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Review of Hergarten 2021 for esurf

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I would just start my review by stating that when I was asked to review "A stream-power law for glacial erosion and its implementation in large-scale landform-evolution models" by Stefan Hergarten, I told the editor that I was happy to do the review, and felt that I had the relevant expertise, but also a conflict of interest. Although I do not know him personally, Dr. Hergarten and myself are coauthors on a recent paper on a topic very similar to the one dealt with in this paper. Further, I have even more recently published a paper that has an overlapping focus to this one. However, although the topic matter between our two papers is very similar, the goal of each one is distinct, and therefore in my view the work is complementary. I informed the editor that I felt I was still able to maintain impartiality, though I wanted to be transparent about my relationship to Dr. Hergarten and his most recent work, and he agreed it was a minor conflict. The review that follows is unusual in that I discuss my own work extensively. I do so partly because it forms the basis of my understanding of the subject matter of this work. I also mention my work frequently because Dr. Hergarten had not evidently seen it before submitting this work, which is understandable due to how recent it is. Due to the overlap between the two papers, my most recent paper is often very relevant, particularly to the first half of this manuscript. I hope this is not interpreted as a plug for my own work - in the end Dr. Hergarten is free to decide how much he wishes to use. It is rather an effort to arrive at the best possible topography-based model for glacial erosion.

In “A stream-power law for glacial erosion and its implementation in large-scale landform-evolution models” Dr. Hergarten presents the derivation of a topography-based model of glacial erosion in the spirit of the classic stream power incision model (SPIM) for fluvial erosion. After establishing the similarities between the SPIM and his new model for glaciers he proceeds to outline and solve the differences in the glacial model that are responsible for numerical challenges that do not exist for landscape evolution models (LEMs) based on the SPIM. This leads to a 2D LEM with coupled glacial and fluvial erosion and sediment transport which makes more assumptions than existing glacial LEMs, but also runs dramatically faster. This is important in opening up the ability to explore the parameter space of the model in a way that would not be possible with the more sophisticated models of glacial erosion.

There has been recent activity in developing simplified models of glacial erosion and taking advantage of decades of work with the SPIM to understand in a more quantitative way than ever before the role that glaciers play as agents of landscape evolution. In this I am clearly biased - but I find the work in this manuscript to be topical, relevant and important given this recent activity. Some of the key problems standing in the way of a 2D model of glacial erosion - in particular the implementation of a channel width that is generally larger than the model grid spacing - have been brought up by Dr. Hergarten, and solutions have been implemented. I find this already a significant contribution, but Dr. Hergarten has gone further, and implemented several potentially important processes, including sediment transport by glaciers and subglacial fluvial erosion.

Though this manuscript is clearly novel, and constitutes an important contribution, I also have a few criticisms that would ideally be addressed before publication.

Dr. Hergarten is clearly very good with theoretical model development. However, one downside of that is that the theoretical development taking place in this paper far outstrips any empirical support. I don't find this in and of itself to be a critical problem - Dr. Hergarten has shown mathematically and then numerically how one would go about constructing a 2D LEM with glacial erosion. My criticism here rather lies with the packaging of the work. I think that there are many places where the limitations on the empirical understanding of the problems at hand should be made much more clear, and ideally discussed in more depth. Also the tone of the introduction could be modified to make it clear that this is more of a numerical implementation of yet to be developed - and tested - models of glacial erosion. In the same vein, I find there are many unsubstantiated comments made throughout the paper, which are based rather on an intuition for how glacial erosion works. I see where the intuition comes from, and I even often agree generally - but at the end of the day, even if these are my intutions, I would have to admit that I actually don't really know how the system 'should' behave. Intuition is important, particularly in model development. However, I think that it is important to maintain a standard of impartiality and try as far as possible to support intuition and intuition based statements with data and observations. That said, this work is rather new and there may often not be empirical data to cite - in this case however, there should be a higher level of transparancy about what behaviour has been observed and what is an educated guess. I have highlighted many of the places where I feel that more empirical support and/or circumspection would be valuable- but I also would encourage Dr. Hergarten to modify the tone throughout the paper to be more in line with an relatively untested theory of a physical process which we honestly don't understand very well yet. It may be a great way to identify where future empirical work could make the biggest impact for the development of good glacial erosion models.

