RESPONSE TO REVIEWS - ESurf Manuscript

Effect of changing vegetation on denudation (part 2): Landscape response to transient climate and vegetation cover

By: Schmid et al.

Responses in blue, original comment in black.

Response to Associate Editor: Rebecca Hodge (Univ. Durham)

I am happy that the authors should proceed on the basis of the comments and reviews received so far. The reviews are admittedly mixed, but on the whole the reviewers feel that there is merit in this work. When addressing the the points that the reviewers identify, the authors should consider how they present and justify the model parameter selection, and whether any model calibration is possible or appropriate. There are also some suggestions for how the model results could be broadly compared with examples from the literature. As identified by a couple of reviewers, a conceptual model might help to summarise the model findings. Finally, I would also encourage the authors to consider whether there are other parts of the paper that could be made more concise.

Dear Prof. Hodge:

At first we like to thank the referee's and the editor for the very useful comments on our manuscript which we hope will improve the quality of the presented research and make it more useful for other interested readers. We also would like to thank Taylor Schildgen and Erkan Istanbulluoglu who used the open discussion to bring in their valuable expertise to the manuscript in the form of short comments, which were also very helpful.

We hope that the revised manuscript we present here helps with clarification of the referee's criticism and fits the high standard of esurf.

The most extensive changes to our manuscript are in the Background and the Methods section to address the points of criticism that referee's brought up about the readability and clarity of our model-approach and parameter selection. Discussion section 5.6 (Model Caveats) has also be expanded to address reviewers comments. We also added a new subsection 2.1 which we hope will explain the general approach we used for conducting our model experiments. We also tried to create clearer reference to the applicability of our results to a field-application by adding new discussion section (section 5.5). Since both, Taylor Schildgen and Erkan İstanbulluoglu, as well as Referee 2 brought up the point that a conceptual model or at least a figure describing our results more conceptually would help with understanding the results, we hope to find a good middle-ground with the new figure 18. Since most referee's commented that the paper is too long in general, we reduced text throughout the manuscript to make it more easy to read and to reduce repetition, but due to additional paragraphs and sections we needed to add to address the other valuable points of the referees, the paper did not become shorter at the end of the review process. However, for the longer-format version of ESurf we prefer to have a thorough manuscript that provides a valuable reference for interested readers rather than a short manuscript that is missing sufficient details for a thorough understanding.

Attached in this document are our line-by-line answers to the referees comments and we also decided to answer the short comments, because we found them very helpful and wanted to acknowledge that these scientists took the time to review a paper for which they were not the designated referees which is a nice effort and in the spirit the open-discussion format of EGU journals.

At the end of the document the revised manuscript with tracked-changes is attached.

Please let us know if you have any questions.

Sincerely,

Manuel Schmid and Todd Ehlers (corresponding author) on behalf of all authors.

Response to RC1

Responses in blue

Summary: The authors attempt to link changes in landscape form to changes in climate and vegetation. 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 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 the 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 published.

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.

To address this concern raised by the reviewer, 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.
To address this issue 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.) To address this 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.

To address this issue we will address this comment in the expanded the text in the model caveates section (Discussion section 5.5) to address these points.

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.

To address this comment - 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.

To address this comment - 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.

We will address this reviewer's concern 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.

To address this comment, 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.

Response to RC2

Responses in blue

The paper uses an established landscape evolution model (Landlab) to evaluate the effects of precipitation and vegetation cover change separately and combined on a myr time scale. It is timely and addresses the important question in earth science if vegetation is a main driver of denudation, and hence fits well into ESurf. Thank you for letting me review this manuscript; I am not a landscape evolution modeller, and hence it was a challenge for me, and I have to leave more technical comments to the experts. I find the topic and results fascinating though. My perspective is more process-oriented, and this is also where my criticism, but also fascination originates in. Sorry for the delay. I find the paper overall well written, but it is too thick in times.
The discussion suffers from being too long at the one side, but could gain a lot from a comprehensive figure that summarizes the outcomes conceptually. Please consider that not all people who are interested in this topic have experience in landscape evolution models, and have potentially never seen the outputs of Landlab before.

1. Thank you for this interesting and thorough review which helps a lot with advancing the scope of the study. Actually the shown figures are not landlab-specific output but timeseries of topographic metrics. We acknowledge that this could be solved by also addressing your next point of criticism, with more specific figure captions.
To address the reviewers point we have added a figure to visualize the concepts behind the model more clearly.
Concerning the text being too thick in parts: We have done our best to shorten the manuscript where possible, but the large number of reviews received for this paper (4 total) have required text additions to clarify points so the overall length of the manuscript. ESurf is a long format journal article venue and we chose it specifically to have sufficient space to explain concepts as needed, while keeping the text as concise as possible.

