Author comment on points by Referee 1

Dear Dr Hilmar Gudmundsson,

We would like to thank the referee for their constructive comments. 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 or 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 #1

Received and published: 6 November 2015

General:

Debris cover may have a twofold effect on the hydrology of a glacier: 1) through alteration of radiative and thermal surface properties, it influences the surface energy balance; for considerable debris thickness this leads to a reduction in meltrates which in turn may 2) affect the nature and evolution of the internal drainage network. The first point has been addressed by the authors elsewhere, and the present manuscript (MS) deals with the second effect.

The characteristics of meltwater drainage and their evolution over the ablation sea- son are investigated using dye tracer tests, both from the debris-covered, lower part of the Miage glacier as well as from the debris-free zone further upglacier. The tracer tests reveal a reduction of transit velocities downglacier, opposite to what has been found elsewhere. Whereas transit velocities from the upper, debris-free part, increase over the ablation season, transit from the lower, debris-covered area does not exhibit a similar evolution. These observations are interpreted in terms of channelized versus distributed drainage system configurations, such that the fast transit from the upper glacier is taken as a signature of a channelized system whereas the lower part would be drained through a distributed system. The authors conclude further that the low meltrates in the debris zone inhibit the morphological switch to a channelized configu- ration.

Criticism:

The MS has two major weaknesses: the first one related to the unclear motivation for the study and the second one concerning the interpretation of tracer tests in terms of drainage system configuration and evolution. To become publishable, major revisions are required to remove these shortcomings

1. Motivation for study

1) The study needs to be better motivated

by a) clearly formulating the questions and hypotheses to be investigated and b) outlining the potential significance of the hydrology of a (partly) debris-covered glacier. Re a) In the introduction two aims are listed of which only the second one actually is addressed.

We thank the reviewer for their points about the aims and hypotheses. In respect to the first aim this has been altered so that it is now broader and incorporates the influence of the debris on the supraglacial topography as well as the supraglacial flow. We have addressed this aim in the paper by analysing the supraglacial topography, calculating the supraglacial catchments and performing spot measurements of supraglacial discharge and flow. We combined these measurements with previous knowledge of the melt rates across the glacier to gauge the influence of the debris on the supraglacial input hydrograph (see especially sections 4.1 and 5.1).

The first aim has been edited see lines 65 to 68.

We agree it would be helpful to clearly state the guiding hypotheses for this study and have added the following text at lines 74-87:

'Our investigation is guided by two overarching hypotheses: First, that continuous debris cover over the lower glacier inhibits the development of an efficient channelized drainage system, both through suppression of surface melt beneath thick debris and the formation of hummocky surface topography which impedes drainage capture and leads to low magnitude and dispersed meltwater inputs to the glacier; and second, that above the upper limit of continuous debris cover, the development of an efficient channelized drainage system is promoted both by the enhancement of melt beneath thin and patchy debris and the formation of ridge-valley topography which enhances surface drainage capture and concentrates rapid surface melt into high magnitude moulin inputs. These hypotheses are investigated by the measurement of supraglacial flow and 48 dye injections into 16 surface streams over 2 ablation seasons. These measurements are interpreted to explain the drainage system configuration emanating from the continuously and partly debris-covered parts of the glacier, their interaction and evolution over the course of the ablation season.'

2. Need for study

Re b) I did not really understand the need for this study; which aspects of glacier drainage do the authors expect to be different for a debris-covered glacier in contrast to a "clean" glacier? And why would this be important to know? Such motivation is not clearly stated in the MS but I suspect that special hydrological behav- ior may have implications for the shape of the discharge hydrograph and possibly for the dynamics through its influence on basal water pressure. I would expect that most debris-covered glaciers display a combined situation such as the one presented here, where debris cover in lower part inhibits channelization (following the interpretation in the MS on which

I have some doubts, see below), but connects to an existing system from upglacier. Wouldn't you expect that for such a configuration, the effect on glacier dynamics was minimal due to the governing role of the existting channel system from the upglacier area.

