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Reply on CC1

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Author comment on "Modeling glacial and fluvial landform evolution at large scales using a stream-power approach" by Stefan Hergarten, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-1-AC2, 2021

Dear Flavien Beaud,

thanks for your comments! I hoped that some more researchers from the community of modeling glacial erosion would share their opinion, and that they would perhaps even go beyond keywords. I am convinced that your expertise as a reviewer when considering the proposed concept seriously would have been helpful to improve the paper.

Let me briefly comment on some of your points.

For example, in Section 2, l. 63-64, the derivation of the glacial stream power starts with the statement "If we consider a rectangular cross section [...]". Yet, we know glacial valleys are not rectangular and there is no explanation of that simplification.

This is just some kind of entry gate for the readers. Those who think this way should stop reading at this point and not waste their time.
I hope there will be some readers not arguing on this level, but are able to think about the consequences of this simplification, although
I am aware that not all will be able to solve the exercise of finding out for which shapes of cross sections it remains valid.

Another point of concern is the recurring citation of Prasicek et al. (2020) to justify numerous simplifications. In the Prasicek et al. (2020) study, the goal is to assess timescales of respective processes: glacial erosion, climate and tectonic; not to reproduce landforms. The assumptions Prasicek et al. (2020) are making to assess the relative effectiveness of processes over time, become inadequate when applied to a model that aims at reproducing landforms themselves.

Sorry, I am not such an expert on this since I only developed parts of the theoretical framework of the Prasicek et al. (2020) paper. So far I thought it was about longitudinal equilibrium profiles for a dendritic topology and not about thime scales. But maybe I just missed the key point of that paper.

The paper cited to support such scaling (Bahr, 1997) refers to glaciers themselves, not glacial landscapes, and is therefore not applicable to glacier erosion.

I thought it was about length-width scaling of glaciers and used nothing else from this paper. And in contrast to previous studies, I was honest enough to explain that transferring scaling relations from entire glaciers into individual glaciers is nontrivial.

In term of the specifics of glacial erosion, even assuming a simple relationship between erosion and sliding velocity to a power, this power is likely > 1.

I think I mentioned this, and anyone should feel free to run the model with an exponent > 1. Some researchers may even prefer this for the fluvial regime since there were some studies that suggested exponents > 1 (although I think there are many pitfalls and artifacts there).

Using a shallow ice approximation has be shown to not work very well on steep landscapes (Egholm et al., 2011). These simplifications and their implications for landscape evolution results should be explained.

This is not just a problem of steep topographies, but applies to all combinations of shallow-water and shallow-ice equations with incision-type erosion laws if lateral stresses are not taken into account. Iti s explained briefly in the paper, and in principle it is an argument in favor of approaches where erosion is driven by a linear element.

Model of glacio-fluvial incision (l. 229-232): "While Beaud et al. (2016) developed a more elaborate model for the incision by meltwater within narrow channels, the meltwater component should preferably not introduce a level of complexity much beyond the simple models of fluvial and glacial erosion used here. So let us assume that erosion by meltwater can be described by the same formalism as fluvial erosion." That statement is in opposition to the results presented in the Beaud et al. (2016) study. Since that model is the only to date to describe such mechanism, dismissing the results or making different assumptions should be substantiated.

I cited this paper because it is apparently the first modeling approach towards bedrock incision by meltwater channels. However, I was neither able to recognize any fundamentally new theoretical concepts nor any results that could be generalized to larger scales. So it is not clear to me where the contradiction is. Did you find that meltwater incision in total independent of the meltwater discharge or find that the pressure gradient is totally decoupled from the gradient of the ice surface? Maybe I missed the key point again.

In summary, in its current form, I do not believe that the current study makes a convincing point that a stream power glacial erosion rule can, or should be used.

But unfortunately, other authors recently proposed a glacial stream-power law that is very similar to my version (Deal & Prasicek, Geophys. Res. Lett, 2020). They were definitely faster than me. Feel free to write a comment to their paper!

I hope you find my comments constructive and am available to answer further questions if necessary.

To be honest, not really. But I definitely appreciate your contribution to the discussion.

Best,
Stefan Hergarten