Interactive comment on "Effect of changing vegetation on denudation (part 2): Landscape response to transient climate and vegetation cover" by Manuel Schmid et al. Anonymous Referee #1 Received and published: 5 March 2018 Review of Schmid et al. Summary: The authors attempt to link changes in landscape form to changes in climate and vegetation.

Reviewers comments are in Italics, **the authors responses are in bold text**.

Note to readers and editors:
We provide a response to this review here before the second review has been received, we explain how and where the revised manuscript will be addressed and we will supply a single revised manuscript after we have a second review. Thus, no revised manuscript is now attached with this response.

Although this is a worthy topic to explore, it would be a challenge for even a more complicated model because we simply don't know enough about the processes and feedbacks involved. Considering that this model greatly oversimplifies what we actually do know, its contribution to our understanding is not obvious. Those familiar with these processes will view the results with skepticism, and those who aren't may believe the results without fully appreciating all of the short-cuts and assumptions baked into the governing equations. I know how much work goes into C1 modeling exercises like this so I always try to be open-minded when reviewing these types of manuscripts but, in this case, I cannot recommend publication.

1) The reviewer raises an interesting point, related to different philosophies to modeling of landscape evolution. We respectfully disagree with the reviewer's assertion that the community simply doesn't know enough about the processes involved to approach problem. Science progresses by a stepwise confrontation of what we don't know. The approach followed in our study is to identify emergent behaviour based on a simple set of assumptions. As more is known (from much needed field studies) about how to better parameterize models then of course improvements can (and hopefully will) be made on our approach. As highlighted below, our manuscript builds upon approaches already published in the literature. We also respectfully disagree with the reviewer that a more complicated model is needed at this stage. Inclusion of a more complicated model at this time would only result in including poorly constrained parameters (particularly over the long-timescales we investigate). In the following response to this review we expand upon the difference of opinions in how to approach this problem, and also address these reviewers points, many of which we agree with and can readily implement into a revised manuscript.

Based on the comments below we believe the reviewer comes from the perspective of short-term observations of detailed eco-hydraulic processes. While this field of study is definitely useful, that is not the scope of this study, where we are interested in long-term (millenial to million year timescale effects of vegetation and climate on topography). There simply is no way to include short-timescale ecohydraulic

modeling approaches into a study that is simulating million-year timescale processes. This is because a) computationally CFD and 3D (e.g. hydrogeosphere) type modeling approaches can not be conducted over these timescales, and b) the data inputs needed to constrain models over long time scales do not exist. Thus, a sensitivity study such as we present that seeks to identify emergent behavior between vegetation-climate-surface process interactions is the first place to start.

We refer the reviewer to a publication of Dietrich et al. 2003, which nicely summarizes different perspectives of how landscape evolution models are meaningful to answering scientific questions. More specifically we follow a approach Dietrich described as 'essential realism' whereby:
"This condition combined with non-linear, threshold dependent erosion processes leads to a significant component of indeterminacy in the evolving topography. Therefore, it is unrealistic to expect to predict the exact topography of a landscape at any particular time, including the present. Instead the gross trends, the quantitative relationships, such as illustrated in Section 2 and the references cited therein, are the features landscape evolution models can realistically hope to explain."
Thus, by the very nature of how this study is set up we are interested in an emergent behaviour in vegetation/climate interactions over geologic timescales. Thus, while we appreciate the reviewers perspective, she/he is looking at this problem from a very different perspective, and disregarding how an entire sub-community of geoscientists approaches modeling of Earth surface processes..

We note that in the reviewers comments below she/he has issues with the governing equations and modelling approach used, we note that we follow the published approach of Istanbulluoglu and Bras, 2005 and highlight that this study lacks the detailed consideration of processes the reviewer would like, yet has had a highly significant impact in the field of long-term landscape evolution modeling (98 citations). Furthermore, the general approach of applying landscape evolution models over long timescales has provided many valuable insights into different geomorphic and geologic problems (Howard 1994, Tucker and Slingerland 1997, Whipple and Tucker 1999, Collins and Bras 2004, Jeffery et al. 2013, Yanites et al. 2017, and many more ….)