The authors are grateful that the referee has mentioned that the need for the study needs to be more explicit. The authors are aware of the effects of the hydrological system as mentioned in the manuscript and from related work. In the manuscript the sentence at lines 49-52 'Understanding the nature and evolution of the glacial drainage system is important because it controls how meltwater impacts glacier dynamics (Mair et al., 2002), with the glacial dynamic response affecting erosion rates (Hallet et al., 1996)' specifically states that the hydrological system does impact on the glacier dynamics, and that this would be one reason why this study would be important. We also mention in the conclusions lines 547-548 that 'the debris is likely to influence melt water travel times and therefore the proglacial runoff signal'. Related work has shown that the proglacial runoff signal is unusual and this is likely due to the debris' influence on both melt rates and the hydrological system (see Fyffe, 2012). The impact of the debris on glacier dynamics is also not minimal, for example, since there is a reduction in the amplitude and magnitude of inputs, the greatest variations in velocity are higher upglacier near the upglacier moulins, rather than lower on the glacier where melt inputs are (usually) greatest (again see Fyffe, 2012). Both of these points are important but should be the subject of separate papers. As the reviewer points out, we can speculate about the ways in which extensive debris cover might affect the configuration and evolution of a glacial drainage system, but to our knowledge, ours is the first study to test such speculation through field measurement. This lack of understanding is a key motivation for the study and is stated at lines 60-63. However, to strengthen the rationale the following has been added into the Introduction:

After (Hallet et al., 1996) on lines 52-55. 'Any overall impact on glacier dynamics will also have an influence on the glacier's mass balance. If the debris influences the glacier hydrological system then this will impact on the timing of the transfer of water through the system, influencing the proglacial runoff signal.'

3. Interpretation of tests

2) More careful interpretation of tracer tests is required. In the present MS, low transit velocities are interpreted to result from an inefficient, dis- tributed drainage system. However, previous tracer tests of single drainage pathways yielded low transit velocities at low discharge rates, although there was little doubt on the channelized nature of the pathways (Gulley et al. 2012, Werder et al., 2010, Schuler et al., 2004). The low meltrates of debris-covered ice imply low discharge rates and it is therefore expected that tracer transit from that area would have low velocity, re- gardless of drainage system configuration. The simplistic interpretation "low velocity = distributed drainage" is hence compromised.

We completely understand the reviewer's points here, especially regarding the fact that supraglacial velocities on the debris-covered glacier are likely to be lower (they are, as stated in section 4.2). However, there are a few details which help to back up our interpretation that the slower tracer tests are not just due to lower input velocities.

Firstly, consider one of the earlier traces into the lower glacier, when traces were most suggestive of a distributed system. If we look at both of the June 2011 traces into S5, supraglacial velocities were 0.24 - 0.25 ms^{-1} whereas tracer transit velocities were only 0.07 ms^{-1} (see Table 4), it is clear that the tracer transit velocities are substantially lower than supraglacial velocities. Now this could be simply due to an increased subglacial sinuosity in a channelized system, however to decrease the velocity from 0.24 to 0.07 m s^{-1} would require the channel length to be 3.4 times the straight line distance, a sinuosity indicative of very tortuous meanders which would seem unlikely. Furthermore, since the water must flow in the main subglacial channel for at least part of the way so that it reaches the northern lobe proglacial stream, the tracer transit velocity between the supraglacial stream and the main channel must be less than the average of 0.07 m s^{-1}. Hence, the very low transit velocity cannot be explained by the low discharge rate. This suggests that the englacial/subglacial system from S5 and S7 in June is less efficient than the supraglacial system.

Also, it must be stated that we did not use the trace transit velocity alone to interpret the traces. Yes, we do mention in section 4.3.1 that the velocities from the traces into the lower glacier were lower, but we also state that the traces were broader and had multiple peaks, with more detail provided on the change of the shape of the traces given under section 4.3.2 Lower glacier. In the interpretation in section 5.4 lines 438-440 we stated 'Traces into S1, S3, S5 and S7 had a slower u and in some cases (especially S5 and S7, Fig. 7b and c) displayed multiple peaks, indicating the water spent at least some time within a less efficient hydrological network, with multiple flow paths characteristic of a distributed system', Note that it is the existence of multiple flow paths (shown by the multi-peaked traces) and not the trace velocities alone which makes it more likely that we traced a distributed system. We were careful otherwise to say that the systems were more or less efficient rather than state the type of system. We use the term distributed in its broadest sense i.e. that which includes all less efficient drainage types (linked cavity, braided canal, porewater flow) and have specifically not identified which is more likely given that this is not possible to judge from dye tracing. Unfortunately, the traces themselves were not shown in previous studies by Nienow et al. (1996), Schuler and Fischer (2009) or Werder et al. (2010), but in Gulley at al. (2012) all of their traces showed one peak, even if this was broader and slower earlier in the season when supraglacial velocities were lower. Schuler et al. (2004)'s traces did sometimes show small secondary peaks, but the main peak was still very prominent and the traces took no more than around 4 hours to pass through. The only previous traces with multiple peaks found were of those into boreholes (Hooke and Pohjola, 1994), and although their traces came through during days to weeks, we could only study the first 24 hours after each injection because we were tracing multiple streams over short fieldtrips. Even then, the June lower glacier traces were extremely broad (e.g. for S5_060611 it was over 7 hours from injection until the maximum of the highest peak, with the dye concentrations not returning to below background until over 14 hours after injection). We therefore think it is reasonable to describe the drainage as 'distributed' since the hydrological network is slow, inefficient and has multiple flow paths.

