Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., author comment AC4
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Reply on RC4

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-AC4, 2021

Dear Marc Jaffrey,

thanks for the more specific version of the review and in particular for the personal email you sent to me. This makes your point about some of the numerical models used on this field more clear to me. I am fully with you that it is a serious problem of science that things become "true" just by publishing and citing repeatedly.

Nevertheless, it seems indispensable for me to state that your arguments about the equations are completely wrong in my opinion.

- Concerning Eq. (4), you state: "*Equation 4 is incorrect: See chapter 7, eq's 7.6, 7.10, 7.17, 8.35, 8.36, 8.65 and sections 8.1, 8.4, 8.5 and 8.6 in Cuffey, Kurt M., and William Stanley Bryce Paterson. The physics of glaciers. Academic Press, 2010. Sliding velocity cannot be reduced to ice thickness and slope under any approximation. As Cuffey and Paterson in discussion the Shallow Ice Approximation say in chapter 8, `The rate of basal slip must be specified directly or through a relation to bed stress such as Eq. 8.25*'"

  Equation 4 just requires $v_s \sim \tau^\Psi/\sigma$ (Budd et al., J. Glacial.,23, 157-170, 1979) where τ = shear stress and σ = effective normal stress. According to the shallow ice approximation, $\tau \sim hS$ (h = ice thickness, S = slope of the ice surface). In this form, it yields $v_s \sim h^\Psi/(h-p)\, S^\Psi$ where p = water pressure. Among the references you provided, at least Harbor, Hallet & Raymond, Nature 333, 347-349 (1988) and Tomkin, Geomorphology 103, 180-188 (2009) assumed $p \sim h$, which exactly yields Eq. 4, $v_s \sim h^{\Psi-1}S^\Psi$. You may question the assumption that the fluid pressure at the bed is proportional to the ice thickness and claim that coupling with a distinct model for melting and for the flow of melt water may yield better results, and that such models are available. However, your statement "*Sliding velocity cannot be reduced to ice thickness and slope under any approximation*" is wrong.

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About the type of the differential equation: "*Ice thickness is not a diffusion process. See section 8.5.5 and equation 8.65 , 8.70, 8.77, 8.78 and 8.79 in Cuffey and Paterson. In the Shallow Ice Approximation ice flux q~h not the partial derivative of h wrt to x. There is a divergence relationship, first order partial derivatives, but diffusion is second order in the spatial partial derivatives. Without substantial justification, ice thickness cannot be treated as diffusion with strong diffusivity.*"

Sorry, but the shallow water equations and its derivates (shallow ice equations and Savage-Hutter equations for granular flow) assume a free surface and hydrostatic pressure in vertical direction. Then the horizontal force is proportional to the gradient of the free surface and not to the gradient of the bed (as you presumably assume in your reasoning). As a consequence, the velocity in the shallow ice equation also depends on h and on the gradient of the ice surface. Then the divergence term from the mass balance yields second-order spatial derivatives of h in total. The resulting spatial differential operator is elliptic, so that the entire time-dependent equation is a parabolic equation (= diffusion type for the broader readership).

Best regards,
Stefan