Author comment on points by referee 2

Dear Dr Hilmar Gudmundsson,

We would like to thank the second referee for their very helpful and thoughtful comments. As in the reply to Reviewer 1 our comments in response are detailed below alongside the referee's comments. Where changes have been made they have been marked in tracked changes in the included Word document and where line, figure and table numbers are mentioned these refer to the edited Word document. Our responses to the reviewer's comments are in blue italic text with purple italic text highlighting changes made to the manuscript. We have added numbers to each of the points made by the reviewer to allow us to refer to these later in the document.

Best Regards,

Catriona Fyffe

(on behalf of all co-authors).

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:

1 Supraglacial influence

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 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.

We agree with the reviewer that more information is required about the supraglacial streams that were injected. This point is very similar to point 5 made by reviewer 1, so please see that section for our response.

2 Subglacial drainage system configuration

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 morianes 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.

This is a useful point made by the reviewer and something we had not fully considered. The reviewer notes that we assume that the subglacial drainage system must consist of a single arterial axis. Admittedly this was assumed in the conduit closure calculations, but this was because it allowed the modelling of the 'worst case scenario', i.e. gave the largest likely initial conduit cross-sectional area to close. This should not be taken as an indication that we assume this to be the most likely subglacial drainage system structure. It is known that there was only one main proglacial stream though, so the conduits must confluence to form a single channel eventually. Within the rest of the paper the channelised system is termed a 'channel system', 'channelised system' or 'conduit system' so as to avoid assuming whether there are one or several drainage axes. We did calculate the subglacial hydraulic potential from the surface and bed DEMs, as well as the location of subglacial conduits. However, as the reviewer alluded to, the poor quality of the bed topography meant that we were reluctant to publish the results. The map of subglacial conduits did reveal that there was likely 2 (and perhaps 3) drainage routes beneath the main tongue (divided by the main moraines), but the clarity of the subglacial drainage routes diminished around the bend of the glacier. That would tie in with the reviewer's idea that the moraines could modify the drainage system.

The appendix has been edited (see lines 568-570) to clarify that the assumption of a single subglacial conduit was for calculation purposes and should not be taken to imply that this is a more likely drainage system structure. There has been a short paragraph added in section 5.4, mentioning the possible effect of moraines to increase the complexity of drainage (see lines 475-478).

3 Temporal evolution of drainage system

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 longterm 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.

This is a fair point by the reviewer and the authors agree that perhaps the idea of the progressive evolution of the drainage system was put across too strongly, when it is known that variations in inputs due to the weather can influence the development of the drainage system structure.

Changes have been made to lines 396 and lines 400-402. The decision was made not to shorten the discussion because the authors thought it pertinent to fully explain the situation, since although it may happen often in reality, it is rarely measured. The days when dye injections were conducted have been highlighted with the use of grey bars on Figure 2. The caption has been altered accordingly.

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?
Both occurrences of 'mid part of the glacier' have been changed to '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.
By magnitude the authors meant the quantity or amount of meltwater discharge. Although 'volume' could be used, this isn't quite correct because we are talking about discharge (volume per unit time), and we are trying to portray how big that value is. The amplitude on the other hand is the difference between the maximum and minimum flows in a day (as defined by Swift et al. (2005, p141)).
Swift, D.A., Nienow, P.W., Hoey, T.B., Mair, D.W.F., 2005. Seasonal evolution of runoff from Haut Glacier d'Arolla, Switzerland and implications for glacial geomorphic processes. J. Hydrol. 309, 133–148.
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?

Dilution gauging was conducted during all the field visits (up to twice daily when in the field), in both 2010 and 2011. The authors are aware of the possibility of rating shifts and are thankful that the referee brought to our attention that the brief coverage of the hydrometry in the methods could lead to confusion. The single rating was applied to the whole record for two reasons. Firstly because despite frequent gauging the flows gauged within each field trip often did not cover a large range (due to the subdued proglacial hydrograph) so the application of separate rating curves for different periods was not possible. Secondly when all gaugings were corrected to the same datum they did match, giving us confidence that the application of a single rating was reasonable.

The text has been clarified, see lines 123-127.

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?

The stream areas were determined by measuring the stream width using a tape and measuring the depth at on average 9 points across the channel using an ice axe and tape. Velocity was measured by timing the passage of a float (lemon peel) a set distance, multiple times (8 on average). The width times by the average depth gives the cross-sectional area, which multiplied by the average velocity gives the discharge.

