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Interactive comment on "Efficacy of bedrock erosion by subglacial water flow" *by* F. Beaud et al.

F. Beaud et al.

fbeaud@sfu.ca

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We would like to thank Dr. Jansen, an anonymous reviewer and the editor Dr. Stroeven for their consideration of our manuscript. Below, is how we propose to address the comments and suggestions point-by-point. Our responses are in italic and the proposed changes to the text in bold. Note that in supplement you will find the manuscript with the proposed changes in bold.

1 Referee #1; Dr. Jansen

1.1 Specific comments

[852:25] For the benefit of those outside the bedrock channel community, it might be useful to outline the "tool and cover effect" more fully here; i.e. the modulation of bedrock erosion rate stemming from the balance between the supply of grains acting as erosional tools and the fraction of the bed exposed to impacts. Some explanation appears later (859:11-14), but something earlier and more explicit is preferable.

Corrected text on [852:25]:

[...] Abrasion is the result of particles entrained by the flow (saltating or in suspension) colliding with the bedrock and is governed by the tools and cover effect, whereby particles ***(i.e., tools)*** *entrained by the flow impact exposed bedrock* ***but can also shield it if they are immobile (i.e., cover)*** *[...]*

[853:5] Given that previous work (Beaud et al. 2014) shows that abrasion and quarrying processes reflect quite different governing roles for sliding versus hydraulic potential gradient, are some important dynamics being missed here by excluding quarrying from this analysis of meltwater erosion? It might be useful to discuss the ramifications of this omission.

Quarrying in fluvial systems is indeed governed by different dynamics than abrasion, it however requires a highly jointed bedrock or a bedrock with a weathered upper layer to be active at all (Chantanantavet and Parker, 2009; Whipple et al., 2013). Note that in the paper mentioned in the comment (Beaud et al., 2014) we treated abrasion and quarrying resulting from ice-bed contact and that in the present paper do not treat theses processes and focus on the action of water flow. We added the following text to the discussion at [875:12]:

In this study, we treat only the case of bedrock erosion by abrasion and we ne-

glect the effect of quarrying. Although the latter can lead to erosion rates up to an order of magnitude larger than abrasion, it requires that the bedrock be highly jointed (Whipple et al., 2000; 2013). Quarrying is a two-step process: (1) loosening of blocks around pre-existing cracks (or possibly opening of new cracks) and (2) mobilization and transport of loose blocks (Whipple et al., 2000, 2013; Chatanantavet and Parker, 2009; Dubinski and Wohl, 2013; Lamb et al., 2015). The depth of loose cracks could be related to sediment availability (Chatanantavet and Parker, 2009), and mobilization and transport of quarried blocks scale with the transport stage (Dunbinsky and Wohl, 2013; Lamb et al., 2015). Therefore we expect that the patterns of quarrying would be similar to the transport stage, yet limited by the thickness of the loosened layer.

[853:10-15] The enhanced erosional potential towards the ice margin is consistent with the scenario described by Jansen et al. (2014) for inner gorges in N Sweden where incision is also attributed to meltwater during rapid deglaciation. The Swedish gorges stood within 100 km of the ice margin for $\sim$100–170 y, and thus their dimensions (20–35 m deep, and $\sim$100 m wide) lend some plausibility to the maximum incision rates (50–200 mm/y) predicted by the incision model (okay, that's enough self-promotion!).

We are currently working on the follow up of the present paper in which we try to apply the SME model to timescales commensurate with glacial cycles. In the current paper the maximum erosion rates concur with the results of Jansen et al. (2014) although the R-channels modelled are only a few meters wide. Therefore in term of volume, the extrapolation of our results hint that it would take several thousand years to excavate such a feature (see [880:5-8]). In term of the location of erosion our results suggest that the idea of the erosion potential proposed by Dürst-Stucki et al. (2012) and Jansen et al. (2014) holds besides the fact that we show a decrease in erosion and transport stage very close to the ice margin. We would like to remain cautious in the present paper in terms of long term extrapolation and would rather keep such discussion for the follow-up paper. We added the citation of Jansen et al. (2014) in the text at [853:13]:

Dürst Stucki et al. (2012) calculate an erosional potential of sub- glacial water based on the hydraulic potential gradient under a valley glacier and find that the erosional potential increases toward the terminus and could explain the deepening of inner gorges during a glaciation ***(e.g., Jansen et al., 2014))****.*

[854:21] Greater subtlety concerning the question of inner gorge formation would be well placed here. Preservation of gorges through multiple glaciations, as advocated by Montgomery and Korup (2011), applies well to some localities but not others. For instance, postglacial inner gorge incision is demonstrated by McEwan et al. (2002, Arct. Antarct. Alp. Res.), and Schlunegger and Hinderer (2003, Terra N.), as well as the cited example of Valla et al. (2010).

