Authors’ Response to Reviews of

"Modelling 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) "

Geoscientific Model Development,

RC: ***Reviewers’ Comment***, AR: Authors’ Response, ☐ Manuscript Text

1. Response to the editor

Dear Andrew Wickert,

We resubmit a revised version of our manuscript “Modelling 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)” to consider for publication in Geoscientific Model Development. First, we want to thank the two reviewers for their constructive reviews. We appreciate their effort, which helped us to strongly improve our manuscript. We addressed most of the raised issues and revised our manuscript according to the reviewers’ suggestions. In all other points, we have given reasons why we have a different opinion. Both reviewers considered our manuscript as a nice contribution to Geoscientific Model Development and we are confident that the revised version of this manuscript meets the high-quality standards of this journal. Before going into the details of the point by point response, we would like to emphasise the main modifications of the revised manuscript.

- We have added an additional figure to help the reader understand how the two models work. The figure shows the differences between the two models along a small section of the glacier to illustrate the direction of ice flux and how the flow lines converge.
- We have restructured the result section describing the benchmark scenarios. We believe that the inclusion of subsections makes the experiments more understandable and improves the reading experience.
- We have shortened and summarized the results of the benchmark experiments so that the advantages and disadvantages of each model are more clearly presented. This includes merging Figures 4 and 5 to make the comparison of the mass balance models more understandable to the reader. We also removed Figure 11 and the section on the valley floor evolution because the description was largely a repetition of earlier observations and provided little new information. In total, the manuscript is shortened by five pages.

The main goal of these modifications was to clarify the purpose of the benchmark study, which is to help future users understand which model is better suited for which application. Thank you very much for the editorial handling.

Best regards, Moritz Liebl on behalf of all co-Authors.

2. Review#1: Leif S. Anderson

AR: Dear Leif S. Anderson,

we thank you for your constructive and positive review. The review does not question our methods, results or interpretation, and we have followed the reviewer's advice throughout. In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments have been copied into this document and marked with RC. You can find the corresponding response on the right side. Information on the lines numbers refer to the track changed version.

RC: ***This is an interesting manuscript with a that compares a recently developed empirical stream power law for glacier erosion with erosion patterns estimated from a process-based model of glacial erosion. Overall I find that the manuscript is well written and that the methods of comparing the models are sound as well. It is exciting that a more efficient method for simulating glacier erosion in 2D is being developed!***

We are glad you support the idea of a new effective glacier erosion model. We appreciate that you find our method and results robust and detailed. In the following, we address the comments to mostly minor shortcomings of the manuscript line by line.

2.1. General comment #1: Length of the manuscript

RC: ***[G1] I found that the manuscript was a bit long, so any effort to contract the sections would be helpful. Shortening and synthesizing the results of the experiments would be welcome from my point of view as I found myself trying to find the benefits and negatives of each model but struggled to ID them. I note there is Section 7 which outlines Scope, limitations, and perspectives, but shortening before this would really improve the experience of the reader.***

To make the purpose of our benchmarking study more clear we added at the beginning of the "Bechmarking scenarios" section:

> Our work aims to assist potential users in selecting the ideal model for the specific issue they are trying to solve. Therefore, we are performing a series of experiments that ensure balanced benchmarking between OpenLEM and iSOSIA.

In addition, the results of the benchmark experiments have been condensed and summarized to make the advantages and disadvantages of each model more apparent. To give the most important examples here: Figures 4 and 5 have been combined to help the reader understand the scaling of mass balance models. We also removed Figure 11 together with the paragraph on the evolution of the valley floor, because the description was basically a repetition of prior observations and offered little new information.

2.2. General comment #2: Additional citations

RC: ***[G2] In some areas statements should have citations to support statements of fact. See line-by-line comments below.***

We included citations regarding the sensitivity of glacial landscape evolution to changing mass balances (Pedersen and Egholm, 2013; Seguinot et al., 2018; Lai and Anders, 2021; Magrani et al., 2022) (line 220) and the influence of the contribution of sliding and internal deformation to total ice flow on erosion rates (Deal and Prasicek, 2021; Prasicek et al., 2020; Hergarten, 2021) (lines 177-178). For more details see specific comments.

