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Comment on egusphere-2022-352

Eric Deal (Referee)

Referee comment on "Modeling large□scale landform evolution with a stream power law for glacial erosion (OpenLEM v37): benchmarking experiments against a more process-based description of ice flow (iSOSIA v3.4.3)" by Moritz Liebl et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-352-RC2, 2022

In this paper, Liebl et al. compare the behaviour of the relatively well understood and physically motivated glacial erosion model iSOSIA to a new, simpler model of glacial erosion. The advantages of the simple model are that it can feasibly be run on landscape evolution timescales. The big question is then whether or not the simple model is good enough to capture the essential dynamics of glacially driven landscape evolution or not. Comparing their simple model, OpenLEM to iSOSIA is the natural step to answer this question. Overall, the authors find that many aspects of glacial landscape evolution are well replicated by the simpler model, with some important differences and caveats. They finish by using OpenLEM to run a longterm landscape evolution model complete with glacial erosion and sediment transport.

Overall the question the authors are addressing is an important one. There is a dearth of models for glacial erosion over landscape evolution timescales that are simple enough to be paired with modern fluvial erosion models. Benchmarking a proposed simple model of glacial erosion against one whose behaviour is better known and more trusted is a good step towards addressing this problem. The work is novel, and the authors demonstrate a good awareness of the state of the art. The paper is well written and logically structured; They have taken time to choose logical numerical experiments and describe them clearly. Overall, I find that the authors are aware of the shortcomings of their approach and discuss them fairly in depth. Finally, the figures are generally very well made.

However, the paper is fairly long. Part of that is due to the inherent nature of a benchmarking exercise, still I urge the authors to consider two steps to shorten the paper. First, there is a slight tendency to describe the results of each experiment in detail. Perhaps some more summarizing of the results and key conclusions of each experiment would be helpful to shorten the paper throughout. Second I suggest the authors consider removing the final experiment of running a longterm model with sediment transport from the paper. It is exciting, and very interesting, but has little to do with the benchmarking. It requires the introduction of several new model components, and does not help to answer the questions posed by the authors initially. I encourage the authors to submit this work separately.

The description of how the model works is at times hard to follow. I think that a few figures/diagrams illustrating how everything comes together would be incredibly helpful. In particular in section 2.2, the description of just how flow lines come together, how Ai and ice formation rates, erosion rates and total ice flux volumes are calculated across the channel width could perhaps be aided by a diagram showing this. Also, section 4.2, a diagram describing how the climate model works in the iSOSIA model would be helpful. Finally, I have one significant issue with the paper as it is, that I would like to see addressed before publication, discussed below.

I have an issue with the treatment of channel width in the implementation of OpenLEM. Recovering channel width from a model of glacial flow and erosion requires resolving the physics of ice flow at the sub-channel scale. You should be able to model the stresses within the ice, solving for cross-channel stresses induced by lateral velocity gradients across the channel as well as lateral gradients in the ice surface elevation across the channel. As the authors themselves point out repeatedly, the SPIM does not do this for rivers, instead it assumes a constant, equilibrium channel width that is instantaneously carved by the water at all times. This makes sense, over the timescales of landscape evolution, rivers probably achieve an equilibrium hydraulic geometry effectively instantaneously. In addition, the scale of the channel is usually smaller than the resolution of the landscape, so river width is considered a sub-grid process and can be kind of forgotten about. As the authors also discuss, this is harder to do with glaciers. They have widths that are often larger than the resolution of the landscape grid. So I appreciate that this is a challenging issue that has to be addressed. However, from my perspective, what OpenLEM does is sort of pretend that the simple glacier erosion model can capture the physics of ice flow within a channel, and then try to model the evolution of a channel width over time. To me this simply doesn't work. iSOSIA is designed to handle exactly this sort of problem, but OpenLEM is not.

OpenLEM already makes effectively all the same assumptions as the SPIM, why not assume that U shaped profiles are achieved immediately? Over landscape evolution it likely doesn't matter that U-shaped valleys take time to form. Most importantly, I don't think it has been shown that it is really a problem to treat glacier width as a sub-grid process. It might slightly alter the flow paths, but the tributary glaciers are all going to still flow into the trunk channel. The landscape will not visually look like a glacial one, which I think what might be driving the authors to develop the ad-hoc width treatment. But the parameters that matter at the landscape scale, such as valley elevation profiles, erosion rates, eroded volume, overall orogen height, etc. should be fine. Perhaps the authors can demonstrate that allowing glacial width to be sub-grid doesn't work. This would be an interesting and valuable contribution, and would add a lot to the paper. Even in this case, I think there must be a better, more physically informed approach rather than modelling in-channel ice flow with OpenLEM, which is what the authors effectively do. At the end of the day, OpenLEM is simply not the tool to try to establish the evolution timescale or morphology of glacial channels, as the authors almost seem to be doing in this paper at times. As the authors say themselves: "we must keep in mind that OpenLEM is not a model for simulating ice patterns on a surface, but landform evolution."

Detailed comments

Line 70 - Can the authors say how Hergarten 2021 was different than Deal and Prasicek 2021?

Line 167 - I am not a fan of Ai. I think it makes the erosion law seem more similar to the stream power model and easier to handle than it actually is. The ice flux Qi only reflects topography in a complex way, incorporating topography itself, but also climate and the properties of the ice flow itself. This is in contrast to the rather direct relationship between topography and catchment area for rivers. If I'm not mistaken, Ai even decreases downstream at times below the ELA. I think it would be better to use something like Qi to make this more clear.

Line 182 - Is this the steepest descent of ice surface or bedrock surface?

Line 183 - What is meant by catchment size equivalents? Does this mean a volumetric flux dh/dt*w = pi*w - div(pi*A) or something like this? How do the units work out? I think it would be helpful to write out equation 2 again in the catchment-size equivalents form, it is not quite clear to me how this is working.

Figure 5 - Why does the linear climate model lead to ice profiles that seem so ragged compared to the nonlinear models?

Figure 6b - I'm wondering about the ice in the three lowest elevation tributaries. Why is there ice filling these valleys, but no ice coming from upstream? Is it just flow up valley from the main trunk channel? What is it about OpenLEM that allows this but not iSOSIA?