We would argue that the interpretation of Montgomery and Korup (2011) is actually valid for the 3 three examples cited. The argument for the preservation of gorges through glaciations in Montgomery and Korup (2011) is two-fold: (1) fluvial incision rates required to carve a new gorge during interglacial times would need to be some of the highest we observe in the present and be sustained for the whole interglacial period; (2) glacial erosion rates necessary to erase a gorge during a glaciation would be unrealistic. More specifically, Montgomery and Korup (2011) review inner gorges across Switzerland including the sites studied in Schlunegger and Hinderer (2003). McEwen et al. (2012) record high post-glacial incision but state: "[...] the four gorges [2–4] clearly originated in pre-Holocene times according to our calculations and were probably initiated in a subglacial environment where lines of weaknesses in the bedrock could have been exploited by subglacial meltwater under pressure [...]" (McEwen et al., 2002, p.253). In a similar fashion Valla et al. (2010) show high post-glacial erosion rates, but their results only account for a few thousand years during which the gorge underwent such erosion. Therefore, despite the fact that all these studies report high post-glacial erosion rates, they do not necessarily explain the entirety of the gorges excavation and do not rule out the possibility that subglacial metlwater erosion may have played a role.

Changes made to the text on [854:21]: "The origin of inner gorges was originally attributed to postglacial fluvial erosion, although Montgomery and Korup (2011) conclude [...]" changed to "The origin of inner gorges was originally ***entirely*** *attributed to postglacial fluvial erosion, although Montgomery and Korup (2011) conclude [...]".*

[855:4-6] Genetic relations between these species of bedrock channel might be largely a proximal-distal issue: inner gorges reflecting the topographic confinement found in mountain areas whereas tunnel valleys seem restricted to open distal lowlands.

It is a possibility. In either case, the excavation would happen (in the absence of volcanism) close to the glacier / ice sheet margin, where channelized water flow could be expected, and therefore in a distal location when compared to the ice divide. In that sense we would argue that the topographic restrictions and the interplay with possible direct glacial action (tunnel valleys) or fluvial erosion during the interglacial periods (inner gorges) are the processes mainly responsible for this differentiation. It would therefore make sense if these bedrock channels are excavated during any stage of a glacial cycle as long as substantial water flow takes place such that subglacial channels are viable. We have not changed the text.

[857:1-2] Is this a fair assumption? Accommodating the growth of channel dimensions over time would seem to be an important part of a dynamic model.

We believe this is a fair assumption as the changes in bed topography as a function of time are much smaller than the changes in R-channel size. Furthermore, at the end of the season the maximum change in bed topography would be of the order of a decimetre while the maximum radius of an R-channel is, in our simulations, of the order of a few meters and can change within several days or a few weeks. The changes in hydraulic potential resulting remain insignificant given the ice geometry that we use. This process would however become important if the sediment transport was introduced in the model because the rate of sediment deposition or entrainment would be comparable to the rates of opening or closure of an R-channel.

[876:20-23] Egholm's iSOSIA might be ideal for this purpose.

This is a possibility, although we believe that there are still significant caveats that currently prevent the assemblage of such a comprehensive model of glacial erosion. For instance, we believe that such a model would need to be first implemented for short timescales and, for such timescales, there are some inconsistencies between the latest glacial quarrying law (Iverson, 2012) and the previous understanding of that process (see Beaud et al., 2014). A comprehensive model would also require a more detailed treatment of subglacial hydrology, i.e. the coupling of distributed and channelized drainage systems. To clarify that statement in the text we modified the following sentence: [876:20] "Another means of obtaining insight into sediment supply rates and patterns would be through the coupling of the present model with a model of glacial abrasion and quarrying, although patterns of glacial erosion remain only poorly understood (e.g., Beaud et al., 2014)." to: ***"Another means of obtaining insight into sediment supply rates and patterns would be through the use of a comprehensive model of glacial erosion, i.e. a model encompassing transient subglacial hydrology (between distributed and channelized systems), ice dynamics, glacial abrasion and quarrying. Such a model is however yet to be developed as patterns of glacial erosion remain poorly understood (cf. Beaud et al., 2014)."***

[878:8-13] This is an interesting speculation. Could this approach be usefully inverted to explore the origins of certain of over-deepenings?

