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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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1 Major concern 1

The procedure for the expansion of the EPL (p. 3462) is unclear and appears to be resolution-dependent. Specifically, it is not stated how far the EPL is extended when the efficient drainage system needs to be expanded. Is this a single grid cell? What is the grid size used for the example application? Is the model sensitive to the grid resolution used? If the EPL expands by an entire grid cell, it seems the speed of the expansion of the efficient system could be sensitive to grid resolution – if you reduce the resolution by half, then the efficient drainage system would expand twice as far when this method is applied. This has clear consequences for the calibration to the

observed extent of channelized drainage on Haut Glacier d'Arolla in section 4.3. Given that the primary argument for the hydrology model formulation used here is that it does not require high resolution necessary to resolve individual channels, the sensitivity of this method to grid resolution is important to assess. Also, the model description does not describe exactly how the volume of water in the EPL is redistributed after the EPL domain is expanded (p3459/L24). Finally, is there a physical justification for giving the EPL and the IDS the same hmax? Expansion of the EPL is presumably quite sensitive to the value of hmax.

The part explaining 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. Specific size of the grid for the presented experiments is 50m and performing new simulation for different mesh (25 to 100m) does not lead to significant differences in the model results. These results have been added to the Figure 11 to convince the reader of the low sensitivity of the model to its grid refinement. The definition of hmax at the flotation limit has been chosen to reflect the fact that from this pressure the lift of the glacier give way to a higher drainage capacity. For the case of the EPL, this upper limit could probably be modified to use the hydrofracturation pressure but regarding the fact that the water head in the EPL increase quickly, the definition of the upper limit does not seem to be so important due to the low storing coefficient of the EPL layer.

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2 Major concern 2

Limitations of the hydrology model need to be clearly identified. While it is clear that the authors intend to introduce a simplified subglacial hydrology model, there is little discussion of the limitations with this approach relative to more sophisticated treatments of subglacial hydrology and their potential impacts on model results. For example, the capacity or transmissivity of both drainage systems has no ability to evolve (c.f. , e.g. (Alley, 1996; Flowers, 2002; Ian J. Hewitt, 2011)) 2) and ice dynamics is only a minor input to hydrology (c.f., e.g. (Ian J. Hewitt, 2011; Kamb, 1987; Kessler Anderson, 2004; Schoof, 2010)). While making such simplifications may be desirable and justified for application of a simplified empirical model, the consequences of such limitations should be discussed. For example, as the authors note, the fixed capacity of the EPL prevents it from accommodating additional inputs late in the season (p3473/L28). This result that the hydrological system can become more sensitive to inputs later in the season is contrary to most modelling approaches and observations. On a related note, presumably the intention of formulating a simplified approach to subglacial hydrology is to facilitate modelling at large scales (e.g. whole ice sheets), and the example of Haut Glacier d'Arolla is used primarily because it is so well-constrained. However, that intention is not explicitly stated; for process-scale studies of small mountain glaciers (as is done here), a more physically-based approach to subglacial hydrology would likely be preferred.

A discussion part has been added to assert the advantages and drawbacks of the approach.

I am concerned (or perhaps confused) by some inconsistencies in the constraints used for the model calibration: p3469/L2: On p3467 it is explained that the earliest dye tracing experiments were on 10 June and the proglacial discharge was already 10x higher than winter. Yet, here on p3468 are discussed channel extent observations for 'beginning of spring'. If these descriptions are referring to the same observations, their meaning is inconsistent – the earliest available dye traces appear to represent some

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time significantly later than the beginning of spring. p3469/L10: Similarly, here a minimum admissible channel extent of 200 m is stated. Where does this value come from? Based on my previous comment, it seems like there is no minimum admissible value based on the available observations, since the earliest observations occurred after the subglacial hydrological system had begun to evolve for the season.
This is clearly a personal mistake, end of spring was meant here, this mistake has been corrected throughout the manuscript. The 200 meters minimum channel spreading was taken as a reasonable value for an end of winter configuration. This value has been dropped in the revised version as the modelling of the EPL spreading gives a sufficient constraint on the upper value of the IDS transmissivity.
Title: I strongly recommend modifying the title to be more descriptive. As the hydrology model formulation is highly unusual, emphasizing the unique approach in the title would alert potential readers. Similarly, heralding this paper as 'dedicated to glacier sliding' is not commensurate with the length spent on describing the application of the combined hydrology/ice flow model.
The title has been modified to take these remarks into account.
In a modelling context, the term 'coupled' typically is used to refer to model components that transfer information between components which substantially affects subsequent calculations. In the present study, the hydrology model only uses the normal stress computed from the ice dynamics model, which presumably does not vary much in time. While the term 'coupled' is technically correct here, it would be nice to see a simple assessment of how important this two-way flow of information appears to be. A few sentences may be adequate to provide an indication of how strong the coupling is – does the normal stress vary enough to substantially impact the hydrology?
Due to the pressure description of the hydrological model, the only variable that is needed by the hydrological model from the ice dynamic one is the normal stress. In the other way, the hydrological model send the water pressure to the ice model in order to compute the sliding velocity. Small modification in the normal stress could impact the hydrological system if they lead to an activation of the EPL. Regarding this point,

