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The Cryosphere Discussions

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

Anonymous Referee #2

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General comments

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).

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.

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.

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.

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 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?

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}.

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.

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 timestep 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).

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?

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?

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 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.

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.

p3458,line27 - The description of an infinite reservoir is possibly confusing terminology.

The description of phi as a leakage 'factor' is confusing terminology I think, as factors are typically dimensionless. Leakage 'lengthscale' or leakage 'coefficient' (for the inverse of phi) might be more appropriate.

p3466,line25 - 'down to' rather than 'up to'?

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.

p3471,line3 - T_e rather than T_i?

p3474,line11 - I think I'd qualify the 'matches'. The agreement does not look perfect.

Table 2 - why aren't all the variables classified as well or poorly known?

Figures 5,6,7 Units for T should be mˆ2/s.

Figures, 8,9,11,12,13 - 'Julian Days' suggests this is the year 4713 BC. 'Day of year' would be better.

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.

References

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

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