Section 5.4 has been edited to clarify these points.

4. Velocity-discharge hysteresis

Tracer tests repeated in quick succession over the course of a day (Werder et al 2010, Schuler et al 2004, Nienow et al 1996) yielded a wide range of transit velocities (0.1 – 1 m/s) depending on the timing of tracer injection relative to the diurnal discharge cycle. This range is comparable to the range of velocity variations over the entire season, as reported in this MS (0.06-0.8 m/s). To possibly detect a seasonal evolution, either the timing of tracer injection relative to the discharge cycle must be kept constant (e.g. always at the time of max Q) or, optimally, the evolution of the diurnal ranges need to be measured repeatedly over the season. Hence, valid conclusions on the seasonal evolution of the drainage system cannot be drawn from the material presented here, without further information on timing of tracer tests; such information however is not given.

We thank the referee for this point, that the range of the velocity changes could be explained by velocity-discharge hysteresis. However, we do not think this impacts on our findings of seasonal evolution for the reasons given below.

Firstly, however we must apologise for not including the times of the trace injections, these have been added 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 (lines 178-182).

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. 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). It would be unlikely therefore that the patterns seen were entirely due to inflow modulation within a 24 hour period. For our interpretation of the differences in September over the two years, this was based on comparing the traces into two different moulins which were again traced at a similar time of day to previous traces.

The articles used as examples above by the reviewer (namely Werder et al 2010, Schuler et al 2004, Nienow et al 1996) are based on multiple traces, conducted at short time intervals where channels are thought to be efficient conduits (even if the overall efficiency varies due to the hydraulic conditions at the time). They were also all conducted on clean glaciers where the diurnal change in melt input is likely to be large (even when input was from a lake outlet this still varied diurnally (Werder et al., 2010)). On the lower glacier on Miage Glacier, the diurnal variation in melt input is likely to be small (we know from modelling work that the diurnal signal is more subdued (Fyffe et al., 2014)), we also measured the supraglacial stream input discharges (in 2011 only, see Table 4) and they were found to be quite consistent - for instance the S5 supraglacial discharges were 0.027, 0.032, 0.031 and 0.028 $m^3 s^{-1}$, although the S7 discharges were slightly more variable. Considering that the dye is coming through the hydrological system over a long time period a change in the injection time of a few hours is unlikely to make a big difference to the trace. Finally, when looking at the seasonal evolution of the system we looked at the breakthrough curve shape (in terms of the change in the number and prominence of peaks), which often evolved over time. Changes in the shape of the breakthrough curves were not considered in the velocity/discharge hysteresis studies mentioned by the reviewer (although some secondary peaks were *seen in Schuler et al. (2004)'s work the main peak was still clearly prominent during all tests).*

5. Influence of supraglacial streams

The authors note that the investigated pathways consist of multiple components: I agree with their interpretation that a slow system connects to preexisting channel. But the interpretation of the nature of this slow system is ambiguous if we do not know the partitioning between the different components; tracer tests yield information integrated along the entire pathway from the injection point to the detection site. The authors have explained that the dye injection in the debris zone was performed into the supraglacial flow but it was unknown where and how it connects to the interior of the glacier (P5379 C2160 L23ff). Due to the low transit velocity through the debris, the supraglacial component may be a considerable part of the entire pathway/ transit time (P5389 L10ff) and the tests do not allow valid and unambiguous conclusions about the nature of the sub- glacial system.

This is an important point. We did not give full details of all of the injection points for brevity, however we can provide more detailed information about the lower glacier streams in the table below.