At the end of the paper, I find that Dr. Hergarten has produced a convincing 2D model of glacial erosion that accounts for the most critical aspects of glaciers while retaining simplicity to keep the model fast. Future work may show us that some critical processes have been ignored, but I think that based on current knowledge, he's done a great job. However, there are several places throughout the paper that I disagree with the model development - particularly due to the fact that Dr. Hergarten temporarily makes assumptions that I don't think are appropriate, and does so without discussing how significant these assumptions are. My concern is that the critical elements that set glaciers apart from rivers are not appreciated. This seems detrimental to me for two reasons: readers will not appreciate the hardest aspects of the model development and where the focus of future work should be, and readers may think that any of the equations in the paper would be fair game for future work, when in fact I feel that only the 2D model is sufficiently complete to capture glacier erosion on landscape time and space scales. In particular, I think that the role of the ice surface slope versus channel slope and the ice accumulation rate (via some climate model with the ELA) need to be highlighted rather than somewhat implicitly being included in the 2D model. If Dr. Hergarten wishes to retain the current structure of the paper, then many of the equations shown - in particular 7-19 should be clearly described as intermediate steps which are not sufficient to describe glacial erosion until the assumptions of zero ice thickness and constant upstream ice production rate are removed in the 2D version of the model.

Finally, there are only a few empirical observations that can be currently used to test our theories of glacial erosion. One big one is U-shaped valleys. But the fact that valleys are parameterized in this model precludes this from being used to validate the model in any sense. One other observation that can be used is the observation and qualitative theory of the glacial buzzsaw - that very little terrain exists significantly above the ELA. There is also the associated, somewhat implicit conclusion that glacial terrain will have a different slope-uplift rate scaling. I urge Dr. Hergarten to take advantage of the speed of his new 2D model to compare more than just a few profiles of fluvial landscapes to glacial landscapes. It would be great to see a bit of an exploration of the parameter space - how does the overall channel slope or slope of the orogen change as a function of uplift rate or climate, and how is this different for purely fluvial landscapes compared to glacio-fluvial landscapes, or when sediment transport is turned on? Has Dr. Hergarten created a model that is fundamentally different that the SPIM, or do these landscapes look like fluvial landscapes with wider channels?

Overall I think this is a great piece of work that just needs some expansion, a bit more in depth explanation and a bit more polish. In line with my first main comment, there should be better citation of the literature to support comments made throughout the paper. It would be good to see a bit more careful handling of the assumptions made, and some more in depth analysis at the end of the paper.

Eric Deal

Detailed comments

Line 13: 'The difference in mathematical and numerical complexity may be the main reason for this imbalance' - This could be supported by some citations. Also I'm not sure I totally agree with it. When it comes to erosion, both processes are difficult to observe directly. It seems to me that one of the biggest reasons for the discrepency may be that rivers are much more prevalent. This makes them more important to study for many reasons important to society - particularly around river engineering projects. The same logic would also apply for landscape evolution models, where rivers are, in a global sense, much more important than glaciers. This could lead to a clear focus on fluvial erosion as the dominant process, resulting in better models of fluvial erosion available to be integrated into landscape evolution models. I think it is worth keeping in mind that nearly 100 years separates the first mention of erosion power being proportional to slope and discharge and the first time the stream power incision model was integrated into a landscape evolution model.

Line 15: While hack definitely mentioned transport capacity proportional to slope and area, wasn't this more in terms of gravel-bedded alluvial rivers? I feel like the standard first reference of Howard 1994 for the stream power incision model (SPIM) is used so because it was the first time that this was applied in the context of bedrock rivers, which would to me make it the correct reference for LEMs.

Line 19: I think you mean 'Lumped parameter K'

Line 21: 'more ore' > 'more or'

Line 21: Can you expand on the idea of universal a bit more? I'm not sure I understand what you mean by that. If, perhaps, you mean that they have universal values, I'm not sure that I agree with that. I know that the ratio of m to n is often observed to be within a narrow range, but the value of n in natural systems is highly variable, with observations commonly ranging from 2/3 to > 4. In any case, I feel like in addition to explaining this a bit more clearly, the statement needs to be supported with citations.

