Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., author comment AC1
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Reply on RC1

Stefan Hergarten

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-AC1, 2021

Dear Eric Deal,

thanks a lot for your thorough and even inspiring review! I must apologize that I was indeed not aware of your latest paper in GRL. The first part of the theory was developed in a slightly different way, but is in fact basically the same. And you were definitely earlier, although I spent a lot of time on the numerics before submitting my manuscript.

There is only one point where I clearly disagree with your opinion. This is the way erosion models of the stream-power type are formulated for non-constant precipitation. Probably owing to the fundamental studies of Hack, erodibilities have been expressed in terms of catchment size instead of discharge until now. If we want to keep this convention, there is no way of avoiding the definition of a reference precipitation p_0 and imagining that a given erodibility refers to p_0 . Then we can replace the catchment size A by either q/p_0 where q is the discharge or even by pA/p_0 , but where p is the mean discharge over the upstream catchment. Both versions become increasingly cumbersome when proceeding to the shared stream-power model and also for the fluvio-glacial version. Beyond this, I do not really like the concept where a mean upstream precipitation occurs in the erosion model and a local precipitation in the climatic component. Therefore, I do not like the version used by the Tübingen/Potsdam groups for some years, which also applies to the version with the product IA in your 2012 paper. My version of defining the ratio q/p_0 as the catchment-size equivalent of the actual discharge provides a clear definition that keeps the equations similar to the original stream-power model. I am convinced that reminding the reader of this definition at some occurrences of A is sufficient to avoid confusion. So there is no realistic chance to convince me here.

There is another point where I am not sure at all. In your final comment, you suggest to consider the version where the thickness is parameterized in terms of flux and slope instead of flux alone. However, my results already show that such a parameterization leads to a weird scaling of thickness vs. width in a steady state. While width is proportional to $q^{0.3}$ then, thickness is proportional to $q^{0.7}$. As far as I can see, your recent approach including deformation softens this problem, but thickness still increases more rapidly with flux than width. I guess that the problem already comes in when parameterizing the width by the flux alone without taking into account the slope. If so, it already affects the 2020 EPSL paper by Günther Prasicek where both of us were coauthors, but unfortunately I did not think about this at that time. So it would also be interesting to look at thickness vs. width in your recent results. But for my concept, this mainly tells me that we must consider all approximations as a package and compare it, e.g., to simulations with iSOSIA.

Best regards,
Stefan