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Interactive comment on "A subglacial hydrological model dedicated to glacier sliding" *by* B. de Fleurian et al.

B. de Fleurian et al.

basile.defleurian@uci.edu

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The authors already mention the non-synchronism of the different datasets but then do not really care about it. The model is forced using the 1993 moulin input, results are then compared to tracer-derived drainage system evolution in 1990 and surface velocities in 1998. The timing of the borehole level observations used as "second metric" (P3468) is left unclear. Since the input to a large extent controls the behaviour of the drainage system, a more careful argumentation is needed to convince the reader that this non-synchronism is not a problem. For instance, the authors could have shown and compared the hydrographs for all 3 years (I am sure that the data is available).

The non-synchronism of the data is clearly an issue but could not be overcome. The

comparison of the hydrographs from the years 1990, 1991 and 1995 for which dye tracing experiment have been performed show a similar pattern with differences in both timing and amplitude of the discharge that does not seem to impact the evolution of the extent of the channelized system (As stated in p3470 lines 20 and followings of the initial manuscript). The 1993 hydrograph show a lower discharge between days 195 to 210 but at this time, the efficient drainage system is almost fully developed and so this discrepancy shouldn't lead to a major impact on the model results. The hydrographs are not shown in the paper as their comparison doesn't help the understanding of the simulations and would overload an already long paper which goal is not the reproduction of a given event but more the validation of a modelling approach. At last, the modelled velocities should be compared with the general pattern of a spring speed up event and the 1998 velocities are only provided to give a general pattern to these event characteristics, this particular event have been chosen because it is characteristic of the speed-up event and present a large number of measure locations. This last point have been clarified in the manuscript. The second metric that was cited in the preceding version of the manuscript is no more used in this revised version, we still present the water head issued from the model but for comparison between simulations only.

As a "first metric" to constrain the model, the authors compare simulated extent of the EPL to the extent of the channelized drainage system, as inferred from dye-tracer experiments. In recent years, there has been raised some doubt about the robustness of the interpretation of these tracer tests (Gulley et al., 2012; Werder et al., 2010, Schuler et al., 2004). Since from model results, also the mean macroscopic velocity of the fluid can be derived, it could be insightful to compare these to the tracer velocities, which are more robust, since they are measured and not subject to interpretation. Also, the authors should decide whether they would like to evaluate the "second metric" (borehole water level) or not. In the first case, the corresponding water level variations should be shown (Fig 8); in the latter, the last paragraph in Sec 4.1 becomes

obsolete.
The concern that has been raised about the interpretation of the tracer tests does not apply to our approach as we look for the shift between inefficient and efficient system and not to the existence or not of channels. The use of this metric allow to have a glacier wide parameter which is not the case of the macroscopic velocity for which we need to prescribe a starting point which could give significant difference in the measurement whether the injection point is located in the efficient or the inefficient drainage system. The borehole water level have been dropped as a metric and is only kept for different simulations comparison purpose.

Furthermore, I found the discussion in light of previous literature deficient, especially with Flowers et al (see below for references). Flowers and coworkers have adopted a similar concept of an "equivalent porous layer" to represent a multi-component, subglacial drainage system that evolves its capacity in response to discharge forcing. The Flowers-model has been rigorously tested using a wealth of field data from Trapridge glacier. The same model has also been coupled to an ice-flow model (Marshall et al, 2005; Flowers et al, 2005). Since the presented model is conceptually similar to the Flowers-model, a revised MS should include a discussion of potential differences in terms of both performance as well as computational efficiency (the Flowers model employs a pressure-dependent conductivity to account for drainage system evolution and seems therefore computationally more efficient than the dual-porosity approach employed here).
The main difference between our approach and the one by Flowers is that in their case the "equivalent porous layer" have to adapt it's conductivity to represent both inefficient and efficient drainage systems. In our approach, the EPL is used only when an efficient drainage system is needed to drain the water produces on or under the glacier. A discussion comparing our approach to the existing one have been included in the manuscript.