Line 26: This is an abrupt transition to 'fully implicit schemes'. Perhaps a new paragraph, as well as more context? I can guess you mean LEMs, but this is not very clear from the text. Why is implicit important? You state a few reasons, but the advantages of implicit over any alternatives is not clear to the uninitiated, and any drawbacks associated with implicit are not discussed. Given that there is a focus on retaining the ability to model this equation implicitly throughout the rest of the paper, I think it might be good to have a short discussion on implicit versus explicit schemes before moving on.

Line 37: There is some recent work on just this topic. Myself and Günther Prasicek recently published a paper which shows how a model of glacial erosion that is very comparable to the stream power incision model can be formulated where the erosion rate can be computed directly from topography and an ELA that plays the role equivalent to P in the SPIM. The analytic steady-state solution depends on an approximation of local ice surface slope as the average ice surface slope over the glacier (this doesn't really impact the steady state solutions however). But just to point out that it is possible, we have also written (but not published) a numerical solver without this last approximation, just using the upstream ice flux and then solving for local ice flux and ice surface slope which is stable and order n, though not implicit.
Deal, Eric, and Günther Prasicek. "The Sliding Ice Incision Model: A new approach to understanding glacial landscape evolution." Geophysical Research Letters: e2020GL089263.

Line 61: It is a bit more fair to say that the equation governing the relationship between ice flux, q and ice thickness h, is a 5th order polynomial with two terms, h^3 and h^5, and the coefficients in front of them (given the standard values from the literature - such as those taken from Prasicek et al, 2020) determine that the transition from sliding dominated to deformation dominated flux occurs at a thickness of a few hundred meters. This is right in the middle of the range expected in alpine glaciers, and makes it difficult to justify a sliding only approximation (or a deformation dominated approximation, unfortunately). I'm not saying that sliding only is a bad place to start, but I don't think that it is useful to discuss the ratio of vd to vs. That makes it seem like sliding only is an ok approximation. I think it's better to be clear about the fact that it is not a good approximation, but perhaps a mathematical necessity.

Line 66: I'm a bit confused about this concept of 'eliminating' h. I agree that there are two equations, and if h is the only relevant unknown, it can be considered redundant info, and does not have to appear in the equation. But this doesn't mean h doesn't exist. It can be calculated at any time using equation 4 or 7. Of course, it is clear to me that you know that. However, my confusion comes in later when it is discussed that this is the case with no thickness, and you will explicitly consider h from that point on. Perhaps I have really misunderstood the argument here - but I would argue that eliminating h from the relationship between vs and qi does not mean it is not explicitly accounted for, or that it can be considered 0. Instead I would argue that the place where you make h=0 is when you consider the relevant slope in the problem to be the bedrock slope instead of the channel slope (and perhaps to a lesser extent where you consider the elevation of precipitation to be the bedrock elevation instead of the bedrock plus ice thickness elevation). I think this distinction is important because it is the difference between the ice surface slope and the bedrock slope which is a big part of what makes this problem hard to solve from topography compared to the SPIM, and it is also what makes the behaviour of the model different and interesting. I think it would be more informative and beneficial to the readers to make it clear that this is the crux of the problem, and that this is where you are making the key approximation for this first model. I would even encourage you to label the slope more explicitly, so it is clear when the equations are referring to bedrock slope and when they are referring to ice surface slope.

Line 70: I would argue, that since you have already implicitly stated that the goal is to recover a model of glacial erosion from topography (Line 37 and again in this line), that the task here is not just to describe w, but also to describe q in terms of A and some climate model like P does for the SPIM. This is the other hard part of the problem compared to the SPIM and should be mentioned here.

Line 75: I'm a little uncertain what a glacier polygon is - does a single polygon correspond to a full glacier - could this be expanded on a tiny bit?

Line 76: I appreciate the careful point made here about characteristic width versus actual width! However, I think that the conclusion 'it makes sense to follow the concept…' sort of undoes the care taken directly before it. Perhaps better would be something like "the proposition of Prasicek is not at odds with the observation of Bahr, though it cannot be concluded solely on the observations of Bahr. In the absence of any model that is better supported by data, and in order to stay in line with standard fluvial models, we use the model of prasicek." - this makes it clear that the model for channel width remains at this point unsupported by direct empirical observations (beyond those in the prasicek paper itself, which while they represent hard work, are, to be honest, not constraining any models of channel width).