Nevertheless, we realize many other readers of this journal, will share a similar perspective of this reviewer, so in the revised version of the text, we will highlight these contrasting perspectives in the start of the background section with a new subsection (landscape evolution modeling and the applicability of these results). We hope that these changes and also the other changes outlined below will reach a happy middle-ground with this reviewer, where the (essential-realism) approach of Dietrich et al. is recognized as a valid way of understanding emergent behaviour in systems for processes that are to complicated to simulate over geologic timescales.

My comments are mainly focused on the governing equations. Because I believe them to be either unsupported or flawed, I don't address the results in detail. If the governing equations *of a model are not honoring reality in some fundamental way, then its output will be unreliable.*

2) We are unable to respond to this comment without details from them. We respond to the detailed examples given below. We agree with the reviewer that well-calibrated field-studies of geomorphic transport laws are lacking in the literature, and we have based our analysis on what little information is available.

It didn't seem like the authors tried to determine whether the model was working correctly. Thanks to 10Be, we have erosion rates for many watersheds around the planet, including catchments that are similar to the ones modeled here. Before we can believe that the model works, the authors ought to run it on one with published erosion rates. This would have been an important first step before embarking on the rest of the project.

3) We apologize for any confusion at this point, the manuscript currently states that the goal is not providing a calibrated study of the Chilean Coastal Cordillera (see Introduction, last paragraph and Results section 4.). This is because catchment average 10Be denudation rates have not been published for this areas, nor are river sediment-flux data available. The rock uplift rates used, are based on the long-term averages from thermochronology, as cited in section 3.2. However, we have to disagree with the reviewer on two grounds that 10Be measurements would be useful here: (1) even if 10Be measurements were available, they would not be directly comparable to this study because the integration timescale of in-situ produced 10Be will be 10's to 100's of years such that average rates over these timescales would be produced (e.g. see Schaller and Ehlers, 2006 EPSL), and comparison to predicted erosion rates vs. a higher fidelity time scale would not be meaningful. (2) We again emphasize (see starting response to reviewer) that the goal of landscape evolution modeling is not to reproduce reality. This simply isn't possible given how little we know about the relevant processes over millennial timescales. Sensitivity studies based on what is known (as done in this study) are the first step forward.

Given that our entire model setup and approach is a sensitivity analysis to poorly understood processes acting in the Chilean Coastal Cordillera, we have evaluated model performance using first order topographic metrics (e.g. relief, slope, and Ksn) for the application area. Comparing model predictions to these first order attributes of topography avoids over interpreting the model results. A more detailed comparison than this, would require data that doesn't exist. Thus, we've framed the interpreted model output and modeling approach around first order topographic metrics for these area. This is a conservative approach to interpreting climate and vegetation effects on topography.

<u>To address this concern raised by the reviewer</u>, we will add additional text in section 3.1 to emphasize more strongly that this is not a calibrated study of the Chilean Coastal Cordillera.

This model is driven by, essentially, two governing equations. The first describes soil creep via linear diffusion. The authors use a formulation, proposed in a paper from 2005, that links the diffusivity to vegetation density that was based on little data and only accounts for physical processes (eg, rainsplash) and ignores bioturbation. Are there field observations to support that physical processes dominate soil creep at their field site? Importantly, there is no support for the nonlinear equation that relates diffusivity to vegetation density.

4.)We thank the reviewer for highlighting that clarification is needed on this point. The approached used in our study was first published in models by Istanbugulough and Bras, 2005, and Collins et al. 2004. The field/laboratory investigations supporting a negative exponential relationship for diffusivity Kd as a function of vegetation cover comes from Alberts et al., 1995, Dunne, 1996 and Dunne et al. 2010. Thus, there is a history of peer-reviewed articles supporting our approach.
We will modify the manuscript to add the above references.

Over a narrow range of precip, Ben-Asher et al (2017) found a linear inverse relationship between diffusivity and precipitation which, when combined with the present study's relationship between veg and precip might yield something like the negative exponential equation adopted here but the authors have not demonstrated that. Moreover, a paper that examined diffusivity across a wider range of precip (Hurst et al, 2013) found the relationship to be weakly positive – which runs counter to what the authors have assumed. There is little support, then, for the way the soil creep equation has been parameterised.

