seem to be direct evidence either way. A calculation of subglacial hydraulic potential would reveal if it might be possible to maintain side-by-side drainage axes for some appreciable distance downglacier, rather than assuming all drainage converges on the central axis. Strong perturbations to glacier surface topography due to debris cover can have implications for subglacial hydraulic structure:

Fischer, Urs H., et al. "Changes in geometry and subglacial drainage derived from digital elevation models: Unteraargletscher, Switzerland, 1927–97." *Annals of Glaciology* 40.1 (2005): 20-24.

Shugar, Dan H., et al. "The response of Black Rapids Glacier, Alaska, to the Denali

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earthquake rock avalanches." Journal of Geophysical Research: Earth Surface 117.F1 (2012).

5390. Point #5. Consider whether modification to the subglacial drainage system architecture (see comments above) should be added as an unexamined but potential contributor.

5391.10-15 Though 'opposite' to what one might normally expect, can we establish that this is an important result? If ablation is suppressed where ice is debris-covered, perhaps the runoff from this region is not actually important compared to the runoff generated where the ice is clean (above the debris cover in this case). Side note: Other studies have found that the patchiness of typical debris cover means there is both enhanced and diminished meltwater production across debris-covered zones that tend to average out. I'm not sure of this is the case on Miage Glacier.

5392.3 Based on Nienow et al (1998) following Spring and Hutter (1982), A_G should probably be $5.8 \times 10^{-7} \text{ Pa s}^{-1/3}$, rather than $5.8 \times 10^{-7} \text{ Pa s}^{-1/2}$. This looks like the traditional "B" instead of "A".

5392.5-10 Why not compute the subglacial hydraulic potential and determine a plausible channel routing, rather than assuming a straight-line? It would at least give some indication of how tortuous the channel might be.

5392.16-17 It seems as though ice thickness, and the uncertain hydraulic pathway, would impart much greater uncertainties in the closure calculations than ice density. Why was the uncertainty in density explored instead of thickness along a plausible/hypothetical flowpath?

Technical corrections/queries (page.line):

Check subject verb agreement: 5374.9 encourages => encourage (subject = melt rates and runoff concentration) 5374.16 inhibits => inhibit (subject = inputs)

Consider use of "e.g." when citing papers that are examples, rather than original or

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sole sources, of the information given. A good example is in the introduction: “Understanding the nature and evolution of the glacial drainage system is important because it controls how meltwater inputs impact glacial dynamics (Mair et al., 2002)...”

5381.9 ; => ,

5383.1 singular => single (also on 5386.20)

5389.7 exits => exists?

5392.1 Seems odd to cite Oke for the value of g. Suggest leaving the reference off.

5392.12 “this” = ? Closure time?

5404. Figure2. Please add to caption “proglacial” discharge, and specify where precipitation was measured (which station).

Interactive comment on The Cryosphere Discuss., 9, 5373, 2015.

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