Name	Distance to GS (m)	Description of injection site	Location
S1	997	A small, shallow, slow flowing stream, unclear when it becomes englacial.	On the southern edge of the northern lobe, originates from the ice cliffs in between the central and northern lobe.
S2	1295	A small stream at the base of an area of ice cliffs.	On the southern lobe, to the south of the central lobe and the C1 GPS point.
S3	1560	Very small stream, which appears to go down a moulin as there is no trace of the stream past this point, but the stream is covered by substantial boulders which hide the moulin.	In between the C2 and C3 GPS points on the lower glacier.
S4	2050	Dye into a very small stream that was believed to lead into a much larger moulin which could be heard beneath some large boulders.	On the top part of the southern lobe, on the bend of the glacier.
S5	2161	The largest stream network on the lower glacier, but the stream is still small and slow flowing, with a shallow gradient. It meanders beneath high ice cliffs, and is occasionally covered by debris. The moulin could not be directly traced because of access difficulties.	The S5 stream is the main stream on the eastern side of the lower glacier, which flows from east of C5 down to a moulin located 446 m from the injection point (straight line distance).
S5b	2024	Small supraglacial stream with a shallow gradient, high debris content, and meanders beneath short ice cliffs.	Is a tributary of the S5 stream, it flows from the area just downglacier of C4 through fairly flat debris to the confluence with the S5 stream just downglacier from the S5 injection point.
S6	2620	Small, slow flowing stream, some water ponded upstream.	Between C5 and C6 GPS points, above the bend of the glacier.
S7	2987	A small, shallow stream that flows at the base of some ice cliffs, with the stream disappearing into the ice as the gap between the ice cliff and the ice on the other bank of the stream close together. This may be analogous to the 'cut and closure' mechanism (Vatne, 2001). It is fairly certain the stream becomes englacial but it may not reach the bed for some time.	Is on the eastern side of the glacier in between C6 and C7.

S8	3149	Dye was poured into an englacial conduit, within which a stream could be heard flowing from above.	Situated in between the central and eastern moraines to the south east of the C7 GPS point.

As you can see unfortunately for some of the 2010 injections there is less clarity about when the streams became englacial (S1, S2 and S6). However, for S3 and S4, we were as sure as possible that the stream became englacial within a couple of meters of the injection point, for S5 we did know the location of the moulin and in 2011 measured the supraglacial flow velocity so that this could be accounted for (also note the comments made in point 3 above about the supraglacial flow velocities and how they compare to the trace transit velocities), S5b was traced because access to the S5 injection point was impossible, S7 did become englacial a short distance from the injection point but through the cut and closure mechanism rather than a moulin and the S8 injection was directly into an englacial conduit.

Section 3.5 has been edited to include this extra detail on when supraglacial streams on the lower glacier became englacial (lines 187-195).

Therefore, although there is doubt as to the length of the supraglacial network for S1 and S6 (S2 didn't return a trace), we can be reasonably certain that we have traced the englacial and subglacial network during the remainder of the traces.

6. Inflow modulation vs hydraulic damming

Interpreting the seasonal evolution of the channelized drainage system in Sec 5.3, the authors appear to get lost in terminology concerning "inflow modulation" vs "hydraulic damming". These expressions are largely synonymous but on P5387 L6: "unlikely that inflow modulation ... was the cause ... " " more plausible ... resulting in hydraulic damming" (L13). This is contradictory as long as the two terms are synonyms, if this is not the case, the authors need to better define the exact mechanisms behind these terms and how they differ.

Again, this is a valid point, as from the reviewer's earlier comments above our interpretation was not as clear as intended. This has been altered to clarify our points. The idea is that the change in the trace breakthrough curves between June and July is not due to inflow modulation that occurs over short timescales (i.e. due to changes in supraglacial discharges into the moulin over the day) but rather from the hydraulic damming caused by a decrease in the channel geometry sometime between June and July. Although inflow modulation and hydraulic damming describe a similar effect, inflow modulation is the idea that the transit velocity is determined more by the supraglacial rather than proglacial discharge (Schuler and Fischer, 2009), whereas hydraulic damming is the idea that water is backing up in the moulin (Nienow et al., 1996). Nienow et al. (1996) did use this latter term to describe the situation where the hydraulic damming was caused by an increase in the water input into the moulin (therefore also inflow modulation), but hydraulic damming itself can also occur because the conduit is more generally too small to transfer the inflow discharge efficiently (not just at peak discharge).

So in this case the hydraulic damming is due to the channel geometry not being large enough to transfer the water generally, rather than the water discharge increasing so the conduit is overwhelmed for a few hours at peak discharge.

The text in the second paragraph of section 5.3 (lines 409-421) has been edited to clarify this.

7. Technical points

Technical:

I recommend using different terms to better distinguish 'transit velocity' of a tracer traveling from A to B from 'flow velocity' of water at a given point. Even for an ideal tracer, to determine the mean flow velocity, one would need to know the length of the pathway which usually is unknown. We only can make a plausible/ minimum estimate of this length.