5.)Thank you for bringing these references to our attention. The Ben-Asher study is interesting, although we can not apply it here because they link diffusivity to precipitation, not vegetation change. The Hurst et al. paper is unfortunately not usable for our purposes. First of all because experimental design of the study looks at a diverse range of lithologies and finds essentially no correlation between precipitation and diffusivity (sediment transport coefficient). The Hurst paper clearly states in the Fig. 1 caption that the weakly positive relationship has a R2 =0.27 (n=24) for a linear regression of only a subset of the data. For the reviewer to conclude there is 'little support' for our approach based on a R2 of 0.27, from a study that doesn't consider vegetation differences does not seem to be a correct application of the Hurst study to ours. Furthermore, in the Hurst study, the authors do conclude that they find two different values for basins with the same vegetation density but those catchments were situated in a different lithologic regime and so the authors themself conclude that this difference is probably due to soil properties emerging from different underlying lithologies.

The second governing equation describes erosion by flowing water. Before describing my main concerns, I should point out that this section (lines 193-215) was difficult to follow and was missing some critical details. For example, variables appear without explanation or description and the relationship between eqn 5 and the others that follow was unclear. Also, there was no explanation of how rainfall is applied (eg, storm frequency and magnitude), how runoff is generated, or how runoff generation is affected by vegetation density (eg, via interception). The point about storm frequency and magnitude is especially important *because changes in climate will affect the distribution of both of these but not necessarily in a uniform way. Given that, here are my main comments regarding the way that erosion by flowing water is treated in the model.*

6.)We will modify the text to clarify how eqn 5 links to the other equations. Thank you for noticing this. We will also clarify some problems with parameter-descriptions in the equations and apologize for the inconvenience and not catching this in the first place.

Concerning the surface water hydrology - we will add text in Section 3.2 to explain the approach used. Our approach is similar to other long-term landscape evolution studies and we provide references (Croisssant and Braun, 2014, Jeffery et al. 2014, Yanites et al. 2017). The reason for using mean annual precipitation in many long-term landscape evolution modeling studies is (a) information about paleo (and even modern in many cases) precipitation duration, intensity, and interval are not known and inclusion of an unconstrained parameter in a model is counterproductive for a sensitivity study, and (b) the model simulation time step can be significantly larger (e.g. 100 years in our case) when mean annual precipitation is used - thereby allowing millions of years to be considered. However, including storm events increases simulation time dramatically by requiring hourly time steps and can make studies like this intractable.

<u>We will address this comment in the revised text</u> where the precipitation distribution is discussed (methods section) to highlight that a stochastic distribution of precipitation is not implemented, and why (above reasoning).

1) Linking the roughness coefficient to the vegetation in the way that was done here ignores the fact that the effect of vegetation goes beyond a simple measure of 'vegetation density.' For example, imagine two landscapes with 70% vegetation density: one is covered by shrubs such that the ground surface between each plant is essentially bare while the other is covered by grasslands. These two landscapes, despite having the same vegetation density, will have different manning's n values on the hillslopes. Since we know that vegetation community changes with climate, the model's attempt to scale manning's n on the basis of vegetation density is not realistic. Indeed, I looked at the field sites via Google Earth and it was clear that vegetation community does change as a function of precip in those regions. I can easily imagine situations where manning's n actually increases with a decrease in vegetation density, the opposite of what is assumed here.

7.)The reviewers comment states that the metric of "vegetation cover" that we used in our study oversimplifies the interaction between different plant communities and mass transport and erosion. While we want to acknowledge that this is a very good point which certainly holds true, we also want to defend our decision in using vegetation cover. The available satellite data which gives one the possibility to make a spatial distinction between different types of vegetation has a resolution of 500m, which would resemble 5 grid cells in our model domain and represents a integration

over 11 years, which makes it hard to extrapolate these data to a distinct vegetation-community for longer timescales. We argue that, our approach of applying a very simple transient forcing which resembles a change in "simple vegetation-density" is probably not resembling reality, it would still be much harder to get a realistic transient time series of shifts in plant functional types for changing climatic conditions. This is however part of ongoing research to incorporate a fully functioning dynamic vegetation model into this landscape evolution exercise.

<u>To address this comment</u> - we will modify the methods section and section 5.5 of the text to mention this caveat and to highlight to readers that this is a simplification that can be hopefully improved up with additional data sets and calibrated erosional laws in future studies.

2) Given the comments above, both landscapes will also have different critical shear stresses. For example, soil with shallow grass roots will be more difficult to erode than the bare soil between shrubs. It doesn't appear that the model takes this into account.