Line 86: I think that there is an h in the equation where it should be eta. Also, not a big deal but I would maybe suggest a symbol for the width exponent that is not chi - that has taken on a pretty specific meaning in this field.

Line 87: Assuming a constant rate of ice production over the entire upstream catchment is a very significant assumption. I would argue that perhaps the defining characteristic of a glacier versus a river, at the landscape scale, is the fact that q is not proportional to A but in fact a convolution of a climate and the topography. This is technically also true in the fluvial world, but that turns out to be a second order detail. We know, or at least strongly suspect, that this is a first order feature of glaciers. Why else do glaciars have ELAs, termini, terminal moraines and long skinny lakes far below the snowline left strewn about the landscape as a reminder of their past extents. I actually don't think that this has a huge impact on equation 12/13, but the statement adds to the confusion that comes later with using Ai. I feel that it is still possible to hide all the complicated interaction between climate and topography in Ai - but this is not really made clear in the text. The use of A in Ai is also a bit confusing, because it makes it seem like it could be as simple as A ~ x^(1/eta), especially when combined with the statement of assuming a constant rate of ice production. It's not really clear for how long the assumption of constant ice production holds throughout the text. Does it also apply to equation 17, in which case Ai really is similar to A. However, in this case the most interesting and perhaps important feature of glaciers is missed - that they melt. I want to say, I really appreciate where this work ends up, and I find the numerical model development to be important work. I also understand why you structured the paper the way you did. However, I strongly disagree with the way that this assumption, alongside the not-really-mentioned approximation that bedrock slope S can be used in place of ice surface slope are introduced without much discussion or justification. The path to equations 18/19 is made to seem simple, even inevitable. However, this is only the case when these two assumptions can be made, and these two assumptions are not really fair to our understanding of the workings of glaciers. It is my concern that readers may think that equations

17/18/19 would constitute a functional model of glacial erosion. This would only be true if there are in fact a lot of physics hidden in Ai - but the need for this is not made clear when the equations are introduced. I urge you to remove this statement entirely. You could, for example, take a path similar to the one that we took. This was simply to point out that models of channel width for glaciers are poorly constrained, and it anyways makes more physical sense that the channel width is proportional to ice flux rather than contributing area - this is because for glaciers there is no connection between contributing area and ice flux due to melting. Therefore, we simply propose equation 13 as an alternative to equation 12 and move forward with that. There is no need to invoke the fairly limiting assumption that there is a constant rate of ice production over the entire upstream catchment.

Line 93: This is a cool result to be sure! If we apply the sliding only approximation to our equations (gamma = 1) then we recover the same values for the exponents as you have here, which is also cool, though we always consider S to be ice surface slope. We found that this approximation lead to fairly substantial misfit between the steady state morphology of a glacio-fluvial profile when compared to the model presented by Prasicek 2020, which has fewer approximations than either topography based model of glacial erosion (yours or ours). However, we use an approximation for mixed sliding and deformation that does not increase the mathematical complexity of the model, yet leads to almost no misfit at steady state between the more complete model of Prasicek 2020 and our topography based model of glacial erosion. We stronly encourage you to use it as it increases accuracy without increasing the difficulty of solving the model. One caveat: we have carefully tested the accuracy of the various approximations at steady-state, but cannot attest to how well the accuracy holds during transient conditions. Very likely the approximate, topography based model exhibits some misfit during transience, but we would still expect the mixed sliding/deformation approx (gamma=2/3) to perform better than either sliding only or deformation only approximation.

line 97: Would be nice to have a citation supporting the statement about psi.

Line 99: Can you expand on what exactly this implies for the factor of proportionality in 14? I'm not sure I followed that very well.

Line 100: I agree, of course that this is the main difference between 2 and 14, but I don't agree with the reason. As you yourself have mentioned, there is a significant advantage to a topographic based model of erosion, and this is likely the real reason that 2 is written in terms of A and not Q. I think that this is an important distinction, because it had to be demonstrated for fluvial landscapes that $Q \sim A^p$, where p is close to 1. This jump will, unfortunately, not be so easy for glaciers.

