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

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This paper describes a dual porosity approach to modelling the subglacial hydrological drainage system, and couples the model to an ice flow model. The drainage system model is an alternative to recently published models in that it treats efficient drainage as a porous layer (EPL) rather than with explicit channels. Results are compared qualitatively with observations from Haut Glacier d'Arolla - again this is something that has not been attempted for other 'coupled' models.

The approach seems a reasonable one, and should be explored further. The paper is well written and, on the whole, well explained. However, I have some issues / questions about the implementation that I think should be resolved.

My primary concerns relate to the transient growth of the equivalent porous layer (EPL) and to the fact that there is no mechanism for the layer to ever disappear. The model seems to be appropriate only for describing the winter - spring transition of the drainage system. Whilst this is perhaps the most exciting time to observe the drainage system and its influence on ice motion, recent observations from large ice sheets (for which this approach is ultimately indented, I assume) suggest that the whole annual sequence is important for ice dynamics. Without a mechanism to allow the EPL to change size dynamically (ie to shrink as well as to grow), I am unconvinced by the appropriateness of this approach. Linked into this is of course the physical mechanism for creating and destroying the efficient drainage system. Other, more involved, models of the drainage system use the energetics of water flow to capture the evolving transmissivity of the drainage system, with channels growing due to excessive discharge. Here, the efficient drainage is switched on due to pressure reaching flotation, which is a different mechanism (this is similar to the model of Boulton et al (2007), which should perhaps be referenced). The distinction in this initiation mechanism should be discussed more I think. In addition, the details of how the initiation proceeds during the 'transient' stage need to be described more clearly (see below).

The question of an evolving drainage capacity for the EPL is one that has to be treated in the future evolution of the model. Our goal in the present paper was to present the approach while keeping the number of parameters to a minimum in order to get a convincing parametrization of the model for the modelling of the spring transition between inefficient and efficient drainage system of a glacier. Introducing a mechanism to allow the evolution of the EPL transmitivity would need a new parameter that would probably be derived from the water energetics and creep/closure mechanism but should be scaled to the use of our equivalent layer. The water flux relationship that is used for the channel opening of the major drainage system model could not be transposed to the EPL. Using the water pressure instead of the water energetics and creep mechanism allow a simple parametrization of the EPL opening that proves to be quite reliable. The approach of Boulton et al (2007) is somewhat different in the way that they computes the water pressure in the channels from the water discharge and so the opening mechanism of the channels in their study is also driven by water energetics and creep closure.

There is apparently no mechanism to turn the EPL off again, so once it has been initiated the drainage system is then in its efficient state for ever more. Ideally, I think there should be a solution to this issue before the paper is published, but at the least it should be heavily flagged.
As stated above, the EPL has a fixed transmitivity. We are aware that this limitation prevents 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.

I think it would be beneficial for the conclusions to discuss some of the merits and drawbacks of this model rather than simply to summarize what has been done in the paper.
A discussion part has been added to assert the advantages and drawbacks of the approach.

Finally, the title of the paper seems slightly odd - particularly the 'dedication' to glacier sliding. The relation to sliding does not seem to be the main focus of the paper, and I'd suggest 'A two-layer model for inefficient and efficient subglacial drainage linked to ice flow' or something similar might be more descriptive.
The title has been changed

1 Specific comments

The value used for the layer thicknesses e_j are not mentioned anywhere that I could find. It goes into determining the S_j, and I think this parameter is also pretty uncertain (along with the transmissivities and leakage) ? It should have quite an impact on the temporal pressure fluctuations.
The value of e_j were not given in the paper, this has been corrected. Some simulations have been performed to assess the sensitivity of the model to these parameters. The model shows similar response to the variation of layer thickness than the one that is observed for a change in IDS transmissivity in the case of the transient simulation. Not to overload an already long manuscript, no figure has been added and this point has been treated in part 4.3.

The mechanism for initiation and transient evolution of the EPL needs to be better explained. It is not clear how the water pressure in the IDS ever decreases once it reaches h_{max}, if such nodes are then given a Dirichlet condition $h = h_{max}$. What is the criterion for stopping applying this condition?
The part explaining the activation and evolution of the EPL has been rewritten. The decrease of the water head in the IDS is only the fact of the transfer flux that is activated when the EPL is active. This decrease of the water head makes the application of the upper limit unnecessary.

p3458,line19-20 - These two alternatives for the EPL do not seem to be correct since it seems to be possible to have the EPL and $h_i < h_{max}$.
The two alternatives have been reworded to correct this error.

What is the initial water pressure in the EPL? It seems that the overflowing water from the IDS has to build up the pressure in the EPL before it is expanded, but the time

taken for this to happen is presumably dependent on where it starts. It seems to me to make sense to imagine that it starts at h_{max}, since that's the pressure in the IDS. Then it should immediately expand downstream.
We chose to initialize the head of the EPL to a minimum rather than to the head of the IDS. This treatment allows a short time before the spreading of the EPL which would represent the time that is needed to open an efficient drainage system.

