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

Anonymous Referee #1

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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 season 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 configuration.

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) 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. 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 behavior 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.

2) More careful interpretation of tracer tests is required.
In the present MS, low transit velocities are interpreted to result from an inefficient, distributed 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, regardless of drainage system configuration. The simplistic interpretation "low velocity = distributed drainage" is hence compromised.

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.

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

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 subglacial system.

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.

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.

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.

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.

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

P5395, Reference to Kienholz (not "Keinholz")

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.

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.

FIGURES

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

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

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.

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?

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

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