This is also especially important with regards to the Figure captions, which are often not specific enough.

2. See the answer to paragraph above.
To address the reviewers point we will rework and expand the figure captions to make it more clear to readers to understand the underlying data.

Please also add something to the title that clarifies the type of study, e.g. the time period considered or/and that it is a landscape evolution model study.

3. Thanks for this thoughtful advice
To address the reviewers point we will try to make the title more specific about the type of study that was conducted

I have two main criticisms that made the paper more cumbersome to understand; the first regards the origin of the vegetation cover and the oscillation part of the paper, it is not clear on which base you chose these assumptions.

4. Thanks for bringing this to our attention. The basis for choosing the vegetation cover values was data from the modis-mission for catchments situated around the Earthshape focus areas. The oscillating time series was chosen as an

approximation of 100kyr milankovitch cycles which has been identified as one of the main frequencies in Earths climate cycles (Broecker & van Donk, 1970, Muller and MacDonald, 1997). This frequency was chosen because on the long-timescale our simulations were conducted, the 100kyr cycle is the most stable cycle with the highest periodicity induced by planetary motion.
To address the reviewers points we will add/modify text in the manuscript in section 3.2 and potentially figure 5 to make this more clear to readers

The second is that the title covers a large topic; however the interpretation of your output is quite limited and stays very close to the model output. It doesn't include literature or discussion points from studies outside of the landscape evolution world, e.g. the effects of knickpoint retreat, or an interpretation from the process-domain, e.g. denudation rates on deforested catchments without vegetation cover (rates summarized e.g. in Montgomery, 2007).
From my perspective, there are two ways to resolve this, either you claim a larger importance and add e.g. an overall conceptual figure and include literature from other fields, or you modify the title and narrow it to the landscape evolution world, which is what I would opt for.

5. That is a good and well-reflected point, thank you! We'll try to get a better representation on the scope of this study by adjusting the title and adding more explanation in the background part of the paper. We acknowledge that a model-setup like the one we used with is probably not suitable for answering questions of soil-erosion through agriculture because of the 1-layer setup assuming detachment-limited conditions that are thought to prevail in bedrock dominated basins. The Montgomery study you brought to our attention, while being a very interesting and helpful study aims to quantify exclusively matters of soil-erosion rates on agricultural landforms which is not the real scope of this study.
To address the reviewers point we will make it clearer in the title which field of study this paper aims to address

I think this would also reduce the weight of earlier criticism of the paper which I understand where it comes from. The fact that you apply an average vegetation cover, hillslope denudation and river incision is represented in the same equation, and that there is no representation of groundwater in the model justifies the question what the significance of the study is, and I suggest to try to do a better job in clarifying this.

6. Thanks for this point of criticism. We want to clarify that we do not apply an average hillslope denudation and river incision to our model, but that those values are results of this as the different differential equations for hillslope diffusion and fluvial erosion are solved over each node. We do however apply a spatial uniform vegetation cover on our focus areas. We acknowledge that this is a simplification of reality but due to the conception of our model which focuses on quantifying the first-order behaviour of topographic metrics for different settings of initial vegetation cover, we would argue that this is a reasonable approach.
To address the reviewers point we will add text in section 3.2 to explain the justification behind this approach better.

In parts it sounds like the reason for this paper is to develop the model setup for the following papers, which doesn't really help to assess how your paper advances science. Generally, I find the mix between a setup of non-natural conditions (e.g. precipitation without vegetation change) in combination with the "loose" tuning to the Chilean catchments problematic.

7. We thank the reviewer to point out that we need to clarify the general model setup and why we choose these non-realistic conditions of varying only precipitation or only vegetation cover in combination with metrics specific for the Chilean catchments. The general idea behind this was to delineate the separate effects of a coupled system (vegetation / precipitation) on landscape through specifically designed model experiments. Another way of doing this/ thinking about this would be to conduct a large-scale flume experiment with constant imposed rainfall and different values of vegetation cover and vice versa but numerical modeling brings us the opportunity to explore these relationships and explore the implications of existing physical relationships proposed in literature.
To address the reviewers point we will clarify why we have chosen these specific transient forcings in section 3.2

If you would like to investigate the effects of both, precip and veg independently, then why not use a catchment that has equally distributed aspects and slopes, so that you can make more comprehensive interpretation of how catchment topography controls the flux?