equation 17: I understand the logic behind this equation, and obviously it's mathematically sound. However, I strongly feel that using a variable termed Ai is confusing and maybe a little misleading. It hides the complexity of the relationship between landscape and climate in a glacier network. A parameter 'po' can of course be defined, but what is the physical significance of it? In our work, we also had the same urge as you have here - to show the similarity between topography-based models of glacial and fluvial erosion. In the end we chose to define $q_i = IA$, which is analogous to the fluvial equation $q = PA$. The difference between this approach and 16/17 is that I is undeniably a function of position, where P can be considered a function of position, but can also be approximated as a constant - an approximation that has been tested and shown to be not terrible. The other difference is that A is A with no distinction between fluvial or glacial and therefore $q_i \sim Ix^{(1/\eta)}$. However, this means that the closest one can come to the SPIM is $E = K_g((I(x)A(x))^\theta S_{\text{ice surface}}(x))^\ell$. I would strongly encourage you to not use Ai, because it is confusing. Since Ai is effectively unknowable given the setup here, it is also not really a topography-based erosion model at this point anyways. I think you should either stop at qi, and point out that the SPIM can and is often written this way, or adopt an approach similar to the one that we did in Deal and Prasicek 2020.

Equation 19: I think at some point between equation 4 and 19 it needs to be clearly and visibly pointed out that the ice surface slope has been exchanged for the channel slope. The thickness of glaciers is within an order of magnitude of the height of the topography, and therefore the slope of the ice surface and the slope of the channel do not have to match at all - it is even standard for them to have opposite signs as the terminus of the glacier is approached: 1.Alley, R. B., Lawson, D. E., Larson, G. J., Evenson, E. B. & Baker, G. S. Stabilizing feedbacks in glacier-bed erosion. Nature 424, 758–760 (2003).

Equations 20 and 21: I strongly disagree with the use of the symbols A and Ai here. I do get the equations - the math is solid, it all checks out. There is nothing fundamentally wrong with the math here. However, I feel like these equations result from a little mathematical gymnastics that hinder the intuitive understanding of the model. It seems to me what's being shown here is actually fundamentally q, normalized by a constant po. We could write 20 as

A = q / po, where q = s*p + sum_over_donors(q)).

The constant po does allow this to be called A, but particularly in the glacial case where po is effectively unknowable a priori, this is not a helpful equality. I think it would be easier to follow and more properly understood if it were referred to as q. One argument to back this up is figure 5: A cannot decrease downstream. That's not how a concentrative network can function. However, Q can and does decrease downstream in some real rivers and most real glaciers. The other argument is that A is a topographic parameter - it should be calculated from the geometry of the landscape with no concern for things such as precipitation rate. The need to include p, pi and po in equations 20 and 21 is a giveaway that they are not really calculating area.

Line 134: 'over an area around to the cardinal flow path" - wording is a bit awkward, can drop the 'to'.

Line 136: I think you mean equation 13

Line 137: Change 'prolonging' to 'extending'

Line 137: This is clearly a place where no standard has been has been proposed or accepted, so it is exciting to consider the possibilities. I feel that you dismiss these two cases perhaps too rapidly. A couple things come up for me - what makes them endmember cases? It's not clear to me on what spectrum? What variable is reaching an end value? Also what makes them unrealistic? It seems to me that with single timesteps of potentially hundreds to thousands of years converging immediately to U-shaped valleys is not necessarily immediate or unrealistic. This would be the analagous case to the SPIM, where the channel shape cannot evolve, but is immediately specified and whose erosion rate is constant across the entire channel. I understand that the situation is different here because channels are no longer subgrid, but still this seems to me one of the more reasonable choices. Two questions come up for me when thinking about how to handle channel width. First, even though channels are no longer subgrid, is there really enough spatial resolution in the channel that modeling nonuniform erosion rates across channels would be at all realistic? Second, is it worth the effort? What effect would nonuniform channel erosion or non U-shaped valleys have on the largescale evolution of the landscape? I can imagine that right as the channel network develops these dynamics could play a role, but after the network topology is mostly established (e.g. just after the first few timesteps - long before steady state) does it really impact ice routing or erosion rate at a scale greater than say a few channel widths?

