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Discussions

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

Anonymous Referee #3

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Review of "A subglacial hydrological model dedicated to glacier sliding" Submitted by B de Fleurian, O Gagliardini, T Zwinger, G Durand, E le Meur, D Mair and P Råback

This manuscript (MS) describes a dual porosity approach to represent the subglacial hydraulic system. The model is solved in a finite-element framework and coupled to the Elmer/Ice ice-flow model. The model is applied to simulate one ablation season of Haut Glacier d'Arolla demonstrating the overall model behavior. Model parameters are constrained by several observations from that glacier. This validation effort makes the MS a valuable contribution to TCD, whereas a pure model development paper would have been a more appropriate contribution to GMD. Nevertheless, there are a number of shortcomings that need to be addressed in a revised version, before the MS should be published.

General:

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 behavior 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).

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). 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.

Furthermore, I found the discussion in light of previous literature deficient, especially wrt 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).

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.

Detailed comments:

As commented by Referee 1, please replace “transmitivity” by “transmissivity” throughout the MS.

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

P3452 L 26: "the basal drag of glaciers. . ." ("s" missing)

P3453 L9: ". . .the upward pointing vector normal to. . ."

P3454 L13: "filtration", do you mean "percolation"?

L16: ". . .the velocities..."

P3458 L 11: change "specificities" to "characteristics"

L14: what do you mean by "resolution of the equation"?

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").

L20: ". . .the EPL becomes active,. . ."

L24: this shows that the model does not yet include an important characteristic of the system it attempts to represent, largely limiting its applicability.

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.

P3460 L8: it may be worth to limit $\varphi >= 1$

P3461 L4: ". . .is the solution vector"

Eq 17-22: double use of variables: $\mathbf{K_j}$ is not the same as in eq 8 and 10 (hydraulic conductivity)!

L12: ". . .and Δt IS the time step" ("is" missing)

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

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

P3466 L 6/8: switched notation? Shouldn’t it be $\mathbf{T_j}$ in L6 and T_j in L8?

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.

P3468 L 23: ". . .and a shorter EPL"

L25: ". . .is dominated by the EPL"

P3469 L1: "high values. . .lead to. . ."

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"

L5: where does the lower bound on EPL length (200 m) come from? is it based on observations?

The description of Fig 6 should be improved to increase readability!

L 25: "large leakage factor implies a low exchange between IDS and EPL".

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.

P3471 L16: ". . .the. . .metric shows. . ." ("s" missing)

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.

L23 ff: this result is not that surprising, given that the locations of input moulins were prescribed to match the observations.

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

P3473 L 3: please insert a small horizontal space in "m Pa^{-3} s^{-1}" to make clear that the "m" refers to meter and is not a prefix to "Pa"

All multi-panel figures should be labeled a, b, c and in the descriptions should be adjusted accordingly (Fig 5-15).

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

P3488, cap Fig6: "the dashed line. . ." there is no dashed line in the figure!

P3490, cap Fig8: ". . .compared to the tracer-derived position. . ." and again, there is no "dashed line" in the figure but mentioned here.

P3492, cap Fig10: "The moulins used for the simulations. . ." why are not all moulins used?

Fig 11 has been included in Figs 8 and 9 and is not needed.

P3494, Fig12: caption and y-axis of lower panel: "horizontal" velocity instead of "longitudinal" ?

Right-hand y-axis of upper panel: unit should be m^3 s^{-1}

P3495 Fig 13: caption and y-axis of lower panel: "horizontal" velocity instead of "longitudinal" ?

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.

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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.

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