2.3. General comment #3: Including internal deformation

RC: ***[G3] As a more technical note: It is not clear to me why including internal deformation in an erosion model would matter. Basal sliding is the only way that glaciers physically erode the bed. Motion at the bed due to internal deformation by definition is zero. For that reason I suggest that suggestions throughout that link internal deformation to basal erosion be updated.***

We would like to refer to the paragraph in lines 174-183 in which it is now explained in more detail why the inclusion of internal deformation in an erosion model could be important. There it is also explained why the original implementation of OpenLEM (Hergarten, 2021) did not consider internal deformation. For more details see specific comment [S9].

2.4. Line-by-line comments

RC: ***[S1] Line 21 – " suggest 'addressed' here instead of 'attacked'.***

Thanks, done.

RC: ***[S2] Line 41 – drop 'the'***

Thanks, done.

RC: ***[S3] Line 70 – In this paragraph I am a bit confused about what Hergarten does differently from Deal and Prasicek and how that related the stream power law for rivers and new laws for glaciers.***

We specify this later in the description of the OpenLEM model in lines 174-176.

> In contrast to iSOSIA and to the glacial stream-power law proposed by Deal and Prasicek (2021), the version implemented in OpenLEM assumes that the entire ice flux arises from sliding along the bed and neglects the contribution of deformation, which becomes increasingly relevant with increasing ice thickness (Cuffey and Paterson, 2010).

RC: ***[S4] Line 97 – drop 'that'***

Thanks, done.

RC: ***[S5] Line 98 – drop 'that'***

Thanks, now we split the sentence here.

RC: ***[S6] Line 104 – drop 'in order'***

Thanks, done.

RC: ***[S7] Line 127 – add 's' after 'equation'***

Thanks, done.

RC: ***[S8] Line 134 – Only sliding contributes to basal erosion by glaciers. Motion via internal deformation goes to zero at the bed. So I am not sure about this portion of the sentence: 'and even dominates glacial erosion.'***

See general comment G3. Changed the sentence to:

> For temperate glaciers, however, sliding of the ice along the bed contributes much to the total velocity and even *Rev1:* ~~dominates~~controls glacial erosion.

RC: ***[S9] Line 176 – I don't understand how neglecting internal deformation limits the applicability to thick glaciers since internal deformation does not erode glacier beds.***

The contribution of ice deformation to the total ice flux increases as the glacier becomes thicker. As OpenLEM assumes that the total ice flux is composed of sliding, which drives erosion, OpenLEM overestimates erosion at a given ice flux compared to other models including deformation.

We added the following sentence in lines 176-177:

> At a given ice flux, neglecting deformation causes an overestimation of the sliding velocity and thus of the erosion rate (Prasicek et al., 2020; Deal and Prasicek, 2021; Hergarten, 2021).

RC: ***[S10] Line 250 – This paragraph could use more citations.***

We have deleted this paragraph because it is a repetition of the paragraph around line 220. There we have included citations about the sensitivity of glacial landscape evolution to changing mass balances (Pedersen and Egholm, 2013; Seguinot et al., 2018; Lai and Anders, 2021; Magrani et al., 2022).

RC: ***[S11] Line 338 – A few more citations here would be good.***

See general comment G2, where the same matter is already discusses in lines 174-176. Inserted the following citations now in line 351:

> Since deformation becomes more relevant with increasing thickness (e.g., Cuffey and Paterson, 2010), the erosion rates are overestimated at low elevations, where the trunk glacier becomes thick.

RC: ***[S12] Line 588 – there is an extra ')' here.***

Thanks, deleted the extra ')'.

3. Review#2: Eric Deal

AR: We thank Eric Deal for thoroughly reviewing our manuscript and for providing constructive comments. Almost all raised issues have been implemented in a revised version. In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments have been copied into this document and marked with RC. You can find the corresponding answer either below and marked with AR, or on the right side of the page. Information on the lines numbers refer to the track changed version.