This has been clarified in the text, see lines 132-134.

5379.22 Please define "dye lot"

The dye lot is the batch of dye used. There can be differences in the fluorescence of different batches so it is good practice to calibrate the fluorimeter every time a new batch of dye is used. The term 'dye lot' is used in the USGS methods papers.

The term 'dye lot' has been changed to 'batch of dye', line 185.

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.)?

This is a good point made by the reviewer that perhaps some of the information given in Table 2 would be better in the text.

The equations for the calculation of the dispersion coefficient (D) and volume of dye recovered (V_r), along with the appropriate related parameters have now been included in the text (see lines 203-220). This also includes a description of how Ac was calculated. There has also been some alterations as appropriate to Table 2.

5380.10-11 By the time the dye would have reached the moulin, would there have been significant dispersion, i.e. is there a significant time lag between the leading and trailing edges of the dye plume?

At S5, we obviously won't know this for certain but the salt dilution gauging of the supraglacial stream can be used as a guide. The dilution gauging was carried out over 66 m of stream each time and the time from the peak to the end of the trace was 7 *minutes on average. If the dye dispersed at the same rate over the whole 446 m to the moulin then the time from the peak to background would be 46 minutes. Given that the time from peak to background for the fastest return curve for S5 was just over 6 hours, the dispersion caused by the supraglacial stream would be a relatively small influence on the dispersion of the return trace.*

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.

Figure 4 has been altered to increase clarity by changing the top panel so it is now just an inset showing the location of the main panels, and by altering the symbology.

An inset map has been added to Figure 5 so that the area of the cluster of moulins at S12-S15 can be seen more clearly. Note that although the catchment directly upstream of S14 is modelled as small, observations on the glacier revealed that its catchment included the larger catchments to the west of the central moraine (the central moraine can be identified as an area of smaller catchments over a debris-covered area). The caption has been altered accordingly.

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.

Captions on Figure 6 and 7 have been clarified to include the years of the injections.

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

The area under the curve (A_c), used to calculate P_r can be affected if the background fluorescence changes during the trace, and this is not accounted for fully (the background is removed but over longer traces in particular this could change during the trace). Errors in the average proglacial discharge (Q_p) or problems with the fluorimeter could also cause an issue with the percentage dye return.

This is clarified in the caption for Table 3 (old Table 4), since the issue for Table 4 (old Table 5) is for S5_120911, and this is likely due to the spikes on the tale of the trace.

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).

The mean trace velocity for S3 is still fairly low (0.27 ms^{-1}) so can be included within the slower trace velocities. Return curves did tend to have a single peak, but then so did some of the other lower glacier traces, and hence the use of 'several displayed multiple peaks' rather than 'all'. Since S3 is not clearly exceptional we have not mentioned it explicitly.

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

All dates in the text have been converted to the full 9 August 2010 convention and trace names have been converted to S3_14Jun10 format. Figures 2, 6, 7 and 10 and Tables 3 and 4 have been altered so that the trace names are dates and dates on the x-axis are given using the same approach.

Figure 8. It would be easy and useful to differentiate 2010 from 2011 data (by symbol or color). *Some of the points are composed of data from 2010 and 2011, as well as some*

from only 2010 and 2011, but it is not thought necessary to include this to allow interpretation.

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.

A figure showing all the upper glacier traces in June has been added (Figure 8), the numbering of the following figures have been updated to reflect this, as have the references in the text.

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

Sometimes the streams were visible and had cut into the snow surface, sometimes their position was marked by saturated snow and at other times they were marked by a depression in the snow surface. The photograph below should give an idea (from June 2010):

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.

The authors are making two points, 1) that there are specific factors about the supraglacial topography and altitude of Miage Glacier which could result in inputs which could channelize the system early and 2) that channelization can occur on clean glaciers given a snowpack that remains relatively long into the season. Although we cannot be sure of the relative importance of the above, we do think it is important to mention the

points in 1) since they have not been highlighted before and could be as important as point 2).
This section was re-written to make the arguments clearer and remove the mention of spring weather conditions, see lines 368-378.

5386.19-20. “However, compared to June, the late July return curves S12_300711 and S14_290711were 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.
The reviewer makes a valid point here, and as mentioned for reviewer 1 (see point 4), we have added the times of the injections into Tables 3 and 4. The injection time range for the streams which were traced on multiple occasions has been given in Section 3.5. The dates of sampling have been indicated in Figure 2, although to avoid clutter we have not tried to illustrate the date of every trace – this detail is given in Tables 3 and 4.