In the future, and with a comprehensive model of glacial erosion this could be a possibility. The numerous feedbacks arising from the interaction between the different erosional processes and the transient character of an ice sheet make it hazardous to invert as is. For the inversion to be useful a model would need to have a coupled ice dynamics, subglacial hydrology, erosion encompassing the three main processes (glacial abrasion and quarrying and subglacial meltwater erosion) and sediment transport. Even in the presence of such a model however, we would advise for caution as the parameters inverted for can be non-unique, e.g. high erosion rates over a short *time could yield similar result as low erosion rates over longer times.*

[879:6-15] One point possibly worth considering is that, unless erased by fast sliding ice, inner gorges are typically deepened progressively over successive glaciations. The erosion rates cited here might produce metre-scale channels, but a bedrock slot deepened over several glaciations would presumably exert some important preconditioning on subglacial meltwater conveyance. Perhaps the effect of inherited bedrock slots could be incorporated into future modelling efforts.

This is a very interesting question. Please see answer to point 6. of the anonymous referee who raised a very similar point.

1.2 Technical points

[852:9] Perhaps stipulate what is meant by "equilibrium" here.

We changed the sentence to: "Water velocities are relatively high in the channelized system and, ***under steady state conditions****, water pressure decreases with increasing discharge".*

[853:4] Lamb and Fonstad (2010) documents a rather small canyon. For something big perhaps Baynes et al. (2015, PNAS) would be more appropriate?

This is a fair point. The studies by Bretz (1969) and Baynes et al. (2015) present canyons incised in columnar basalt resulting from several floods while Lamb and Fonstad (2010) show the erosive capability of a single flood in limestone. We added the Baynes et al. (2015) citation to line 4.

[855:13-14] This clear terminology is most welcome.

Thanks!

[859:10] This is incorrect; quarrying/plucking is by far the most widespread and efficient

mechanism of fluvial bedrock erosion (because densely-jointed bedrock is predominant in most landscapes). Whipple et al. (2000, p. 493) states this, along with numerous others.

To correct the statement we propose to clarify the sentence by specifying the joint spacing for which abrasion seem to be the dominant process according to Table 1 in Whipple et al. (2000), i.e. block size larger than a few meters. There are also many situations in which abrasion dominates (e.g., Sklar and Dietrich, 2004; Cook et al., 2013). We changed the text from "Abrasion is the primary erosional process in most river environments (Whipple et al., 2000, 2013)" to: ***"In river reaches where bedrock fracture density is low (i.e. blocks are larger than 1–2 metres in size), abrasion is the primary erosional process (Whipple et al., 2000; 2013)"****.*

[862:4] "Linear" with/to what? Neglecting downstream-fining is a major simplification. Is it possible to infer some effects based on the experiments in which grain size is changed?

The linearity refers to the exponent of 1 for the fraction q_s/q_{tc} for the cover effect, which means that every immobile particle will cover a new portion of the bed. However, particles can lay on top of other particles or particle–particle impacts can lead to a dynamic cover effect. Turowski et al. (2007) explore "cover" relationships for the SEM and find that the use of a linear cover term leads to an underestimation of erosion rates. To clarify that process, we changed the text as follows:

We assume that the bedrock has a uniform resistance to erosion, erosion occurs uniformly across the width of a channel, ***abrasion is the main erosion mechanism and that the cover effect is linear. A linear cover effect means that as long as the bed is partially exposed, newly deposited particles are assumed to cover bedrock rather than previously deposited sediment, hence the exponent of one on the "cover" fraction*** q_s/q_{tc} ***(cf. Turowski et al., 2007).*** *We also [...]*

As for the neglect of downstream fining, it is a significant assumption indeed. In the *scope of this paper, however, we believe that this simplification is justified to understand the already numerous feedbacks. For the purpose of the follow-up of this project, we performed some simulation with a simple downstream fining model (e.g., Sklar and Dietrich, 2006) while keeping the mean grain $D_{\text{mean}} = 60\,\text{mm}$. This simple simulation yields similar results as a decrease in relative sediment supply (decrease in q_s or increase in D). The downstream fining function is a power-law decay of the sediment size with distance, therefore, in the lower part of the glacier (below km 35) where most erosion takes place, the sediment size changes by a limited amount spatially and is smaller than the mean size. In terms of time integrated erosion the pattern remains very similar although less erosion occurs below km 35 and slightly more between km 25 and 35. We clarified this in the discussion by adding text at [876:14] first and then [876:20]*