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the importance of the coupling is difficult to quantify as a small difference in the stress coupling could lead to different configuration of the draining system and so different local results of the model.

3 Other Specific Science Comments and Technical Corrections

Consider replacing 'transmitivity' with 'transmissivity' throughout the manuscript. Many of the figures are missing a, b, c, labels for the subplots. Please include them to aid in referencing.
Transmitivity has been replaced as suggested.

p3450
L2: The statement about the importance of water at the 'interface between ice and bedrock' is not incorrect, but given the paper's emphasis on describing water interactions within a sediment layer, more general wording would be more appropriate here (e.g. 'bed' instead of 'bedrock').
The replacement has been done as suggested.

p3451
L7: This statement is accurate, but the citations are not all entirely appropriate. Walder (1982) does not present field observations. While both Anderson et al. (2004) and Bartholomaus et al. (2008) hypothesize about the importance of water pressure in inducing sliding, neither study presents water pressure data. Examples that fit this statement more accurately might include: (Bindschadler, 1983; Iken Bindschadler,1986)
Citations have been modified.
L10: Similarly, here Schoof (2010) is not entirely appropriate here as there is no ice

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flow model included in that study – instead an empirical sliding relation is used. A more appropriate references might be (I J Hewitt, 2013). Other possible references include (Arnold Sharp, 2002; Goeller, Thoma, Grosfeld, Miller, 2013; Johnson Fastook, 2002)
Citations have been modified.
L23: While these references are accurate here, more general references to include would be: (Fountain Walder, 1998; Schoof, 2010).
Suggested references have been added.

p3452
L2: More general references here would include: (Alley, 1996; I J Hewitt, 2013; Ian J. Hewitt, 2011; Kessler Anderson, 2004; Pimentel Flowers, 2010; Schoof, 2010)
We kept the initial references as the goal was to present studies based on measurements and not modelling at this point.
L12: Another study demonstrating the similarity of karst and glacial hydrology is (Gulley et al., 2012)
Reference has been added.

p3453
L19-21. This sentence is awkward and confusing. Consider rewording or breaking into two.
The sentence has been reworded.
L23: Schoof et al. (2012) does not consider channels.
This has been modified, Schoof (2010) was meant here.
L22: It may be more general to just say 'lower spatial resolution' rather than 'lower bedrock topography resolution'.
Modified as suggested.
L25: Is it meant here that the use of a diffusion equation allows an easy implementation of implicit time-stepping (due to the lack of an advective term), and the ability

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to implement implicit time-stepping is what reduces computational cost? If so, please elaborate to make this clear.
A sentence has been added in the manuscript to make this point clearer.

p3454
L19: Awkward sentence here with 'represents' used twice. Consider rewording.
Sentence has been reworded to avoid repetition.

p3457
L2: The description is confusing. Please elaborate what is meant by 'surface Qj'. Does this mean inputs of surface meltwater to the bed?
Q_j is the flux per unit of surface, parenthesis have been added to clarify the point.

p3458
L16: What is mean by 'specificities' here?
The sentence has been reworded to state the characteristics of the efficient drainage system (low storing capacity and high conductivity)
L30-32: The description of infinite reservoirs here is potentially confusing. Use of the word reservoir implies a source of water, so it sounds on L33 that water is draining from a subglacial lake or the ocean. Perhaps describing this concept as an 'infinite sink' would be more intuitive.
Reservoir has been replace by sink throughout the manuscript.

p3459
L22: This sentence would be clarified to say 'due to the zero flux boundary condition on the EPL.'
The sentence has been modified as suggested.

