Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-272-AC3, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Bridging glacier and river catchment scales: an efficient representation of glacier dynamics in a hydrological model" *by* Michel Wortmann et al.

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Received and published: 7 October 2016

Thank you for the review and constructive comments. We have responded point by point below with your comments in italic.

I enjoyed working through this manuscript. The authors have introduced lots of new ideas and sensible approaches to this kind of modeling. I agree that they are addressing an important gap between hydrological and glaciological models, at the catchment scale, and this effort is a genuine bridge between those worlds. There are new ideas and the model is applied in two interesting and well-selected locations. The paper is generally well-written, with strong conclusions that are well-supported by the model results. I think this work will find a receptive audience and it is likely that others will

build on the model presented here. For these reasons, I recommend publication, with a number of minor points and perhaps one or two larger concerns to be addressed.

Main concerns:

1. I have one specific concern about the model, which might just need more explanation or might suggest a 'push-back' towards more substantial revisions. On p.5, l.17 and Eq. (1), the critical snow thickness to be ice does not make sense to me. Hc. For typical slopes, like 10 degrees, tan n alpha 0.1 and Hc = 100 m. Does this mean that snow that survives the summer does not turn into ice unless there is at least 100 m of snow/ice accumulated? What about the snow thermal and albedo properties, etc., those should match ice after one year. Also, ice that is thinner than this creeps and slides. It does not wait until it reaches a critical shear stress before it starts moving. That is a misinterpretation of τ_s, which is more a 'balance' value where steady-state fluxes allow an equilibrium ice thickness. If I understand correctly, this seems odd an arbitrary for a glacier not to exist, thermodynamically and mechanically, until this much ice has accumulated. It would preclude many of today's present glaciers (and parts of them), which are thinner than this.

In the model we assume that the snow pack is turned into glacial ice if the snowpack exceeds the critical height. All snow processes are governed by the snow module of SWIM (based on the snow module by Gelfan et al., 2004), which describes the share of ice and water in the snowpack as well. The critical height is used to determine at what point the snowpack is subject to creep and basal slip according to Cuffey and Paterson (2010) and Marshall et al. (2012). While shear stress certainly deviates from the global mean of 10^5Pa, it is the best estimate in catchments without better estimates.

I also did not understand Eq. 3 or the area treatment on the next page, this could be explained more clearly I think. As I read it, Hc is maintained while area decreases in the lowest elevation band? I like this general idea, it is a nice new idea, but it seems unrealistic to maintain a steep and non-thinning layer of glacier ice while the area re-

treats. Shouldn't both decline at once, following a realistic volume-area relationship or what one would expect for a 'wedge-shaped' terminus?

Thanks! We have taken on your idea of a wedge-shaped front rather than a steep one as initially used. The length l of the wedge is proportional to the glacier thickness of the unit, and the melt area is proportional to the hypotenuse of the wedge (section 2.4 was extended). As before, the width is assumed to be the square-root of the unit area.

$$l = \sqrt{A_u} \cdot \frac{H}{H_c} \tag{1}$$

The melt area A_m $[km^2]$ is thus:

$$A_m = \sqrt{A_u} \cdot \sqrt{l^2 + H_c^2} \tag{2}$$

2. As the title of my review suggests, I did struggle with whether this model is sufficiently physically-based and state-of-the-art to actually be useful. That sounds harsh, perhaps, but there are more complex and realistic models out there (many of them cited by the authors), and the model proposed here has several free parameters of an empirical nature, e.g., degree day factors, which are not actual physical variables. These are tuned to observed discharge in the specific basins, and results are reasonable, but how portable are they in space and time? The authors do nicely balance complexity with pragmatism, with a relatively simple treatment of a lot of the processes, but perhaps appropriate for the large-scale objectives. I do agree with the authors that forcing data are not commonly available for more complex models (e.g., energy balance melt models, or more detailed glacier processes such as sliding). I just have concerns that this model is heavily parameterized and tuned in ways that are not representing the actual physics, which make it unclear how broadly useful it is. For instance, no attention is paid to conservation of energy or mass at the catchment scale; precipitation and mass balance are scaled as necessary, and melt rates are turned up or down ad hoc in order to match discharge observations. In the final lines, the authors note an intention

to use this modeling approach in future projections, but is it reasonable to take climate model precipitation and energy fields and manipulate them in this way, paying no heed to basic conservation? I have concerns that this is too far from constrained reality. Still, this model presents an initial step into coupled glacio-hydrological modeling that has yet to be done well at large scales. For this reason I don't think my concern here is fatal.

We understand the concern that the process descriptions may not be state-of-the-art and rely on several free and empirical parameters. The model's scope, however, are catchments with only the most basic observations used for calibration (discharge, glacier outlines) and very limited, error-prone driving data (temperature, precipitation), as they are found in abundance in High Asia. The sampling density of precipitation is of particular concern; many catchments don't even have a single gauge station. More physically based approaches (which may be described as state-of-the-art) necessarily rely on better driving data and have therefore only been successfully applied to small domains. One can only close the catchment-wide mass and energy balance if their input is known, which is decidedly not the case here (precipitation is mostly vastly underestimated, incoming shortwave radiation is extrapolated over several 100km at best). Our glacier-hydrology integrated approach relies therefore on discharge, glacier extent and sensible catchment-wide, annual mass (liquid and solid) balance values (snow accumulation, sublimation, evapotranspiration, groundwater yield). We have added these water balance values to the results section 4.4 to support the validity of the parametrisation.

Some specific questions on this point: How do the final parameter sets vary for the different catchments/sub-catchments? Are there generally sensible, robust, and repeatable parameter values, that you would feel comfortable to use in other environments or in future projections?