RC: ***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.***

AR: We are glad that you support the idea of a new effective glacier erosion model. We appreciate that you find our methods and results robust and logically structured. We are pleased that you feel we provide a good overview of the state of the art and also sufficiently discuss the shortcomings of our approach. However, we believe that your main criticisms (especially in the second half of your review) relate more to the theoretical description of one model (OpenLEM), and not to the results of the benchmark experiments. We would like to emphasize that this study is not about further development of either model, but only about comparison of published existing codes. We hope that this point is now more thoroughly clarified in our revised version. In the following, we address the raised shortcomings of the manuscript line by line.

3.1. General comment #1: Length of the manuscript

RC: ***[G1] 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.***

This was also raised by the first reviewer. The results of the benchmark experiments have been condensed and summarized to make the advantages and disadvantages of each model more apparent. In the following we give the main two examples here. Figures 4 and 5 have been combined to help the reader better understand the comparison of the mass balance models. We also removed Figure 11 together with the paragraph on the evolution of the valley floor, because the description was basically a repetition of prior observations and offered little new information. In total, the manuscript is now shortened by five pages.

3.2. General comment #2: Adding figures/diagrams

RC: ***[G2.1] 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.***

To better explain how the two models function, we have added a figure after line 183, which compares the direction of ice flux and how the flow lines converge along a small segment of the glacier.

RC: ***[G2.2]Also, section 4.2, a diagram describing how the climate model works in the iSOSIA model would be helpful.***

We refer the reader to the new Figure 5a, where the mass balance rate is plotted against altitude. There it can be clearly seen that the mass balance of iSOSIA is nonlinear below the ELA and becomes increasingly negative with increasing distance from the ELA. We believe that a closer look at the mass balance model would only detract from the benchmark experiments.

3.3. General comment #3: The treatment of channel width in the implementation of OpenLEM

RC: ***[G3] 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."

AR: We find that this criticism relates more to the theoretical description of a model (OpenLEM), and not to the results of the benchmark experiments. We agree that the treatment of OpenLEM in terms of the evolution of glacial valley geometry is a major shortcoming. We treat this limitation along with the ad-hoc assumptions as a starting point for describing the models. We benefit from having the developers of both models on the authoring team to perform meaningful benchmark experiments and evaluate limitations and deficiencies that can inform future code improvements. However, we feel that it is beyond the scope of a benchmark study to go into the basic implementations of the models and modify them. To make this clear, we will try to clarify the purpose of the benchmark study, which is to help future users understand which model is better suited for which application.

Together with the inclusion of the new Figure 1, we now explain more clearly why we use this ad hoc approach.

> In contrast to rivers, however, including glaciers as linear objects in a 2-D landform evolution model is not useful. While rivers are typically much narrower than the width of their valleys and also narrower than the grid spacing of the model, this is not the case for glaciers. As a consequence, treating a glacier as a single-pixel line would underestimate the eroded volume and thus the sediment flux arising from glacial erosion. Therefore, OpenLEM uses an ad hoc approach for expanding glacial erosion over a wider area, which is illustrated in Fig. 1.

3.4. Detailed comments

RC: ***[S1] Line 70 - Can the authors say how Hergarten 2021 was different than Deal and Prasicek 2021?***

We think it is better to address this point in the description of the models. We address this in lines 174-176.

> In contrast to iSOSIA and to the glacial stream-power law proposed by Deal and Prasicek (2021), the version implemented in OpenLEM assumes that the entire ice flux arises from sliding along the bed and neglects the contribution of deformation, which becomes increasingly relevant with increasing ice thickness (Cuffey and Paterson, 2010).