Another bold statement made in the MS is that the presented approach "has the advantage of requiring a lower bedrock topography resolution", but it is left unclear how much actually was gained by that. Is it the resolution of the hydrological model or of the full-Stokes glacier model which represents the limiting factor when it comes to computational expenses? What does "required" resolution mean? Required for what? And what is the sensitivity to the spatial resolution? The hydraulic gradient drives the flow through the system and of course the gradient depends on the spatial resolution. So one has to expect that the computed water pressure and hence drainage configuration will display some sensitivity to spatial resolution and it would be good to have it demonstrated.
The present application on a small valley glacier does not allow to use a coarse topography due to the size and steep slope of this glacier. The design of the model was done such that the grid resolution should not be a problem when performing simulation on larger catchment. The computational limitation of the coupled model come from the full-Stokes glacier model and so the grid refinement needed for the hydrological model should not be higher than the one needed by the ice dynamic model in order not to increase the computation time which is needed by the later.

1 Detailed comments

As commented by Referee 1, please replace "transmitivity" by "transmissivity" throughout the MS.
Done.

P 3451
L 16-18: "(in)efficient draining systems" vs"(in)efficient drainage system"

Changed.

P3452
L 26: “the basal drag of glaciers. . .” (“s” missing)
Corrected.

P3453
L9: “. . .the upward pointing vector normal to. . .”
changed.

P3454
L13: “filtration” do you mean “percolation”?
The filtration velocities are the one defined in Darcy’s experiment. Their definition is given underneath
L16: “. . .the velocities...”
Corrected.

P3458
L 11: “specificities” “characteristics”
Changed, the sentence has also been reworded to make our point clearer.
L14: what do you mean by “resolution of the equation”?
Solving was meant in place of resolution, it has been changed in the manuscript.

P 3459
L11/13: change to “i) the EPL is in a transitional state; ii) the EPL is in an active state”, referring to ii) as active in an efficient state is confusing since the EPL is per se hydraulically efficient (different usage of “efficient”).

Effective is used in place of efficient to keep the differentiation between active and inactive and transitional and effective on an other level.
L20: “. . .the EPL becomes active,. . .”
Changed according to the preceding.
L24: this shows that the model does not yet include an important characteristic of the system it attempts to represent, largely limiting its applicability.
We are aware that this limitation prevent us of modelling the closure of the channel system in fall. This limitation should not impact modelling the spring speed-up events presented in this study. Adding a variable transmissivity to the EPL and so a closing mechanism is part of the further development that will be achieved on the model.
Sec. 2.3: assume that the EPL is in an active state: what happens when he > hmax? It appears that the model allows this situation.
In this case, the Domain of the EPL will then be increased to allow the draining of a larger water volume.

P3460
L8: it may be worth to limit $\varphi > 1$
Even if not necessary, it seems reasonable to limit the term before the head difference in equation 13 of the manuscript for stability reason. However, this does not need to limit $\varphi > 1$ as it depends on other model parameters.

P3461
L4: “. . .is the solution vector”
Corrected.
Eq 17-22: double use of variables: K_j is not the same as in eq 8 and 10 (hydraulic conductivity)!
changed to X.
L12: “. . .and δt IS the time step” (“is” missing)

Corrected.

P3462
L9: "...is then treated as a source term..."
Corrected as suggested.

P3465
L14: "...resting ON ..."
Corrected.

P3466
L 6/8: switched notation? Shouldn't it be $T_j$in bold in L6 and T_j italic in L8?
Corrected.

P3467
L29: use "length of the EPL" instead of "maximum length of the EPL" throughout the MS. You have defined the length in Fig 5. Referring to the "maximum length" is confusing here since you are referring to an evolving quantity and here you actually refer to the seasonal minimum.
Replacement have been made as suggested throughout the manuscript.

P3468
L 23: "...and a shorter EPL"
Changed.
L25: "...is dominated by the EPL"
Changed.