The parameter ranges are selected to the best available knowledge of the respective catchment ensuring that both water and ice balance terms are reasonable. We have listed the resultant min., median and max. values of our best 25 parameter sets for each catchment in the supplementary material. The parameter ranges are comparable but are of course always subject to calibration when implemented in a different catchment.

Is the glacier melt model the same as that for the SWIM snow model? I did not realize that degree day melt models were still in broad use, scientifically. The range of melt factors explored here (Table 1) looks like values that are common literature values used for snow and ice melt, without incorporating the effects of shading, aspect, debris cover, etc. I have trouble to imagine that this fully represents the range of potential values.

SWIM has a separate snow model (based on Gelfan et al., 2004) that also uses a degree-day approach but is also able to account for ice and water content as well as refreezing. We have attempted to alter (allowing both increase and decrease) the melt factors to commonly known effects such as terrain shading and debris cover in our model, see in sections 2.8–2.9.

Daily mean temperature is also simplistic when it comes to estimating PDD for snow and ice melt. e.g., a mean temperature of -1 C means no melt, although much of the day will be above 0 C. Minimum and maximum temperature are widely available and can be used to generate a daily temperature cycle - can this be considered?

This effect is accounted for by the melt temperature threshold that is commonly used in PDD models which also accounts for variations of the melt temperature due to latent heat fluxes (Hock, 2003). The model time step is daily, i.e. a diurnal temperature cycle cannot be accounted for.

In general the modeling approach emphasizes the model parsimony, that it does not need many input variables, and only things like temperature that are 'more known'. But it does need regional mass balances, across individual glaciers: both in balance, for the spinup, and maybe also in time, for the model calibration - I was less sure here. Are these widely known, or known well enough to be able to use this model in much

of the world? It seems like this is 'higher order' knowledge then some of the basic meteorological variables you would need for an energy balance model.

We agree that the regional mass balances are indeed a higher order knowledge of a catchment, but they only need be known for longer periods and not at the same time step as the model runs at. The emergence of satellite based geodetic mass balance studies has increased the coverage of such data dramatically over the past decade or so and the model can make good use of such new data. A possible alternatives are reference glaciers that have annual mass balance records. The World Glacier Monitoring Service (WGMS) has a rich database for both remote sensing based and in-situ observations.

Back to degree day factors.. I don't understand why these are based on length of day. Why not actually calculate potential shortwave radiation as a function of latitude, day of year, slope, aspect and shading? This is a more direct and realistic way to include this effect, and can be pre-processed easily. Do the length of day calculations include slope and aspect effects, and shading?

Potential shortwave radiation is indeed an alternative to length of day (the terrain analysis algorithm used here in GRASS actually outputs this as well), yet it does require another empirical parameter to scale the radiation to melt factors while being highly correlated with potential hours of sun. We aimed to avoid this by sticking to the proven positive degree-day approach but scaling it by the day length or potential duration of sunshine the glacier unit receives also taking account of aspect and shading effects.

And some minor points:

Thank you for pointing out those minor points to improve understanding. They were all integrated and some answered in more detail below.

p.1, l.20, "strongly heterogeneous processes like glacier dynamics" - is that really true? glacier dynamics work roughly the same way everywhere. But ice thickness and slope *vary strongly in space.*

This was changed to 'spatially strongly heterogeneous water balance components'.

p.3, l.30, "is described"

p.5, l.3, there is some jargon throughout, like 'cleaning area' - please define. Also 'hydrotope', referenced below on this page. How does a hydrotope compare with an HRU, or are they equivalent?

Cleaning was replaced by aggregation and yes, hydrotope and HRU are equivalent, which has been indicated in this sentence.

p.5, l.12, 30 cm of soil cover actually seems like a lot for steep alpine terrain, which is more likely bare rock. Am I mid-understanding here?

Added: All hillslopes are given the land cover category bare soil, i.e. the soil cover is treated as loosely consolidated, unvegetated soil that describes the loose gravel and small fluvial vans.

p.7, Eq. 10. I like this in general, the approach to separate melt and sublimation. But note that in fact M in field-based PDD calibration studies includes sublimation, i.e. it is actually observed ablation, M+S. But generally S « M where PDD factors are being calibrated, so this is maybe OK. This is not true everywhere though.

Added: Degree-Day factors observed by ablation stakes include sublimation. They should therefore be compared to simulated ablation and not used for the ice melt term without calibration.

p.9, debris discussion. I was not sure that these are the main processes involved. Also consider landslide/rock avalanche debris? Also aerosol deposition, which can be a blanket or it can also be concentrated by ablation and streams, e.g. where streams intersect crevasses. These processes should at least be noted, as they can be locally or regionally important.

Thanks! These processes were further mentioned in Methods/Debris section as well as the Discussion.

p.12, l.28, "complemented". Next line down, "its" Done

p.13, l.19, implemented

Figure 4, outlet of the Rhone. It looks like there are diurnal cycles here - is this really daily discharge, or is it hourly? If the former, what are the oscillations?

Added to Figure 4 caption: 'The oscillations in the Port du Scex discharge are the effects of Sunday dam closures.' As mentioned in the catchment description (section 3.2), they Sundays were excluded from the observations to reduce the effect on calibration results.

p.15, l.13, comparison "is" shown

p.16, l.8, principal

p.16, l.9, I don't think one year can have a climate. The year's weather?

p.16, l.14, "varies"

p.18, l.16, I don't think it is wise to say that it covers "all major glacier processes". As the authors point out, things like glacier sliding, surging, and calving are not included, and these could be considered major processes. Also seasonal albedo evolution and many other details of glacier ablation.

Changed to: It covers most glacier processes relevant to simulating catchment discharge including [...]

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-272/hess-2016-272-AC3-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-272, 2016.