[876:14] *"Accounting for the production of sediment* ***and the evolution of particle diameter*** *at the glacier bed would also largely influence sediment supply patterns."*

[876:20] *"If instead of fixing the sediment supply per unit width, we fix the total sediment supply (simulation not shown), tools are less available at peak flows, reducing erosion, whereas the cover effect is enhanced for a relatively small channel.* ***We also tested a simple power-law downstream fining function (e.g., Sklar and Dietrich, 2006; simulation not show). The results were very similar to those obtained with a decrease in relative sediment supply, because particles were smaller than the reference size of 60 mm in the region of the bed where channels form.*** *Another means [...]"*

[876:23-26] An ugly sentence, better rephrased.

We rephrased as follows:

*"****Finally, subglacial water flow evacuates a significant volume of sediment despite the small area over which R-channels operate and the tendency of these channels to remain stably positioned in association with moulins (Gulley et al,***

2012). The mechanism by which large volume of sediment are delivered to the channels remains elusive.*"*

[876:26] Change to 'subglacial'.

Done.

2 Referee #2; anonymous

1. The introduction was my favorite part of the paper - it was one of the best -organized and most thoughtful summaries of a motivation for the research I've read recently. I can't decide if I was hoping to hear more or less about other fluvial erosional mechanisms - we do know that quarrying can be very effective in fluvial systems at times, so I wonder if it is worth explaining this more explicitly.

We tried to limit the length of the description of quarrying in the introduction as the paper is already relatively long. Even though the efficiency of quarrying is well documented in some specific settings, the amount of literature on this process is relatively limited. We hence prefer to point the reader to the specific literature for more details.

1a. page 852 line 25: tool and cover effect - I like this phrase, but perhaps explain more for those outside the fluvial erosion community?

The first referee, Dr. Jansen, made a similar comment. Please see the first comment in the "Specific comments" section of the answer to Dr. Jansen's review.

2. page 859 line18/19: Vi is the volume of bed material removed upon impact, presumably in a uniform fashion across the bed? I guess i was hoping for a bit more detailed explanation of this series of equations (not necessary, but might be helpful in dragging those of us less familiar with the fluvial abrasion equations along).

The quantities introduced in equation (7) would indeed deserve a more detailed introduction. We added the following text at [859:19].

The impact rate (I_r) is a function of the number of particles and their saltation trajectories. The more particles and the shorter the saltation length, the higher the impact rate. The volume removed upon impact (V_i) is a function of the energy released on impact and therefore of the particle mass, its speed normal to the bed and the physical properties of the bed and the particle. The fraction exposed

(F_e) determines the areal extent of bedrock vulnerable to erosion and represents the cover effect. All the quantities described here are given per unit width.

3. page 861 line 24: any thoughts about testing other grain sizes?

We show some tests with different grain sizes in steady state in the supplementary material (Fig. S3). For the transient simulations, we perform simulations with different sediment supplies (q_s) and we argue that it is very similar to changing the particle diameter D as either quantity impacts the relative sediment supply (q_s/q_{tc}), the latter being one of the main controls on erosion patterns. We added the following sentence at [875:23].

The changes in erosion would be quantitatively similar to a decrease in sediment supply (q_s) and thus a decrease in relative sediment supply (q_s/q_{tc}), which strongly controls erosion patterns.

4. page 863 line 15: interesting and diverse range of simulations - I'd like to know more about how these were selected.

The sediment supply (q_s) was selected to highlight all the feedbacks we observed along one profile. For example in Fig. S2 E–F of the supplementary material, erosion is inhibited in the higher portion of the profile because the transport capacity is too low to expose the bed. Then around km 25 the erosion per unit width reaches its maximum and decreases downstream until about km 47 as the transport stage increases or remains relatively high (Fig. S2 C). These spatial changes permit us to show all the feedbacks that we describe later in the paper due to both changes in time and space. We added the following text at [863:16]: In the steady-state simulations we impose a sediment supply that leads to an interesting and diverse range of simulations ***illustrating most processes and their feedbacks,*** *whereas in the transient simulations [...]*

5. page 869 line 9: did you mean synthetic forcing of water Q?

