

Editor report Yang et al. tc-2019-255

Dear Authors,

Thank you for your considered response to the detailed reviews. I find the paper much improved, and believe that you have sufficiently addressed the concerns of the subglacial modellers by slightly changing the focus and toning down the interpretation of the subglacial modelling results.

For example, I think this paragraph effectively informs the reader on the validity of the modelling results, and demonstrates that it should be taken as a conceptual experiment rather than a representation of real-world conditions: 'As such, our results should not necessarily be extrapolated to infer large-scale or seasonal evolution of the subglacial hydrologic system in response to different surface forcings; however, the results do provide insight into the potential diurnal sensitivity of the subglacial system to changes in supraglacial meltwater routing and the associated modification of the discharge hydrograph. For example, the amplitude of diurnal effective pressure variation for a particular day may range from < 0.5 MPa to > 3.0 MPa in this experimental setup, depending on the surface method used (Figure 3). We hope this will serve as inspiration for future work to pursue broader-scope simulations to thoroughly investigate the larger- and longer-scale effects of surface input variations on effective pressure and ice dynamics.'

However, your results do raise some interesting questions about modelling practices that deserve wider attention. This statement is the crux of the paper for me: 'Many subglacial hydrology models commonly invoke a numerical term (the "englacial void ratio") to represent englacial storage in order to provide short term storage and release of meltwater that cannot be accommodated rapidly within the subglacial system, in the absence of more realistic representation of supraglacial and englacial storage (Hewitt, 2013; 10 Werder et al., 2013; Hoffman et al., 2016). In SHAKTI, this englacial void ratio is also included as an option (Sommers et al., 2018), but our simulations considered here do not employ this term in the equations. Our results present that the supraglacial hydrologic system acts as short-term storage for surface-derived meltwater, as exhibited by the time lag of moulin inputs between models; therefore, application of an appropriate surface meltwater routing scheme may reduce the dependence of some subglacial models on a somewhat arbitrary englacial storage term to produce realistic diurnal effective pressure variations and timing lags (Werder et al., 2013; Hoffman et al., 2016).'

I therefore believe that the paper should be published, since this is an important result likely of interest to many in the subglacial modelling community as well as the wider glacier hydrology readership.

Before publication, I have a few small final amendments to request:

Abstract: I find this a little over-long now and wonder if you could make it slightly more concise. I also wonder if you would care to include 'Our results present that the supraglacial hydrologic system acts as short-term storage for surface-derived meltwater' as a conclusion to the subglacial modelling section? This for me is the most exciting part of your paper!

P2, L18: In common with Reviewer 1, I also dislike 'very long-term' since it is subjective. Instead, just use 'longer-term'.

P8, L29: 'As with all subglacial hydrology modelling efforts, some of these parameters are highly uncertain and poorly constrained'

I dislike this statement. I would suggest rewording to be specific to *your* model setup, rather than casting aspersions at all subglacial models. For example: 'Certain parameters are highly uncertain (for example, bed bump height and bed bump spacing), so we select values used in earlier work with SHAKTI (Sommers et al., 2018) and in the Subglacial Hydrology Model Inter-comparison Project (de Fleurian et al., 2018).'

P16, L23: 'As with other models, the specific effective pressures presented here are admittedly influenced by uncertain modelling parameters and boundary conditions, and should be viewed as a comparison between methods, not as a representation of real behaviour at the bed in these catchments.'

I suggest removing 'as with other models', but otherwise this statement is very useful.

P17, L6: 'significantly small peak' – do you mean 'smaller'?

Thank you for your contribution to the Cryosphere, and your full engagement with the review process.

Best regards,

Elizabeth Bagshaw