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Interactive comment on "HyLands 1.0: a Hybrid Landscape evolution model to simulate the impact of landslides and landslide-derived sediment on landscape evolution" *by* Benjamin Campforts et al.

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Reviewer 1

Summary: The authors present a new landscape evolution model, HyLands, that combines models of landsliding and bedrock evolution. The backbone of the model, SPACE by Shobe et al. 2017, can shift between both transport-limited and detachment-limited cases of landscape evolution and therefore simulate the continuum of bedrock to mixed

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bedrock-alluvial to alluvial rivers. Here, bedrock erosion is modulated by the cover effect, which greatly depends on the rate that sediment is delivered to the channel. HyLands combines SPACE with a landsliding model that allows for sediment delivery in a highly punctuated fashion instead of a steady rate. The authors first demonstrate the steady-state solutions for the SPACE model in the Topo Toolbox. Landscape Evolution Model (TTLEM) framework. They then add landsliding to a natural landscape (Namche-Barwa region) and found that the modeled and observed characteristics of the landslide dynamics match quite well. Last, they devise a model run on a synthetic landscape where there is a 100-year period of intense landsliding to simulate widespread co-seismic landslides. They found that landslides create drainage rerouting from landslide blockage and generate channel knickpoints. They conclude by discussing calibration techniques and potential applications for HyLands.

Review: This manuscript is well written and contains a detailed description of the numerical model, HyLands. The literature review covers the field of numerical landscape evolution modeling and makes a compelling argument for why a model like HyLands is needed. The objectives and motivation are well-thought out and are clear to the reader. The discussion is thorough, and I appreciate the effort the authors took to flesh out potential calibration techniques and applications to their model. They conclude by stating that their model is “well-suited to address a range of new questions related to how channel-hillslope coupling modulates landscape response," which I wholeheartedly believe. However, I think this manuscript should take a more in-depth look at the steady-state behavior of this model. I believe this manuscript should be accepted with some minor revisions.

Verification:

The manuscript shows steady-state solutions for detachment-limited, transport-limited,and mixed bedrock-alluvial cases. These solutions and the associate figure are quite similar to the work in Shobe et al., 2017, and I am not sure it is totally necessary for them to be repeated in this manuscript. Figure 8 shows HyLands working remarkably well compared to the data of Larsen and Montgomery, 2012, but it seems that the model systematically overestimates landslide volumes for all scales of landslides. The author's attribute the overestimation of small landslide volumes to the inability of the model to deposit materials in the landslide scars. What is the reasoning for the model overestimating large landslide volumes?

Synthetic Landscapes:

At what spatial scale is the drainage re-routing occurring? From Figure 9 (d, e, and f) and Figure 10, it does not seem that the channel profile's location has changed significantly. The figures make it seem like the channel moves on the order of one cell size due to valley blockage and the formation of epigenetic gorges. Could these slight reorganizations, over long periods, create major drainage reorganization or river piracy? Related, how computationally expensive is the landslide (non-linear deposition) routing compared to the rest of the model? I'm really excited for researchers to start using this model. I would be interested to know how fast the model runs, and how modifications that complicate or simplify the landsliding component of HyLands would affect the computational efficiency.The first part of the model verification section details the steady-state behavior of detachment-limited, transport-limited, and mixed bedrock-alluvial landscapes. I would like authors to answer: **How does the steady state behavior of a mixed bedrock-alluvial landscape with landslides as the sediment delivery mechanism compare to a simulation without landslides?** My guess would be that the main controlling parameter would be t_{LS}, the return time for landsliding. For very small values of t_{LS}, small frequent landslides will dominate; however, there will be little time for the landscape to recover/build up storage of landslide material. In this case, I believe the model would act very similarly to the initial runs in SPACE. For large t_{LS} values, large but seldom landslides dominate. If the landslides are very rare, I think the landscapes will also act similarly to SPACE. In between these two extremes, I think there is potential for the landscape to behave quite differently. Please consider reading Zhang et al., 2018 (The Advective-Diffusive Morphodynamics of Mixed Bedrock-Alluvial Rivers Subjected to Spatiotemporally Varying Sediment Supply) paper which also considers the tool effect.

