

Earth Surf. Dynam. Discuss., referee comment RC2  
<https://doi.org/10.5194/esurf-2021-1-RC2>, 2021  
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## Comment on esurf-2021-1

Marc Jaffrey (Referee)

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Referee 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-RC2>, 2021

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I recommend rejecting the paper.

In short the author appears to forcing glacial erosion into the framework of fluvial erosion with little or outright incorrect reasoning.

Main Reasons:

- The author does not appear to be familiar with nor understand sufficiently glacier dynamics and glacier erosion for which an extensive body of literature exists. The theoretical underpinnings of the paper are unfounded and in several key places in direct conflict with known glacier dynamics.
- The author has not sufficiently reviewed the literature.
- The paper is not written to address the glaciology community which I presume is a key target audience for the work.

Some key examples of problems highlight above:

- Section 10: "In contrast to fluvial erosion, however, glacial erosion has not been extensively considered in modeling large-scale landform evolution." There is an extensive body of literature on modeling large-scale glacial landform development.
- 35-40: "A comparable representation of glacial erosion where the erosion rate can be directly computed from properties of the topography is not yet available." This is not true and stems from the authors misunderstanding of the relationship between glacier dynamics and existing glacier erosion laws implemented within the glacial geomorphology community.
- 45-50: There are many different form of erosion laws considered by the glacier community of which the author does not acknowledge.
- 50: If the author is talking about the general mathematical concept of mass balance they need to say so. Mass balance in the glaciology community is something different and the connection to diffusion would require further explanation on the authors part.

- **60-65:** This is false. The author makes a critical assumptions here defining the model presented which is a critical misunderstanding of glacier dynamics and the Shallow Ice-sheet approximation. The ratio of the depth averaged velocity and sliding velocity is categorically not controlled by ice thickness, not even a second order control. Sliding velocity is a complex issue which from a dynamic's perspective, the principal first order controls are effective water pressure, bed temperature, bed roughness and a host of other bed parameters and processes. From a mathematical and numerical simulation perspective sliding velocity is treated independently with a sliding law, a Robin type boundary condition, which is typical defined in terms of the aforementioned bed processes, not ice thickness. The author seems to be unaware of glacier dynamics. Since the assumptions in 60-65 are crucial to the theoretical underpinning of the model the rest of the work, regardless of the care taken with the numerical simulation, is unfounded.
- 100: "The erodibility K is a lumped parameter that already includes precipitation " This statement requires a detailed explanation on the author's parts of what they mean by this statement.
- 100: This whole section is problematic. The author seems to be connecting concepts without justifications.
- **225:** "These findings support the idea that erosion by meltwater must play an important role, at least in the lower part of glaciers where the flux of water is much higher than the ice flux." One should always be careful when drawing dynamical conclusions from a numerical model especially when parameter tuning can be used to generate a wide range of results. That is to say, the author is making a circular argument and in this case one that is in conflict with the glacier community. Correlation is not causation.
- 20-25: References to convexity a concept from fluvial erosion is not one familiar to the glacier community and requires explanation.