The method for expanding the EPL sounds rather odd, and possibly grid dependent; see p3459,line17-19 and p3462,line12-16. If it is always expanded in the downstream direction, why does it not evolve into a thin structure extending from the initiation point to the margin? And what happens if the hydraulic potential is completely flat? The method describes sounds as if the EPL grows infinitely fast during a time-step it can be extended by many nodes, and this is presumably why the step jumps in the maximum length of the EPL are seen in the figures (it is however not clear how the maximum length of the EPL is actually defined in general).
The expansion procedure of the EPL has been rewritten to make it clearer. Depending on the grid size and available volume of water the domain will increase from one to several grid cell during a time step. The speed of expansion is addressed by the available volume of water rather than the grid resolution. This is due to the fact that the available volume of water will spread on the active EPL domain and so larger grid size will lead to a lower water head. The extension of the EPL is done on the lower closed point in term of hydraulic potential which allow to widen the EPL for larger discharge which explain the fact that the EPL does not evolve in a single thin structure. This point has been clarified in the manuscript.

For the leakage rate (13), I am not at all convinced by the inclusion of S_j. Why should storage capacity affect the leakage rate? Since S_j is dimensionless there is no need to included it for the reasons described following (13). See Pimentel and Flowers (2010).

p3469,line5 where does the lower limit 200m come from?
The storage capacity is necessary to convert water heads to water volume. The difference with Pimentel and Flowers is that in their case, the leakage is treated in term of pressure.

There is not much description of how the ice flow and hydrology models are coupled. Given that this model is advocated as a simple approach that should be useful for doing this, I think greater discussion of this should be included. In particular, the iterative procedure for the hydrology model is described in detail, but no mention is made of the iterations required for the coupled model. I'd have thought that the changes in the normal stress that induce changes in h_{max} might cause potential stability issues. What sort of time steps are required?
We encountered stability issues when trying to resolve the coupling for the highest part of the glacier where large slopes and small ice thickness render the ice flow model unstable. For the rest of the glacier, the coupling between ice and hydrological model is rather stable. The time step used are of the order of half an hour for the hydrological model which is necessary to capture the evolution of this system during a day and the ice model is then solved every 12 hours. A paragraph has been added to give more information on the coupling scheme.

2 Minor comments

Some of the citations in the introduction are incorrect or not the best examples and these should be checked more carefully. The fact that many observations do not clearly show a relationship between water pressure and sliding speed should also be acknowledged.
The citations have been verified and changed or completed where necessary.

The description of the model in 2.1 could be shortened I think. This is textbook hydraulics, and I think equation (11) could be written down from the outset.
Considering the fact that the model is aimed at glaciologist and not hydrologist, we thought that a more general introduction of the basic equation could be profitable.

p3458
line4 - Just because the EPL is modelled as a porous layer does not mean it is 'physically inefficient'. A highly transmissive layer can be very efficient.
The sentence has been rephrased to make this point clearer.
line27 - The description of an infinite reservoir is possibly confusing terminology.
Reservoir has been replaced by Sink.
The description of phi as a leakage factor is confusing terminology I think, as factors are typically dimensionless. Leakage length-scale or leakage coefficient (for the inverse of phi) might be more appropriate.
Factor has been replaced by lenght-scale.

p3466
line25 - 'down to' rather than 'up to'?
Modified.

p3469
line21 - I would not describe this as saw-tooth behaviour, which implies sharp oscillations rather than an individual minimum. It is also not clear there is enough resolution on the curve to say that the minimum is actually a sharp peak.
The sentence has been reworded to take these remarks into account.

p3471
line3 - T_e rather than T_i?
Modified.

p3474
line11 - I think I'd qualify the 'matches'. The agreement does not look perfect.
It is the general pattern of the spring speed up that matches the one that is usually observed on Haut glacier d'Arolla. As stated further in the paper, we don't aim at a perfect agreement due to the assumptions that are done and the fact that the data are not acquired simultaneously.

Table 2
why aren't all the variables classified as well or poorly known?
Variables with a reference weren't classified as well or poorly known, it has been changed. Unused L has been removed.

Figures 5,6,7
Units for T should be m^{2}s^{-1}.
The unit has been changed.

Figures, 8,9,11,12,13
'Julian Days' suggests this is the year 4713 BC. 'Day of year' would be better.
The axis label has been changed.

Figure 8 caption
the 'computed' position of the head of the channelized system seems an odd choice of words. It might be better to say 'inferred', or similar.

The caption has been rephrased as :"Evolution with time for different values of the IDS transmissivity of : (a) the maximum length of the modelled EPL (lines) and (b) the IDS water head. The position of the head of the channelized drainage system derived from observations (black dots) is presented in (a) for comparison".

References G.S. Boulton, R. Lunn, P. Vidstrand, S. Zatsepin 2007 Subglacial drainage by groundwater-channel coupling, and the origin of esker systems: part IIâ A Theory and simulation of a modern system. Quaternary Science Reviews 26, 1091–1105

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