8.)While the reviewer is certainly right that the shear-stresses would certainly differ, in this study the goal was to have as few free parameters between simulations for different study-sites as possible, therefore we decided to focus on the effect of vegetation cover to the river erodibility factor K. Given that condition, we argue that choosing a common critical shear-stress is reasonable because of the uniform substrate lithology used throughout the entire simulation duration.

<u>To address this comment</u> - we will modify the methods section and section 5.5 of the text to mention this caveat and to highlight to readers that this is a simplification used.

3) It appears that the model doesn't distinguish between overland flow on hillslopes and river flow. If so, the authors are assuming that a source of roughness on the hillslopes – the vegetation – is also contributing to roughness in the rivers. For example, if the authors envision shrubs growing on their hillslopes then they must also be growing in the rivers. Again, I don't see how this is realistic. Moreover, the type of vegetation really matters here with respect to flow depth. Short grasses would have a greater manning's n with low flow depths (ie, overland flow) than with deeper flows (ie, rivers). Conversely, shrubs would have a lower manning's n with overland flow than with river flow.

9.)The reviewer is correct that the stream-power model widely used in the literature and modeling studies does not distinguish between different regimes for surface flow vs. stream-flow. The dominant effect of diffusive hillslope processes over advective stream-flow processes is assumed to be regulated by the critical shear stress and the ratio of diffusive material flux vs advective material flux which is a commonly used approach in large-scale landscape evolution modeling (e.g. see references cited above). The decision to keep vegetation cover spatially uniform over the model-domain comes from the poor constraints about how effective channelized flow in rivers actually removes the superimposed vegetation cover. Previous studies

linked the removal of vegetation to bed-shear stress within a river-channel but the relationship on how effective this process works is still not well understood. There are two processes to consider here: it still is very unclear how different types of vegetation are actually able to withstand surface shear-stress because of bending of branches and stems and, if they are removed, how fast they will grow back. The second process is the adjustment of the ecosystem to stream-flow by shifting the vegetation in or near a river channel to plant functional types that are more accustomed to these positions which would certainly lead to a shift in vegetation type but it is unclear how this will act on the vegetation cover metric. Also, while the reviewers comment will certainly hold true for larger basins with larger rivers and more diverse vegetation types, from field-observations that we made within the focus areas, it emerges that most of the stream-bed is actually made up of a dense root-network and vegetation cover very similar to the surrounding hillslopes because of the small catchment sizes and 100 m model resolution.

<u>**We will address this reviewer's concern**</u> **by modifying the manuscript in Section 5.5 (Model caveats and restrictions) to mention these complications in that they are not represented in the modeling approach because a means for scaling these processes to long time scales is not known.**

4) There was no explanation of how the critical shear stress was calculated. Presumably some assumptions were made regarding bed and hillslope material but these were not described. Does the model keep track of the evolving particle sizes as climate changes? More vigorous runoff will coarsen the river beds and hillslope surfaces but I didn't get the sense that this was incorporated into the model. Also, a lower vegetation density will expose the ground surface to raindrop impacts that will mobilize finer material more readily. Was this accounted for? Again, it didn't seem like it.

10.)As the reviewer correctly mentioned the critical shear-stress was chosen in accordance to values presented in other studies for granitic underlying material. We want to point out that this model was set-up as a 1-layer detachment limited case, following the Fastscape Algorithm developed by Braun and Willet, 2013, which resembles a bedrock dominated landscape where the ability to transport eroded material out of the system is the main driver for the evolution of river catchments. This detachment-limited problem formulation is, even though the general transport formulations are still discussed in recent geomorphic literature (e.g Davy 2009, Pelletier 2011) used extensively for a variety of different landscape evolution models and is believed to produce realistic results for headwater channels (Howard, 1994). We agree that landscape evolution models would benefit greatly from more effective algorithms which would incorporate more hydraulic parameters but studies have shown that even more complicated models which incorporate a higher-level of water-/bed- interactions fail to produce a clearly better prediction of channel morphology (Turowski et al. 2007). Therefore we agreed to use a simple detachment limited formulation for the landscape evolution model which lacks the ability to track

evolving particle sizes of river-bed and hillslopes and to incorporate the effects of coarsening/fining of bedstructure back to critical shear-stress.