This is a fair point, the term 'flow velocity' has been changed to 'transit velocity' throughout the manuscript and in all figures.

Related to the point above, how were the distances used in the MS determined? Along an assumed glacier flowline or a straight line connection or ... ? The MS does not provide information on this point.

The distances were calculated using the methods described in Table 2, which was a straight line distance between the injection point and gauging station, but because of the bend in the glacier for all traces into streams above S4 the distance was that to S4 plus the distance from S4 to the injection point.

In sec 3.4, three meteo stations are described; sec 4 refer to measurements but it is left unclear from which of the 3 stations.

The meteorological data in section 4.1 is from the lower weather station (LWS). This has been clarified in the text (lines 228-237).

P5381 L2: sequence of table numbers should be consistent with occurrence in text, here Tab 5 is referred to before Tab 4.

Thanks for pointing this out, however we would envisage that it would be strange to put the 2011 dye tracing results before the 2010 dye tracing results in the paper. Yes it is usual convention to put figures and tables in the order in which they are mentioned, but perhaps it would make sense to make an exception here.

P5395, Reference to Kienholz (not "Keinholz")

Changed in the manuscript on line 675.

P5399, Tab 2: the authors made a particular choice for deriving these quantities from the tracer concentration curves and several assumptions are made. It would be more appropriate to describe the methodology in the text instead of providing minimalistic information in a table. The unit for A_c must be wrong if it is to represent the integral of concentration (ppb) over time (min). This applies also to Tab 4 and 5.

The unit for A_c has been changed to ppb min in Tables 3 and 4. We decided to use a table to give the quantities for brevity, however following the reviewers comments and also those of referee 2 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 204-221). This also includes a description of *how A_c was calculated. There have also been some alterations as appropriate to Table 2.*

Tab3: information on supraglacial discharge is required but here only one value is given for a few sites. What do these numbers represent? Seasonal mean values? How meaningful is the mean for interpreting tracer tests conducted under changing conditions? Does this table add information to that given in Tab5? Consider removing.

We have removed Table 3, and have instead put the values for S5, S7, S12 and S14 into the text in section 4.2, but as a range of values (lines 243-245 and 250-252). The averages were simply the average of the values we measured.

8. Figures

All Figs: The labels a), b) ... referring to subfigures should be larger.

This has been changed in all relevant figures.

Fig 3: b and c needed? The text refers only to a)

Figures 3b and c are mentioned in section 4.2, lines 242 and 243.

Fig4: include labels a), b) etc ... and refer to them. I am not convinced that the top panel is needed, the overview is too small and does not provide additional information to that in Fig1. Also I am wondering whether Fig 4 could be combined with Fig5 by having the basin outlines overlaid.

This figure has been simplified so that the top panel is now just an inset showing the location of the main panels, with a and b added. Although it's a good idea, adding topography to the supraglacial catchment figure would make the supraglacial catchment figure cluttered and difficult to interpret.

Fig 7a shows negative concentrations for 140610, demonstrating a calibration problem. For the same curve one is wondering about the significance of the signal against the background noise?

The negative concentrations are not a calibration problem since the fluorometer measures total fluorescence which includes the background. The background is removed before a relationship between dye concentration and fluorescence (also with background removed) is used to turn the fluorescence measurements into a dye concentration. In some cases variation in the background while the breakthrough curve comes through can mean calculated concentrations are negative. You are right in that there is more doubt about the significance of the signal for this trace than others, but we decided on balance to leave it in, considering the similarity of the timing of the peak with the other traces.

Fig 8 should also show the span of the individual u and P values, in addition to the mean.

Error bars have been added to this figure to show the range in the parameters measured.

REFERENCES: Gulley, J., Walthard, P., Martin, J., Banwell, A., Benn, D., Catania, G., 2012. Conduit roughness and dye trace breakthrough curves: why slow velocity and high dispersivity may not reflect flow in distributed systems. Journal of Glaciology 58(211): 915-925

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Schuler, T., U. H. Fischer, and G. H. Gudmundsson (2004), Diurnal variability of sub-glacial drainage conditions as revealed by tracer experiments, J. Geophys. Res., 109, F02008, doi:10.1029/2003JF000082.

Nienow, P., Sharp, M. and Willis, I., 1996. Velocity-discharge relationships derived from dye-tracer experiments in glacial meltwaters: implications for subglacial flow con- ditions. Hydrological Processes, 10, 1411-1426.