The upglacier moulins (S12, S14 and S15) were all traced in the afternoon, with traces conducted within 2-3 hours of each other. We also considered the supraglacial input discharges and the proglacial discharges when interpreting the data (to gauge the influence of the input discharges on the day of injection). Also, the main interpretation about the seasonal evolution of the upglacier moulins was based on patterns seen at all three moulins traced on different days (lines 309-310).

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.
This is a fair point made by the reviewer that in reality the evolution of the drainage system may not be entirely progressive and that there may be higher frequency variations due to weather fluctuations. As mentioned in the point above, data about the timing of the injections has been included now. Please also see the comments made to point 3 in this review. Changes have been made to lines 399 and lines 400-402.

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-byside 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 earthquake rock avalanches." Journal of Geophysical Research: Earth Surface 117.F1 (2012).
Please see the author comments to the main point 2 above. A channelised drainage system is defined by being composed of discrete conduits, saying there is a channelised drainage system does not mean there must be one drainage axis, although the flows must converge before they emanate from the proglacial stream, note the use of 'main channel system' rather than 'main conduit' in the phrase above.

5390. Point #5. Consider whether modification to the subglacial drainage system architecture (see comments above) should be added as an unexamined but potential contributor.
Although this is a fair point, we decided on balance not to mention the modification of the subglacial drainage structure by the surface topography here since it is not a conclusion of the present study. We have already included a mention of this point in the discussion (lines 475-478).
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.
If the reviewer means 'important' in terms of being instrumental in the evolution of the drainage system (i.e. the main focus of this study), then yes the runoff from the debris-covered area appears less important than that from the clean ice. However, the runoff from the debris-covered area cannot be neglected since the fact that the drainage system is less efficient could impact on the velocity of the glacier, its water chemistry and the proglacial runoff signal. In previous modelling work we calculated that the melt from the debris-covered area is around 30% of the total melt (Fyffe et al., 2014), which is still a very significant contribution to downstream water resources. On Miage Glacier the debris cover on the majority of the lower glacier decreases melt rates because the debris is fairly thick (0.25 m on average).
5392.3 Based on Nienow et al (1998) following Spring and Hutter (1982), A_G should probably be 5.8 x 10ˆ{7} Pa sˆ{-1/3}, rather than 5.8 x 10ˆ{-7} Pa sˆ{-1/2}. This looks like the traditional "B" instead of "A".
We thank the reviewer for spotting this mistake; A_G *should indeed be* 5.8×10^7 *Pa* $s^{-1/3}$*.*
This correct value was used in the calculations of conduit closure.
This has been corrected in the manuscript, see line 561.
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.
Please see the author comments to the main point 2 above. In addition, the calculated subglacial drainage pathways were quite straight on the upper tongue where they were

clear (due to the constraining influence of the lateral and medial moraines). It is therefore fairly reasonable to assume a straight line. We have decided not to include this in the text due to the doubt over the accuracy of the subglacial topography.

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?

This was solely because of the way the calculations were performed. We calculated the theoretical ice thickness that would be required to result in closure of the largest modelled conduit after 4 months (144 to 160 m). It was realised that there was a large difference between the ice thickness required and the measured ice thickness near the upper moulins (375 to 380 m). Therefore the ice would need to be significantly thinner than measured for the conduits to remain open over the winter.

4 Technical corrections/queries (page.line):

Check subject verb agreement:

5374.9 encourages => encourage (subject = melt rates and runoff concentration)

Changed in manuscript, line 20.

5374.16 inhibits => inhibit (subject = inputs)

Changed in manuscript, line 27.

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

Added ‘e.g.’ as requested, line 51. The rest of the paper was checked but there were no other occurrences where an ‘e.g.’ was necessary.

5381.9 ; => ,

Changed in manuscript, line 248.

5383.1 singular => single (also on 5386.20)

Both occurrences changed, lines 290 and 395.

5389.7 exits => exists?

Changed in manuscript, line 487.

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

Changed in manuscript, line 560.

5392.12 “this” = ? Closure time?

Changed to ‘the time taken for the conduit to close’, line 572.

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

Figure caption changed and the station used for the rainfall and temperature data has been given in the legend for the figure (LWS).