Reply: We explicitly want to thank reviewer 1, to review our manuscript in such a short period, given the challenging times. We are pleased that the reviewer appreciates our work and agrees on the need for the development of LEMs like HyLands. In the following, we address her/his specific comments.

RC 1.1 — The manuscript shows steady-state solutions for detachment-limited, transport-limited,and mixed bedrock-alluvial cases. These solutions and the associate figure are quite similar to the work in Shobe et al., 2017, and I am not sure it is totally necessary for them to be repeated in this manuscript.

Reply: We indeed reproduced the analytical verification methods for SPACE as earlier proposed by Shobe et al., 2017. We also considered moving this part to a supplementary file but decided to keep in in the main body of the text because of the following. Space has been developed and tested in the Landlab framework. We ported the same set of equations to the TTLEM modelling environment. To validate our implementation we tested it thoroughly by comparing model output with these well established sets of analytical fluvial equations. While this is not new from a scientific point of view, we believe it is important for every new numerical model to be tested rigorously against such benchmark equations. Given the scope of the GMD journal, we therefore decided to report on these comparisons and keep the fluvial model verification exercise in the main text of the manuscript. Moreover, the set of model runs we use here, is used at a later stage in the paper to show the impact of landslides on fluvial sediment dynamics. Showing the functionally of the fluvial component is therefore key to support our findings documented at a later stage in this paper.

RC 1.2 — Figure 8 shows HyLands working remarkably well compared to the data of Larsen and Montgomery, 2012, but it seems that the model systematically overestimates landslide volumes for all scales of landslides. The author's attribute the overestimation of small landslide volumes to the inability of the model to deposit materials in the landslide scars. What is the reasoning for the model overestimating large landslide volumes?

Reply: This is a valid point carefully observed by the reviewer. As we explicitly mention in section 3.2.2, we use the Namche-Barwa area solely to demonstrate and evaluate the performance of HyLands. We do not take into account a number of boundary conditions (such as uplift patterns, see section 3.2.2) which prevents us to reproduce exact features of landscape exhumation in this region. Therefore, we ran the model with standard parameters values and did not calibrate any of them (see also RC 1.5). We evaluated the the performance of the landslide algorithm against its capability of reproducing the shape of empirical universal magnitude-frequency and area-volume relationships. We did not aim to exactly reproduce the observed scaling relationships since this would involve calibration and uncertainty analysis of the model, which is beyond the scope of this paper. Regarding the Area-Volume relationship in particular, what we see is that the volume of small landslides deviates from the otherwise linear Area-Volume relationship (in a loglog space). Regardless of the carefully observed fact that overall, tis particular model run indeed seems to over predict landslide volumes. To improve the model fit, there are three essential landslide parameters which will adjust landslide volumes : C, phi and t_{LS}. Calibrating those will be feasible following pathways outlined in section 4.3 of the manuscript. We agree with the reviewer that we could have stated this more clearly and will rephrase some of the sentences in the corresponding paragraph:

- Figure 8.b shows that HyLands is capable of approaching the shape of the universal Area-Volume relationships found by...
- While HyLands seems to overestimate simulated landslide volumes for very small

landslides, the Area-Volume relationship simulated with HyLands approaches a linear relationship for larger landslides, similar to the shape of the observed Area-Volume relationship. Note that overall, landslide volumes as simulated with HyLands are slightly over predicted in comparison to observations. Study area specific model calibration would improve this fit but is beyond the scope of this this model evaluation in which we evaluate the capacity of HyLands to reproduce the shape of the universal area-volume relationship. We attribute the positive deviation from the linear Area-Volume relationship for smaller landslides to the nature of the landslide algorithm ...