<u>To address this comment</u>, we will modify the manuscript in the model setup section to add the above references for how the shear stress is calculated.

One of the model's limitations are well-illustrated by Figure 9. It predicts long-term erosion rates on the order of about 0.2 mm/y. This is on the high end of known soil production rates; I would venture to guess that soil production rates at the sites in Chile are quite a bit lower given the dry conditions (0.2 mm/y is what you get in weak-ish bedrock in the Oregon Coast Range where its wet and has lots of trees doing physical weathering). This means that, at these high erosion rates, the landscape would run out of soil yet the model seems to assume an inexhaustible supply of erodible material. In the real world, the loss of soil would have important consequences for runoff processes and the ability of plants to grow but the model seems blind to these.

11.) The reviewers comment links to the answer we gave above about the detachment-limited setup of our model which assumes a bedrock-dominated landscape and neglects the effects of different soil covers on erosional processes. The 0.2mm/yr long-term erosion rates are a product of the conservation of mass approach within our model domains, which experience a uniform tectonic uplift of 0.2mm/yr. This value is supported by thermochronology studies done in Coastal Cordillera catchments north of the location, and we cite the reference for this value used in the paper. We acknowledge that these regions may have a different uplift history than our focus areas in the Coastal Cordillera, but provide the best dataset for uplift estimation in this region, but there is currently no other observational studies published that constrain this value better. We hope that other studies, done within the Earthshape project, specifically done to determine weathering rates and catchment-wide erosion rates based on 10Be will help to better constrain these input parameters.
Finally, we agree with the reviewer that approach assumes a temporally and spatially constant material is being eroded, and the transition from soil mantled to bedrock lithologies would potentially introduce a different response. However, we can respond that (1) data are not available to provide a believable prediction of soil production rates, (2) introducing an additional processes into the modeling (e.g. the transition from soil/regolith to bedrock mantled landscapes) could be a study on it's own, and (3) there is currently no reason to believe apriori that the landscapes ever were stripped on their soil/regolith such that the erosivity would would vary. Rather than introduce these uncertainties into our analysis, we follow the approach of many other modeling studies working on these timescales and assume the substrate material properties remain constant through time.

Finally, there was no attempt to provide any error estimates in the predictions. I understand that this is not common practice with landscape evolution models but it should be and can be done (see papers by Tom Dunne on stochastic modeling). For example, the model makes certain predictions about how erosion rates may vary over time after changes in vegetation

(Figure 9). Given all the potential uncertainties embedded in the governing equations and how they were parameterized, how confident are the authors that a predicted erosion rate of 0.2 mm/y is statistically different from a predicted C4 erosion rate of 0.4 mm/y? I'm skeptical that this model can predict annual erosion rates accurately to one tenth of a millimeter. My concern is that models like this one, while perhaps useful for demonstrating basic principles to students, are not well-suited for answering important scientific questions, especially when they haven't been tested under the relevant circumstances.

12.) We believe that the reviewers comments are oriented towards a smaller-scale, shorter timescale analysis. As she/he pointed out already, large-scale landscape evolution models are not always fit for implementing these analysis. While we see the merits of knowing the uncertainties in the predicted values, this requires known uncertainties in the observations / input parameters to implement a stochastic approach. For example in this study, it would be hard to define an uncertainty e.g for the change of mean annual precipitation with vegetation cover. We could implement a range of these changes, extracted from other regions on Earth but this would not be an error estimate but solemnly the product of other regional boundary conditions in these regions. Furthermore these approaches are not common in long-term landscape evolution studies because the emphasis (as we started our response to the review) lies in the exploration of emergent behaviour due to transient forcings and not to reproduce reality. We acknowledge that a landscape evolution model which would be able to reproduce exact replications of landscapes and inherent fluxes would be best for the scientific community, but, still due to the problem of some poorly understood processes and constrained variables, we think that there still lies value in focusing on simple models and analyse general behaviour to gain a better understanding of possible underlying, large-scale processes.

Finally, the reviewer's statement that this study is "...perhaps useful for demonstrating basic principles to students, are not well-suited for answering important scientific question,..." is unconstructive. We are not aware of any other study in the literature that demonstrates the counter intuitive and non-linear responses demonstrated in this study. We would find merit in this comment if the reviewer evaluated the results presented and discussion (text and figures) and highlighted how this is already a well-known result.