8. Thanks for this suggestion. I think this is aimed at the comparison of our model results to the topographic data that was extracted from the Earthshape focus basins. We would like to point out that in addition to basins that share equally distributed aspects and slopes, another important factor would be to that the basins also need to show same underlying lithology and we would need additional ground-truthing that the detachment limited case we apply in the model-setup holds true for the observed catchments. The Earthshape catchments were also chosen as part of the dfg-funded priority research project with the aim to produce inter-comparable data between different projects. We are unaware of a better location to conduct such a comparison that contains a similar tectonic setting, lithology, and large climate and ecological gradient.

Please try to avoid to mention that you will model the evolution of the catchments in more detail later, this leaves the taste of salami-slicing. The study should stand for itself. The same is also true regarding the companion paper.

9. Thank you for this feedback. The reason why we mentioned the ongoing work of modeling these catchments is that we are in the process of developing a coupled model-setup between a state-of-art dynamic vegetation model and this surface process model, but we see your point and have modified the manuscript.
To address the reviewers points we will try to clarify that this study is not dependent on the results of these future models but can be more thought of as setting the interpretation-framework for future studies in this direction.

I miss more references in the method section, so that it is clear what of the approach is “best practice”, and which you developed yourself or used for the first time.

10. Thank you for bringing this to our attention.
To address the reviewers point we will add more references explaining the basic model setup and the underlying equations.

Figure 17: Please explain more in detail where these result come from; e.g. the dotted line in b should look more like in a in the grey field?

11. To address the reviewers point we will elaborate more about the interpretation of these results in the figure caption and the discussion section of the paper.

Response to SC1

Responses in blue

This is a very interesting paper. I have some suggestions and general comments for improvement and clarity of the theory used. The length of the paper can be reduced. I felt some descriptions were repetitive under various subheadings which can also be reduced. It follows a fairly standard writing style that described methods, results, discussion etc. but in each of these certain methods are repetitive. For example 4.1, 4.2. are part of the results section but there are paragraphs that still tell what was plotted w/o giving results.

1. First of all we would like to thank you for this constructive and precise review, even though you were not appointed an official referee. We acknowledge that it is a long paper. The repetitive parts you are referring to, were first incorporated into the paper to make it easier for readers to start reading at a specific paragraph of the results section but we agree that its redundant information and makes it harder to read through the whole paper, so we tried to cut the unnecessary information in the relevant paragraphs

In the discussion of the model results quantitative details were presented. Given the model was not calibrated for the study watersheds, I wonder if those details matter or realistic at all. This study could lead to more qualitative results that can be summarized/discussed in a conceptual model. But in order to do that the number of simulations done may not be sufficient.

2. That is a good point. We agreed on giving the quantitative results because, while the model was not exactly calibrated to the different areas, the result of the model runs were verified by comparing the main trend of topographic metrics produced by the model for the different areas with the metrics obtained by DEM-analysis. We added a better explanation of this in 2.1

My main concern is that the results presented here for constant and transient changes individually P and V and combined, is only one potential outcome of a wider range of responses. The paper generally does not ask the "why" question in presenting the various results, but rather literally reports the model results in terms of modeled erosion rates etc..
I wonder if the authors can think of presenting a conceptual model to explain and summarize the various model results.

3. We agree with you that the presented P/V - scenarios are only one possible set of outcomes this model can produce. We added a paragraph in the discussion section in reference to a paper from Owen et al. (2010) which we hope helps understanding the general concept we also added another figure which conceptually shows the processes that we think, control the system.

What was the basis of using 10% and 70% V in the model simulations. I might have missed it.

4. The basis for using these vegetation-cover values was the idea of creating results for areas that represent end-member states for vegetation-cover and climate. Because we wanted to conduct the same simulation experiments for all areas it was not possible to choose the southernmost area of Nahuelbuta because its surface vegetation cover value is nearly at 100% and we couldn't add to that for the step-change and oscillation-simulations.

It sound like for a given mean annual P, you need a mean annual V for the sensitivity analysis of the model.