P3469
L1: "high values...lead to..."
Corrected.
L2: "the observed minimum extent of the channelized drainage system" the extent of the channelized drainage system is increasing during the ablation season and hence at its minimum "at the beginning of spring"
The sentence has been reworded.
L5: where does the lower bound on EPL length (200 m) come from? is it based on observations?
This value is from personal communication of D. Mair, it has been left aside on the new version of the manuscript as it was not completely necessary in the parameter selection process.
The description of Fig 6 should be improved to increase readability!
L 25: "a large leakage factor implies a low exchange between IDS and EPL".
The sentence has been reworded.

P3470
L23: "Fig 10 shows..." Fig 10 does not specify 1993 moulins. Also, it is awkward to refer to Fig 10 before having referred to Fig 8 and 9.
The 1993 moulin position have been put on figure 4 and the text refers to it.

P3471
L16: "...the...metric shows..." ("s" missing)
Corrected.
L20: "...at the opening of the channelized drainage system...". The observations refer to the channelized drainage system which is represented by an EPL in your model.
The sentence has been reworded.
L23 ff: this result is not that surprising, given that the locations of input moulins were

prescribed to match the observations.
We agree with the remark but we think that it is worth mentioning that our model does not create completely wrong drainage path. Moreover, a bad parameter set could have lead to an over or underdeveloped drainage system which would have been seen here.

P3472
L26: "...are fixed to 1 and 3, respectively"
Corrected.

P3473
L 3: please insert a small horizontal space in $\mathrm{mPa}^{-3}\mathrm{s}^{-1}$ to make clear that the m refers to meter and is not a prefix to "Pa"
The spacing is already present as per the copernicus rules.
All multi-panel figures should be labelled a, b, c and in the descriptions should be adjusted accordingly (Fig 5-15).
Letters have been added in the multi-pannel figures as suggested.

P3486
caption to Fig 4: "The glacier surface elevation is contoured..."
Added.

P3488
cap Fig6: "the dashed line..." there is no dashed line in the figure!
The dashed line appear in the lower panel to show the flotation limit at the considered point.

P3490
cap Fig8: "...compared to the tracer-derived position..." and again, there is no

"dashed line" in the figure but mentioned here.
The sentence have been reworded. The dashed line appear in the lower panel to show the flotation limit at the considered point.

P3492
cap Fig10: " The moulins used for the simulations..." why are not all moulins used?
All the moulin from year 1993 are used for the simulation, the sentence have been reworded
Fig 11 has been included in Figs 8 and 9 and is not needed.
Figure 11 have been replaced to show the sensibility of the model to its grid resolution.

P3494
Fig12: caption and y-axis of lower panel: "horizontal" velocity instead of "longitudinal" ?
Right hand y-axis of upper panel: unit should be $\mathrm{m}^3\mathrm{s}^{-1}$
The longitudinal velocities are used in place of the horizontal ones as they are the ones presented in Mair (2003), units have been changed.

P3495
Fig 13: caption and y-axis of lower panel: "horizontal" velocity instead of "longitudinal" ?
The longitudinal velocities are used in place of the horizontal ones as they are the ones presented in Mair (2003).

References:

Flowers, G.E. and G.K.C. Clarke. 2002. A multicomponent coupled model of glacier hydrology, 1, Theory and synthetic examples. J. Geophys. Res., 107(B11), 2287, doi:10.1029/2001JB001122.

Flowers, G.E. and G.K.C. Clarke. 2002. A multicomponent coupled model of glacier hydrology, 2, Application to Trapridge Glacier, Yukon, Canada. J. Geophys. Res., 107(B11), 2288, doi:10.1029/2001JB001124.

Flowers, G.E., S.J. Marshall, H. Björnsson and G.K.C. Clarke. 2005. Sensitivity of Vatnajökull ice cap hydrology and dynamics to climate warming over the next two centuries. J. Geophys. Res., 110, F02011, doi:10.1029/2004JF000200.

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

Marshall, S.J., H. Björnsson, G.E. Flowers and G.K.C. Clarke. 2005. Simulation of Vatnajökull ice cap dynamics. J. Geophys. Rev., 110, F03009, doi:10.1029/2004JF000262.

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

Werder, MA and Schuler, TV and Funk, M. 2010. Short term variations of tracer transit speed on Alpine glaciers. The Cryosphere. 4, 381âĂŤ396.

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