The Cryosphere Discuss., 7, C1682–C1691, 2013
www.the-cryosphere-discuss.net/7/C1682/2013/

The Cryosphere Discussions

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

Anonymous Referee #1

Received and published: 6 September 2013

—General Comments—

The submitted paper "A subglacial hydrological model dedicated to glacier sliding" by de Fleurian, et al. presents a novel model for subglacial hydrology and describes an application to the Haut Glacier d'Arolla using a variety of field observations. The hydrology model uses a dual-continuum approach that allows both inefficient and efficient drainage to be modeled together in a distributed manner that avoids the need to specify the location of individual efficient channels. The hydrology model is used to force an ice sheet model, demonstrating that the combined model can plausibly reproduce the spring speedup observed on Haut Glacier d'Arolla.

The paper is well-written and introduces a novel and potentially useful approach for representing dominant features of subglacial hydrology in a large-scale modeling con-

text. The method is described well (with a few exceptions, below), and the example application to Haut Glacier d'Arolla is, on the whole, convincing. My primary concerns are 1) that a few important details of the implementation of the growth of the efficient drainage system appear to be missing, as is consideration of a potentially significant flaw related to scale-dependence; 2) a general discussion of limitations of the hydrology model is missing. Treating both of these issues would substantially improve the paper, particularly because its approach is different than most existing work on subglacial hydrology. In addition to these major concerns, I have listed additional issues below.

—Primary Specific Comments—

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.

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 hydrologic system can become more sensitive to inputs later in the season is contrary to most modeling approaches and observations. On a related note, presumably the intention of formulating a simplified approach to subglacial hydrology is to facilitate modeling 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.

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 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 hydrologic system had begun to evolve for the season.

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.

In a modeling 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?

—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.

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').

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)

L10: Similarly, here Schoof (2010) is not entirely appropriate here as there is no ice 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)

L23: While these references are accurate here, more general references to include would be: (Fountain & Walder, 1998; Schoof, 2010).

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)

L12: Another study demonstrating the similarity of karst and glacial hydrology is (Gulley et al., 2012)

p3453 L19-21. This sentence is awkward and confusing. Consider rewording or breaking into two.

L23: Schoof et al. (2012) does not consider channels.

L22: It may be more general to just say 'lower spatial resolution' rather than 'lower bedrock topography resolution'.

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 to implement implicit time-stepping is what reduces computational cost? If so, please elaborate to make this clear.

p3454 L19: Awkward sentence here with 'represents' used twice. Consider rewording.

p3457 L2: The description is confusing. Please elaborate what is meant by 'surface Qj'. Does this mean inputs of surface meltwater to the bed?

p3458 L16: What is mean by 'specificities' here?

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.

p3459 L22: This sentence would be clarified to say 'due to the zero flux boundary condition on the EPL.'

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.

L24: For clarity, end this sentence with 'all over the active EPL domain.'

L29: By closing, is it mean shutting off entirely? Presumably he can decrease.

p3460 L17: Does the term 'sediment layer' here equivalent to 'IDS'? If so, I would recommend using 'IDS' for consistency.

p3462 L12: Perhaps 'resolution' should be spelled 're-solution'.

p3464 L18: Please state which hydrologic component is used to calculate N and why.

L25: The formulation leads to a decrease in basal drag for low sliding velocities, not high.

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.

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.

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

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 hydrologic systems, while the transmissivity represents the efficiency within the EPL itself.

p3470 last line: Please elaborate what is meant by 'fully developed EPL'.

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.

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 modeled surface speed is due to sliding? Are there any constraints for Haut Glacier d'Arolla on the amount of sliding occurring during winter?

p3474 L20: Change 'hypotheses' to 'assumptions'.

Table 2: Why aren't all parameters classified as pkp or wkp?

Table 3: Consider adjusting the caption to differentiate this table from the previous, e.g. 'Values of the tunable hydrological parameters. . .'

Figure 2: The black text on the dark gray shading is difficult to read.

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.

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.

Figure 9: The gray line here is difficult to see.

Figure 12: Just a suggestion, but including a plot of he here would help demonstrate the speedup feature around day 230.

Figure 14: Include a note of which year is modeled.

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Interactive comment on The Cryosphere Discuss., 7, 3449, 2013.