We clarified that sentence as follows:

*The reference model uses a synthetic forcing**, in the form of water supply to the distributed system ($\dot{b}_{ca}$, Eq. (3);** sinusoid with a period of 120 days [...]*

6. page 879: I wonder a bit about the role of pre-existing bedrock channels in controlling water pressure and meltwater flow at glacier beds for later glaciations - perhaps speculating on this? your results imply quite a bit of channelization can occur during one glacial cycle, but is this a positive feedback process by which fluvial erosion continues to be locally enhanced during subsequent glaciations?

This is a good question. In terms of pre-existing bedrock channels, they would need to have a topography that is significant compared to that of the overriding ice to play a role in water routing. For example even in the case of a tunnel valley, if its depth is only a few tens of metres and it is overridden by several hundred metres of ice, there is a good chance that the position of moulins and ice topography would still control water routing (see Gulley et al., 2012). For an inner gorge however, because the valley walls are very pronounced, the water will most likely be routed following a similar path to that of the pre-existing thalweg. Furthermore, the position of moulins is highly controlled by the ice flow regime and the ice and basal topography, thus significant changes in ice geometry could lead to a reworking of the subglacial water flow paths. We are working on a follow-up paper in which we apply the model presented here to longer timescales, since we only focus on annual simulations here, we would prefer to reserve speculations on the effect of bedrock channels on water flow routing for then.

7. I'm not personally familiar with field examples of the inner gorges that are discussed, so I wonder if including a photograph as an example might actually be very useful for readers. Just a thought!

We believe that the best illustrations come from published work, so we prefer to refer the reader to the current literature (e.g., Montgomery and Korup, 2011; Durst-Stucki et al., 2013; Jansen et al., 2014) where they can find a detailed description of such

features.

3 Editor's comments; Dr. Stroeven

Here is how we propose to answer to the comments of the editor Dr. Stroeven.

1. Page 858, line 6: please name kex the "exchange coefficient" (cf. table 1).

Done.

2. Page 859, line 5: mention of Table 3 is out of order.

As the reference to Table 3 refers to one of the simulation that we only describe later on, we propose to remove the name of the simulation and the table reference. We changed the sentence to:

*"**Otherwise, for the simulation in which discharge is constant throughout the domain**, we apply a Neumann [...]"*

3. Page 861, lines 1-22: wouldn't it make sense to refer to Table 2 in this section 2.2.2?

We added the following sentence at [861:13]:

*"**Constants and parameters used for the TLEM are the same as in the SEM and are listed in Table 2.**"*

4. Page 861, line 17: insert "approach" (or equivalent?) before "0".

Done.

5. Page 862, lines 25-27: Please rearrange this sentence so it matches the order of appearance in the supplementary material (or vice versa).

Done.

6. Page 863, line 19: Explanations for STP and WDG first appear on page 866, lines 6-7.

To clarify the statement we changed the text at [863:10-23] from

"We use a flat bed and simple ice geometries: all geometries but STP and WDG (Fig. 1) are parabolas, as it is the equilibrium profile of an ice sheet. The STP geometry is an attempt to represent an advancing ice sheet with a steep front, while the WDG geometry more closely resembles the profile of a thinning and retreating ice sheet margin."
to:

We use a flat bed and a parabolic surface for all but two geometries: STP and WDG (Fig. 1). The STP geometry aims at reproducing the steep front of an advancing ice sheet, while the WDG geometry has a wedge shape that resembles the profile of a thinning and retreating ice sheet margin.

7. Page 866, line 27: I believe reference to Fig. 3c should be to Fig. 3d?

Done.

8. Page 867, line 7-8: please add "the" before "transport" (two occurrences).

Done.

9. Page 867, line 9: De-capitalize 3C to 3c.

Done.

10. Page 867, line 18: It is unclear to me why a value of qs is known to the second decimal place when 10-4 and not when 10-2?

This is a mistake. We changed the values so that they are given to the first decimal place.

11. Page 867, line 20: Is the reference to Fig. 3 correct here?

Done.

12. Page 868, line 6: Add reference to "(Fig. 4)" after TLEM?

Done.

13. Page 871, line 1: De-capitalize 6A to 6a.

Done.

14. Page 872, lines 4-7: The section starts with a conclusion from literature which seems in contradiction to the modelling results presented in the same section. I don't think the authors return to this issue later-on. It would be prudent to do this.