Line 140: change ‘a prolongation of the’ to ‘extending the’

Line 142: Again, I am guessing the final handling of channel width is not critical for the behaviour of the model - however, I don’t agree that extending the ice flux Ai to the channel edges is the most realistic approach. If I have understood correctly, the parameter Ai is the volumetric flux of ice in the entire channel - the idea of extending it to the edge is for me a bit confusing. Technically there is only one Ai for the entire channel width at any give location - no extending needed. However, for me to make sense of it I have to go back to the original definition of the erosion rate - a function of the sliding velocity. Therefore the most realistic would be to extend to sliding velocity across the channel - either say that the sliding velocity is constant across the channel, or that there is a known gradient. You have constructed a relationship between volumetric flux and flow depth that then leads to a sliding velocity that was maybe implicitly meant to be the centerline - though this was never stated. Then perhaps in this world, the most realistic would be to recongnize that ice won’t really be able to sustain topograpy on its surface, and the elevation of the ice will be close to flat across the channel - then the model valley could be filled with ice until the value Ai is used up, and the erosion rates calculated from the ice surface slopes across the channel and the flow depth across the channel. However what I think all this messing about with across channel erosion rate is really saying is that we don’t have enough ice flow physics in this model to calculate across channel erosion rates. I feel that we have seen that the U-shaped valley is a sort of equilibrium channel form where the erosion rate can be constant across the channel. This is supported I would argue implicitly by the existence of U-shaped valleys in all glacial landscapes around the earth as well as some modeling studies (Leith, K., Moore, J. R., Amann, F. & Loew, S. Subglacial extensional fracture development and implications for Alpine Valley evolution. J Geophys Res Earth Surf 119, 62–81 (2014).). Accepting this channel form a priori as an equilibrium form similar to the way we accept the channel width-discharge relationship for the SPIM leads to the conclusion, for me, to acknowledge that modelling the evolution of channel width is beyond the capabilites of this model and that the best approach is to extend the erosion rate of the centerline to the channel edges.

Line 157: I feel that the usage of the value of 1 or 2 for most of the key parameters is strange. This plays the role of a nondimensionalization, but is much harder to follow. For example, K = 1 and U = 1 for n = 1, whereas the ‘standard’ values (K = 1e-6, U = 1e-3 for n = 1) give a U/K ratio of 1e3 rather than 1. Probably this doesn't matter, and the landscape can just be vertically scaled - but then I, as the reader, have to do all that thinking about it. I must try to interpret the model parameters in terms I am familiar with, and the whole time I am wondering if that dramatically different U/K ratio really doesn’t matter at all. I feel that this is mental effort more effectively conducted by the author, and I would really appreciate either a proper nondimensionalization, or simply the usage of more familiar parameter values.

Equation 22: what is A here? Is it just s + sum_over_doners(s) or is it equation 20? If it is equation 20, why does p come into Ai,eff?

Equation 22: I think you need to go into a fair bit more detail about how you arrived at this equation. I feel that this is an important point, but I don’t understand why A comes into the ice flux. This seems to me like it would cause the ice to not melt fast enough below the ELA because there would be a portion of the ice flux that is always going to increase downstream regardless of elevation. Also I can’t understand at all why the valley shape would change as a function of Ai,eff - can you provide some insight into that?

Line 174: I think you mean equation 13 again…

Line 208: Why is Kg 2K? Why is it larger, and why 2 times larger specifically? I feel that this statement implies conclusions that have already been made about glacial erosion being more erosive than rivers. Why not just leave Kg = 1 in the absence of any specific knowledge about Kg? Otherwise one could argue that it should actually be 3, or 10 or 100… Alternatively, there are empirical estimates of Kg out there.

Figure 5: It should be specified clearly that x is distance from outlet of river.

Line 224: I think that the statement about the glacial profiles being steeper is fairly disingenious. It is contingent on choosing the right elevation as your reference point. In fact, across the whole profile the glaciers are actually less steep. Also much of the glacial profile is less steep, the only reason that they are steeper for given reference elevations is because of the steep steps in them. As you state yourself, the only reason there is a step is because of the ice dynamics at the terminus - so the second step in those profiles is actually a transient feature! Comparing transient fluvial profiles and concluding something about their erosion efficiency would be considered a fairly flawed analysis. Also to be fair, even the first step is partly a function of having only sliding and partly a function of not considering the ice surface slope. From what we've seen in our models, there is sometimes a step, and often no step - and when the sliding only condition is imposed, there is much more likely to be a step. Also there doesn't need to be a step at all because it is actually the ice surface slope that drives erosion rate, and while it is true that the ice surface slope goes to infinity, this results in a rapid thinning of the glacier, which often manifests in a channel slope with a sign opposite to the that of the ice surface slope - and therefore no step. Finally, while this step may be partly the result of unrealistic model conditions (that ice flux goes to zero) , this can be handled with a harmless solution like saying that the glacier ends when the ice is just a few meters thick, instead of waiting for it to go to zero. Similar to how the linear stream power law implies infinite elevation, but this is clearly not the case.