RC 1.3 — At what spatial scale is the drainage re-routing occurring? From Figure 9 (d, e, and f) and Figure 10, it does not seem that the channel profile's location has changed significantly. The figures make it seem like the channel moves on the order of one cell size due to valley blockage and the formation of epigenetic gorges. Could these slight reorganizations, over long periods, create major drainage reorganization or river piracy?

Reply: Again, a very insightful comment. Testing the impact of landslides on river capture and drainage reorganisation would be a natural avenue for follow up research activities. The model setup we used to showcase the impact of landslides on landscape evolution does however not provide the 'right' tectonic configuration to test this hypothesis. In our synthetic model run, we focus on the the coupling between landslides and river-bed morphology. We therefore use a model set-up which is similar to the one used to evaluate the fluvial components of HyLands (Space). The initial surface of this run is a tilted plain which drains towards the southwestern corner, the only open boundary node. Therefore, from the first run steps onwards, all the water is forced toward this lower left corner. In order tho test whether the model actually reproduces river captures and drainage organisations, we suggest model setups with open flow boundary conditions. Moreover, you would probably like to test the impact of uplift or precipitation perturbations, which are, for the sake of simplicity and model demonstration, all kept constant in the current model runs.

RC 1.4 — Related, how computationally expensive is the landslide (non-linear deposition) routing compared to the rest of the model? I'm really excited for researchers to start using this model. I would be interested to know how fast the model runs, and how modifications that complicate or simplify the landsliding component of HyLands would affect the computational efficiency.

Reply: This is a relevant comment which we believe requires some attention given the aim of this paper (i.e. presenting a novel numerical model). The good news is that HyLands is fairly efficient both regarding landslide formation (the Culmann algorithm) as well as the sediment routing algorithm. In the updated version of the manuscript, we will added a row in the Table 1, indicating the average time required to complete one model iteration (Computation time per iteration). From the synthetic model runs, it can be seen that running HyLands with landslide erosion and sediment redistribution takes about double the time as it would when those processes are not simulated.

RC 1.5 — The first part of the model verification section details the steady-state behavior of detachment-limited, transport-limited, and mixed bedrock-alluvial landscapes. I would like authors to answer: How does the steady state behavior of a mixed bedrock-alluvial landscape with landslides as the sediment delivery mechanism compare to a simulation without landslides?. My guess would be that the main controlling parameter would be t_{LS}, the return time for landsliding. For very small values of t_{LS}, small frequent landslides will dominate; however, there will be little time for the landscape to recover/build up storage of landslide material. In this case, I believe the model would act very similarly to the initial runs in SPACE. For large t_{LS} values, large but seldom landslides dominate. If the landslides are very rare, I think the landscapes will also act similarly to SPACE. In between these two extremes, I think there is potential for the landscape to behave quite differently.

Reply: Evaluating the impact of landslides on long term landscape evolution is part of the motivation why we developed HyLands. Answering the question as to what extent landslides impact steady state landscape outlooks however opens up a bunch of other questions. A first question is related to the impact of different parameter values on the landslide erosion dynamics: the reviewer is right in his assessment that landslide return times t_{LS} will impact steady state landscape topography. However, equally important will be the cohesion factor C as well as the angle of internal friction ϕ. The way in which these factors influence landslide erosion patterns is currently not well understood. HyLands offers a tool to investigate these inter-dependencies using a suit of sensitivity analyses and by comparing simulated landslide patterns with observed landslide properties. Second, also the way in which landslide sediments are being distributed will influence 'steady state' landscape shapes. Again, running the model using a broad range of parameter values will improve our understanding as to what extend sediment redistribution influences landscape evolution. Parameters involved here are those controlling landslide sediment deposition on hillslopes after failure (Eq. 12, parameter Sc) as well as subsequent sediment redistribution by fluvial processes (the SPACE parameters). Finally, the way in which landscapes evolve towards a steady state will be at least as important to evaluate as the steady state result of landscape evolution. Answers to all those questions are currently open for debate. Nevertheless, we believe that this manuscript is not the right place to answer them: we want to use this paper to present a novel model and to evaluate its basic functionality. Albeit showing the results of one particular model run where a landscape is evolving to steady state might answer some of the previous questions, the answer would be a partial one given the strong interdependency of all processes involved in HyLands. Understanding those interdependencies in a rigorous sensitivity analysis would be a first step in answering the question as to what extent landslides influence long term landscape dynamics. The reason we did run the model into a steady state without landslides (Fig. 5) is because we wanted to test if our model is capable to reproduce well established theoretical relationships on fluvial dynamics which currently do not exist for landslides. We believe however that the reviewer proposes a very interesting potential application of HyLands which we now address in the discussion section of the manuscript (under 4.4: Potential applications) where we added the following paragraph:

A particular question which remains open for debate is the way in which landslides influence the evolution of a landscape to steady state. Albeit the stochastic nature of landslides will prevent landscapes to evolve towards time and space invariant topographies, even with landslides, landscapes will evolve towards a quasi steady sate if external drivers such as climate and tectonics remain constant. Although our mechanistic understanding of landscapes strongly improved by studying steady state landscapes, an even more interesting and challenging question would be to study the impact of landslides on the dynamic evolution of a landscape towards such a steady state. The latter being more relevant for most real-world landscapes which are known to be rather in transient than a steady state (Mudd et al. 2017).

RC 1.6 — Please consider reading Zhang et al., 2018 (The Advective-Diffusive Morphodynamics of Mixed Bedrock-Alluvial Rivers Subjected to Spatiotemporally Varying Sediment Supply) paper which also considers the tool effect.

Reply: We thank the reviewer for pointing us to this paper. Shobe et al (2017) discussed similarities and differences between the SPACE model and the approach of Zhang et al (2018). We agree that the tools effect would be another interesting addition to the current model framework.

Minor

Reviewer Point 1.7 — Figure 2: Where is the function, $f(H/H\star)$, I do not think it is defined in the text. I am guessing it is $(1 - exp(\frac{-H}{H\star}))$ and $exp(\frac{-H}{H\star})$. Also, shouldn't the function be on the ordinate and the variable $H/H\star$ be on the absciss a?

Reply: The reviewer is correct about the form of the function $f(H/H\star)$. It is a good point that although these expressions occur in equations 3 and 4, $f(H/H\star)$ was never explicitly defined on its own. We have added its definition to the caption of Figure 2, and in the same place referenced the relevant governing equations (3 and 4).

Both reviewers commented on the choice of axes in this figure. We have reversed the ordinate and abcissa.

Reviewer Point 1.8 — Line 171: "landslide" not "andslide"

Reply: Fixed.

Reviewer Point 1.9 — Line 252: citation for the sink filling algorithm?

Reply: Fixed.

Reviewer Point 1.10 — Figure 4: Not sure if this plot is made from actual data, but it would be interesting to show a similar figure before and after the landslide for visualization.

Reply: This is a hypothetical sketch. Adjusted the subscript by adding 'potentially initiate'

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Reviewer Point 1.11 — Table 1: (a) after Synthetic should be a superscript?Also, you may want to draw another line in the table to make it clear that the Pre, LS-Event, and Post columns refer to the Synthetic landscape and not the Namche-Barwa.

Reply: Fixed.

Reviewer Point 1.12 — Table 2: Same as Table 1, it is not clear that Before intense LS period belongs to the Synthetic runs, instead of the Real DEM run.

Reply: Fixed.

Reviewer Point 1.13 — Line 349: Why did you choose 20,000 years for the return time?Would this value affect your results? If it is too long, perhaps you would not collect enough data to generate Figure 8.

Reply: Good question. We did not calibrate any of the model parameters for reasons discussed in the manuscript and in RP 1.2. Although the other parameters could be set to theoretical values, $t_L S$ is a new parameter introduced in this model. We therefore set the $t_L S$ to 2×10^4 years which is a rather arbitrary value. Parameter sensitivity runs in future work will show the impact of changing the landslide return times. We added the following sentence in the manuscript to clarify: Evaluation of model sensitivity to changing values for t_{LS} would be one of the natural avenues for further work.