RC: ***[S2] 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.***

The catchment equivalent ice flux Ai is a differently scaled variable of exact the same property as Qi. Ai and Qi carry exactly the same information. In the development of OpenLEM, it was decided that the fluxes are expressed in terms of area equivalents. Ai may confuse the reader, but changing the variables to the original in a benchmark paper does not make sense. We added the following sentence in lines 172-173:

> Since A_i is still a measure of the ice flux and was only converted formally into an area, Eq. 7 is fully equivalent to the version proposed by Deal and Prasicek (2021).

RC: ***[S3] Line 182: Is this the steepest descent of ice surface or bedrock surface?***

We return to this point at equation 9. Before that, there have been no assumptions about ice thickness made so far. We therefore believe that a precise explanation would confuse the reader rather than contribute to understanding.

RC: ***[S4] 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.***

For more details see also comment [S2]. In lines 169-173 the term catchment size equivalent is explained.

RC: ***[S5] Figure 5 - Why does the linear climate model lead to ice profiles that seem so ragged compared to the nonlinear models?***

We have to admit that the terms linear and nonlinear are chosen unhappily here, as we use two different approaches to calculate the mass balance. We changed the term "linear" to "linear mixing model (Hergarten, 2021a)", "nonlinear" to "melting model" and "1.5*nonlinear" to "adjusted melting model". For a more detailed description see lines 221-236.

A simple linear approach with a cutoff,

$$p_\mathrm{i} = p \, \min\left(\frac{H_\mathrm{i} - H_\mathrm{e}}{H_\mathrm{f} - H_\mathrm{e}}, 1\right), \tag{1}$$

was used, where p is the total precipitation rate and p_i the rate of ice production ($p_\mathrm{i} > 0$) or melting ($p_\mathrm{i} < 0$). This rate is zero at the ELA H_e, while the entire precipitation is converted into ice above a given elevation H_f.

While this approximation is not fundamentally different from the approach used in iSOSIA above the ELA, melting below the ELA turned out to be more critical. The recent version of OpenLEM offers two options here. In the original version (Hergarten, 2021), Eq. (10) was simply continued to elevations $H_\mathrm{i} < H_\mathrm{e}$, i.e., to negative rates of ice production. However, making this approach compatible with the description of glaciers by a cardinal flow line requires allowing negative ice fluxes. Otherwise, melting would practically act only on the cardinal flow line instead of the entire surface of the glacier. The alternative concept allows for defining an arbitrary function for the melting rate along the cardinal flow line. In order to obtain the total volume of melting ice per time and glacier length, this rate is multiplied by the width of the glacier according to Eq. (8). Both concepts of describing the rate of melting will be tested against the model implemented in iSOSIA in Sect. 4.2.

RC: ***[S6] 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?***

Yes, the ice of the main valley fills up the tributaries. We added the following sentence in lines 446-453:

In the early phase (Fig. 6a,b), OpenLEM not only predicts a longer downstream extension of the main glacier, but also a stronger filling of tributary valleys with ice, although these valleys receive little ice from their upstream catchment. In reality, this effect occurs as soon as the ice surface in the trunk valley is higher than the valley floor of the tributaries. In iSOSIA, this upstream flux of ice is modelled dynamically (following the actual ice surface) and also reduces the ice flux of the trunk glacier. In turn, OpenLEM uses a steady-state mass balance and simply fills up the tributaries with a horizontal ice surface, so that the flux into tributary valleys is zero. While the effect on erosion in tributary valleys is small in both models, it makes a difference concerning the mass balance, which is partly responsible for the longer extension of the glacier in OpenLEM.

4. Modification of Figures

Figure 1: We have added a new figure to illustrate the different treatment of ice fluxes in the two models.

Figure 2: Included a red square which depicts the domain in Fig.1.

Figure 5: Merged the old figures 3 and 4 to one figure and deleted the evolution of ice covered area.

Figure 6: Deleted the squares, circles and triangles which indicate the position of data logger recording topographic changes over time as shown in old Fig. 11.

Figure 11: Removed the figure.

Figure 13: Changed the heading of each subplots.

Figure 14: Colored the the river profiles in the maps as shown in the long profiles.

References

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