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L23: What is meant by activated downstream? I realize this is discussed in section 2.5, but it might be clearer to replace ‘downstream’ with ‘down the hydropotential gradient’ or something like that.
Replaced as suggested.
L24: For clarity, end this sentence with ‘all over the active EPL domain.’
EPL has been added as suggested.
L29: By closing, is it mean shutting off entirely? Presumably he can decrease.
The closing procedure discussed here would rather be a decrease in EPL efficiency finally leading to the closure of the EPL. The manuscript has been modified to clarify this point.

p3460
L17: Does the term ‘sediment layer’ here equivalent to ‘IDS’? If so, I would recommend using ‘IDS’ for consistency.
’sediment layer’ has been replaced.

p3462
L12: Perhaps ‘resolution’ should be spelled ‘re-solution’.
What was meant was solving of the equation, the sentence has been reworded to take into account remarks from the other reviewers.

p3464
L18: Please state which hydrological component is used to calculate N and why.
A sentence has been added to clarify this point.
L25: The formulation leads to a decrease in basal drag for low sliding velocities, not high.
The formulation does lead to a decrease in basal drag with higher velocities once the Iken’s bound has been exceeded. The Sentence has been reworded for clarity.

p3467
L9: Related to the first major issues described above, the EPL transmissivity would be expected to change in time as channels grow larger and more efficient. This should be acknowledged here.
The steady transmissivity of the EPL has been acknowledge in part 2.3.
L23 and p3468/L10-18: The strategy described here states: “The comparison is done using two metrics constructed using large scale features of the hydrological system.” However, the section ends with saying the second metric is not used in this study. I agree that using borehole water water pressures is very challenging and need not be considered here. However, to state that it will be used and then say it won’t be used two paragraphs later is confusing. The whole description of the second metric should either be deleted, or the presentation should be reorganized to simply present why borehole water pressure observations are not considered.
The paragraph has been rewritten to make clearer the fact that the water heads are only used to compare simulations without a comparison to the actual borehole water pressure.

p3468
L22: These units appear to be incorrect. I would expect them to be [m s-1].
Changed.

p3469
L10: How does the leakage factor imply ‘low efficiency in the EPL’? The leakage factor represents the efficiency of the coupling between the two hydrological systems, while the transmissivity represents the efficiency within the EPL itself.
The leakage length-scale obviously impact the transfer between the two layers, the manuscript has been modified to state the proper mechanism.

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p3470
last line: Please elaborate what is meant by ‘fully developed EPL’.
The fully developed EPL is the one that is present at the end of summer and which allow to drain the water discharged at the base of the glacier by the existing moulins. The manuscript has been modified to make this point clearer.

p3471
L25: Replacing ‘EPL’ with something like ‘efficient drainage system’ would be more appropriate here since the ‘EPL’ is a specific implementation of efficient drainage specific to this model, and not a general concept.
Replaced as suggested.

p3473
L7: Does this mean that sliding speed is assumed to be independent of water pressures during winter? Often, wintertime water pressures are observed to be quite high (e.g., Kavanaugh, 2009). How much of the resulting modelled surface speed is due to sliding? Are there any constraints for Haut Glacier d’Arolla on the amount of sliding occurring during winter?
The only measured sliding velocities on Haut Glacier d’Arolla were on the lower part of the glacier (Willis, 2003) and yield winter sliding velocity from 10 to 90% of the surface velocity. At these points, winter water pressure and sliding seem to be independant as boreholes with higher winter pressure can exhibit higher sliding velocities. In the model, the sliding velocity represent roughly 10% of the overall velocity during winter to reach 60 to 70 % of the overall velocity during the spring speed-up events.

p3474

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L20: Change ‘hypotheses’ to ‘assumptions’.
Changed as suggested.

Table 2: Why aren’t all parameters classified as pkp or wkp?
Variables with a reference weren’t classified as well or poorly known, it has been changed and the unused L has been removed.
Table 3: Consider adjusting the caption to differentiate this table from the previous, e.g. ‘Values of the tunable hydrological parameters’
Changed as suggested.
Figure 2: The black text on the dark grey shading is difficult to read.
This text has been change to white.
Figure 5: Is the right panel correct, that the head is less than 2500 everywhere? Is not the bed elevation higher than that over much of the glacier? Perhaps showing water pressure instead of head would facilitate comparison between the three panels.
We now display the pressure, the water head in the right panel was effectively bellow 2500 m due to a very large transmissivity of the IDS.
Figure 8: The caption is slightly confusing. The statement “computed position of the head...” sounds like it might represent computed model output, but if I understand correctly this comes from observations. I realize that the word ‘”computed” indicates that these are not direct observations, but modifying the language would clarify this.
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."
Figure 9: The grey line here is difficult to see.
The grey line has been darkened.
Figure 12: Just a suggestion, but including a plot of he here would help demonstrate the speedup feature around day 230.
The EPL head has been inserted on panel **(b)**

Figure 14: Include a note of which year is modelled.
The year of the simulation (1993) has been added.

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