Reviewer Point 1.14 — Figure 7: Should the unit be m$^{0.5}$, not m? Would log units be more useful?Also, perhaps switch the locations of E and D so D is on top, which corresponds to the color bar. Are the color bars for the 1st and 2nd column supposed to be different?Also, the figure caption shows the time steps for the 3rd column as 5, 500, 1500, and 2000 years, but the row titles show different values. Are they supposed to be different? Is so, why? Last, do landslides stop occurring in the simulation because of the absence of uplift?

Reply: All very good suggestions. Fixed to m$^{0.5}$. I definitely tried log units because those would be more familiar to the reader. Unfortunately, this does not really work out well since small erosion and deposition rates would end up being negative (values smaller than 1). This would prevent us from plotting erosion and deposition on the same plot. Color bars are the same, and since patterns of landslides are almost not different in the previous version of this figure, we dropped the colorbars. There was an error in the caption. We removed this part of the caption as the years are already indicated in the first sentence of the caption. Landslides do not stop to occur. This is more clear on the new version of the figure. Given that the second reviewer also had some valuable suggestions for this figure, we made a new version of Figure 7.

Reviewer Point 1.15 — Figure 8b: I think there are missing symbols in the legend.

Reply: Sorry for that, we messed up the legend. The grey bar is now properly added to the figure.

Reviewer Point 1.16 — Figure 9 (also, Figure 10 and movies): I think the color bar for topography in panels (a), (b), and (c) are incorrect. It should be from 0 to 300 meters, not 0 to 1meter.

Reply: You are absolutely right. We corrected this in both the figures and the movies. Moreover, we made several additional adjustments to this figure in order to improve clarity and to get the message better across (see also SP **??**)

Reviewer Point 1.17 — Figure 9 caption: I think there should be more explanation of how epigenetic gorges are formed in the text. I believe the river jumps out of its original channel after being filled by alluvium and is routed on bedrock. How sensitive is this behavior to the algorithm used to fill sinks?

Reply: We rephrased the corresponding paragraph in the text as:
The drainage re-routing mechanism dominates in the simulations presented here and results in the formation of epigenetic river gorges (Fig. 9). Epigenetic river gorges are characterized by rivers incising into the bedrock of former valley walls due to the blockage of the formal channel by landslide derived sediment (Ouimet et al. 2008).
Regarding the sensitivity to the fill algorithm: after landslide blockage of the river path, a fill algorithm is used to identify landslide lakes and water is rerouted following the steepest path using a D8 flow direction algorithm.

Reviewer Point 1.18 — Figure 10: Where is the rerouting? The channel pathway looks the same to me; is there a better way to illustrate the rerouting?

Reply: The rerouting happens on Figure 9, when landsliding kicks in. A major rerouting happens right after the start of the LS simulations (Fig 9.c to the LS Fig.9, d). Small changes to the flow path continue to occur from Fig. 9.d to f. Once landsliding stops, the channels are not blocked any longer and will mostly stay in place (Fig. 10)

Reviewer Point 1.19 — Line 417: Can you show knickpoint generation with a distance upstream vs.slope plot? The knickpoints are very apparent in the movies, but I do not think a series of topographic profiles would show the knickpoint adequately.

Reply: We do not fully understand this comment. We believe the presence of knickpoints is very apparent on Fig. 9 d-f. We added a reference to this figure in the manuscript to enhance clarity. We also added some text to better explain the phenomena of epigenetic river gorges.

Reviewer Point 1.20 — Line 419: “Figs.” not “Fig. s"

Reply: Thanks. Fixed.

Reviewer Point 1.21 — Line 453: Please consider citing Zhang et al., 2018.This paper looks at how varying sediment transport inputs (e.g. from landsliding) affects bedrock erosion with a tools and cover model.

Reply: Done, see also comment before.

Reviewer Point 1.22 — Line 528: I would be very interested if your model can reproduce this.

Reply: We too.

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