Line 226: Also, while I agree that requiring infinite ice surface slope at the glacial terminus is unrealistic, I am not convinced that the steps are unrealistic. I think this would be a good place to support this statement with empirical evidence. I would argue that steps / steep fronts under glaciers or at glacial termini are actually quite common - and how can we (yet) say where they come from?

Line 227: I also agree that erosion by meltwater is important - but you hardly need my agreement for that - there is plenty of evidence, outside of the existence/non-existence of steep fronts to support this conclusion. I think it would be good to cite that here.

Line 231: I like the idea of this, but can you do better than assuming? There is work on subglacial channel erosion, can some of it be cited here to show that it is at least reasonable to claim an equation of the form of 25?

equation 26: Seems like there is an i subscript missing here on the first A

Lines 230 - 245: I like the simplicity of these equations, but I think you need to be more open about the fact that at this point, equation 27 is wildly unconstrained. Can one even begin to put somewhat empirical values to these Ks? Either that or bring in some more empirical work on subglacial fluvial erosion.

Line 245: "Since there is no discontinuity in the erosion rate at the glacier terminus then, the changes in the flow pattern are much smaller than for the version without erosion by meltwater. The respective profiles depicted in Fig. 7 (dashed lines) reveal a smooth transition from the glacial regime to the fluvial regime". It is clear to me from the text that you find this to be preferable to the previous case with the steep front. I understand this intuition, but I feel obligated to ask why, precisely, is this better? There have been no observations brought out to show that this is really more realistic. How well studied are glacial channel profiles really? Can we really fairly say we already have an intuition for what they should look like? Keeping in mind also that the glacial channels we see today are far, far out of steady state due to us being in an interglacial period. I don't really feel that the section on subglacial fluvial erosion has to be removed or anything, but I do think that the validation of this process based on any attributes of the resulting profiles is potentially folly, and should be avoided. I think you should be more empirically motivated when assessing how realistic these profiles are. It would be sufficient for the development of the model to point out that subglacial fluvial erosion clearly happens - and make some citations, and then, while being open about how unconstrained the parameter values are (unless it is possible to constrain them given existing theory and empirical observations), impartially discuss the differences in the resulting profiles. Or perhaps it is possible to show that the value of Kf is relatively unimportant for the dynamics, that would also be nice.

Figure 7/8 - can you confirm that the value of Kf is the same as K somewhere in the text or figure caption?

Line 256: I think in order to make a statement like this, you need some observations to support it.

Line 262: "Let us assume that the identical relations for glacial and fluvial erosion do not only hold for the detachment-limited end-member, and that the shared stream-power model provides a reasonable description of both glacial erosion and erosion by meltwater. " - This strikes me as a huge assumption. At this point we have not talked about glacial sediment transport at all. I know that for rivers, both models of erosion and sediment transport are based on the fluid shear stress at the bottom of the river, and even without considering the decades that have gone into studying these proceses, there are good reasons to think that they would be related to some degree. Is this the case with glaciers? To be honest, I don't really know how glacial sediment transport works, so I can't comment on it - but it would be, I think, important to see some empirically based arguments, even if they are fairly hand-wavey, as to why we might believe that glacial transport would also be proportional to KA^(m+1)S^n.

Line 321: I'm not sure if this is a reasonable justification. It is akin to saying that if E = KA^mS^n would lead to no steady state when slope goes to zero, so elevation will grow out of control. The answer is not to remove the slope dependence of erosion rate, but to recognize the limits of the model, and that a purely detachment driven model with no explicit fluid dynamics will never find a slope that is zero at steady state.

Section 6: I think it would be important to see some sort of comparison between the non-slope dependent ice thickness you implement in the implicit versin of the model and the real ice thickness implemented in an explicit version of the model to show that the approximation is reasonable and recovers an ok answer most of the time.