The results concerning the timing of erosion are discussed at [878:3:11], the text however lacks an explanation of how that relates to the statement at [872:4-7]. We added the following text at [878:13]:

[...] Scandinavian ice sheet. ***However, these findings challenge the hypothesis that glaciers deliver more sediment to proglacial areas during retreat than during advance (e.g., Hallet et al., 1996; Koppes and Hallet, 2002, 2006; Koppes and Montgomery, 2009), yet more work is required to explore this hypothesis.*** *The lack of [...]*

15. Page 872, lines 9-12: Repeat of "T_1300...". Remove its first mention, and add ?, Table 4? to the second instance on line 11.

Done.

16. Page 873, line 26: I think the authors want to use "cf.", (compare), and not "c.f." carried forward?

The text was corrected to "cf.". We also corrected the other occurrences of that mistake throughout the paper.

17. Page 877, line 10: Exchange "sheer" for "shear".

Done.

18. Page 879, line 5: Exchange "suggest" for "indicate".

Done.

19. Page 879, line 18: Exchange "mm in one year" for "$\mathrm{mm\,a^{-1}}$".

Done.

20. Page 879, line 21: Remove "s" from "channels".

Done.

21. Page 882, line 2: Perhaps refer to some more historical sources than merely "Burke et al., 2012"?.

We added a reference to Brennand (1994).

22. Page 892: Make sure the values and units are the same as in Table S1. For example, there is a difference for "numerical compressibility" which has units in the one but not the other.

The unit was added in Table 1.

23. Page 894: Add reference to "(Table 2)" at the very end of the Table header.

Done.

24. Page 895: Add reference to "(Table 2)" after the second sentence of the Table header.

Reference added after the first sentence and the mention of the sediment supply, it seemed more appropriate.

25. Page 896: Remove "IS" in front of "700" and "1300" in the figure legend.

The labelling "IS700" and "IS1300" is recurrent throughout the paper when referring to the ice profiles only. This is present in Fig. 1, its caption and in the text. We are not clear on that request to remove "IS" from the figure legend only. Please clarify the intention of the suggested change so that we can make edits accordingly.

26. Page 897: Panel B, add a "space" in-between "10^2" and "Pa" on the y-axis.

Done.

27. Page 897: Panel C, This should be "$\times 10^{-2}$" and not $10^{-2}\times$ (cf. Panel B)?

Done.

28. Page 897: Panel D, This should be "$\times 10^{4}$" and not $10^{4}\times$ (cf. Panel B)?

Done.

29. Page 900: Panel A, I wonder if light-grey in the panel and on the y-axis will be legible?

Please see answer to point 30.

30. Page 900: Panel C, left-hand y-axis should state "Erosion rate" and right-hand y-axis "Total Erosion". Move "@50 km" inside the panel.

The grey was darkened and the legend moved inside the panel. However, we would prefer to keep "Erosion" only as the left hand y-axis to keep the consistency with Fig. 6.

31. Page 901: Panels A, C, I wonder if light-grey in the panels and on the y-axes will be legible?

Done.

32. Page 901: Figure caption line 2, add "27.5" in-between 20 and 35.

Done.

33. Page 905: Remove ", dashed lines" and ", full lines" from the caption. Also the qualification "because the bed is alleviated..." only occurs in this caption. Would it not be sensible to have that qualification also in other figure captions where the profile is shown from km 20?

Done.

34. Page 906: Perhaps mention straight-off that these panels show annual values?. This is only mentioned for panel B but is equally true for all panels. Also, refer to the terminus when "the last grid node" is mentioned. This is also done in ms page 977:19.

We added the reference to the terminus after "the last grid node".
"[...] apparent erosion rate calculated as the volume of sediment that is transported across the last grid node, ***i.e. terminus*** *[...]"*
We removed the mention of the model year for (b) and moved it to the first sentence of the caption so that it applies to all panels:

Synthesis of transient simulations (Table 4) through comparison of ***the following quantities calculated for one model year****: (A) [...]*

4 Other modifications

We realized that throughout the paper we were using the notation $\mathrm{mm\,a^{-1}}$ *except on page 880 where we used* $\mathrm{mm\,/yr}$*. This has been corrected so that* $\mathrm{mm\,a^{-1}}$ *only is used.*

Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/3/C562/2016/esurfd-3-C562-2016-supplement.pdf

Interactive comment on Earth Surf. Dynam. Discuss., 3, 849, 2015.