5. Correct. Observed vegetation/precipitation conditions in Chile do indeed demonstrate a non-linear relationship between mean P and V. This is shown in a figure in the paper. These mean P and V relationships are also present in the Part I (companion) paper. Our use of observed and model supported P and V relationships for the 10 and 70% vegetation cover simulations is also why we chose to set up the model results as 'loosely tuned' to Chile because needed to apply a relationship from somewhere. If we didn't do this, then reviewers would most likely ask what relevance the model selected parameters have to reality. We've modified the text in the methods section to hopefully make this clearer.

The parameter selection was not sufficiently developed. The Manning's roughenss for bare soil is very low for overland flow. What was nV. Kv is very sensitive to n, and keeping n as low as reported in the paper will increase the sensitivity of Kv to low values of V..

6. Thanks for catching this error in the manuscript, the said Mannings' roughness of 0.01 is wrongly reported as Mannings number for fully vegetated conditions. We used 0.01 as Mannings number for bare soil and 0.6 as Mannings number for fully vegetated conditions. fully vegetated conditions were assumed to be the case at V = 1 (100%).
While we realize that 0.01 is a number at the low-end of the spectrum of Mannings numbers, we choose this value because it lies between values reported by Chow (1958) for concrete channels and very hard soils, which we found a feasible assumption for the only unvegetated, granitic bedrock dominated area of Pan de Azucar. We changed table 1 to report the complete and correct values.

The interplay between vegetation and precipitation on erosion rates and landscape evolution is the relatively novel aspect of this paper. I don't think this was discussed in earlier papers, especially by separating V and P scenarios and then combining them based on the dependence between V and P. The one comment I have on the reconstruction of P and V is that, the paper relates P to V as far as I can tell (Fig. 5). I have a hard time rationalizing this as clearly V responds to P, rather than the opposite.

7. This is a good point. We decided that we wanted to tackle the problem from the side of vegetation-modulation and adjust the climatic parameters to certain imposed changes of vegetation cover. We had initial results from a dynamic vegetation model (LPJGUESS) which suggested a 10% change of vegetation cover in those areas since the last glacial maximum, which was the basis for using a 10% transient change in vegetation cover. However, please note that our selection of P and V values for simulations is based on present day observed relationship (empirical) from Chile. Thus,

we are implementing what is currently the case in Chile, rather than assuming some functional relationship between the two.

I wonder if the results, especially for the last case where a complex response as observed, would be any different if V was predicted from P, and P oscillated using a sin function.

8. Again, we like to point out that the paper presents only a suit of possible results. As referred to in the paper, the outcome of the transient simulations is a complex interplay between the initial vegetation cover/climate state of a model-domain and the imposed change in climate and vegetation cover. If we would have chosen to start with fixed values of P and then chose the according vegetation cover, it would boil down to: 1. the magnitude of changes in P that were chosen for the simulations and 2. the transient conditions would resemble a symmetric distribution of dP for step-changes and oscillations and a non-symmetric distribution of dV which would of course influence the results in a way that it would be another possible set of results.

The paper is long. The authors report details about model results as V and P varied. Details such as the rate of erosion etc can be omitted as these are apparent in the plots and in a theoretical study that does not claim to represent a certain region the exact rates of erosion would probably not interest the reader. I suggest focusing more on the conceptual findings of the paper.

9. We have tried to shorten the paper as much as possible. While we agree that the absolute values of rates of erosion may not be applicable to comparison to field-data, due to a possible offset in steady-state rock uplift rates, we think that there is value in reporting these rates and parameters in the text, because it makes the paper easier to read, for readers who may be only interested in a selected few of the results section and the text helps understanding the figures. Furthermore, if we did not report values for change, then reviewers would ask us to quantify statements such as “large increase”, or “small change”. So, we’ve tried to streamline the text as much as possible, but have left some values in to hopefully keep readers with diverse expectations for writing style happy.

To that end, however the model cases considered seems to be limited. The paper describes very interesting responses of erosion and landscape evolution driven by changes in vegetation and precipitation, separately and in combination. However given this theory it would be important to discuss when these cases occur.. For example, given the complex response presented in the last scenario, I wonder how plausible is the modeled complex response, are there any observational evidence on this in super arid regions?

10. As the reviewer notes, a key aim of the study is documenting the effects of precipitation and vegetation change on denudation. As these two factors (P and V) always change together we present an individual analysis of each so their relative contributions can quantified. There are no observations available to test the step change or sinusoidal change of P or V independently, but our modeling provides a means to understand the relative contributions of each. The manuscript currently emphasizes the reasoning for exploring P and V changes with a systematic increase in model complexity. For the coupled experiments presented at the end of the paper, the combined effects of P and V changes are shown. The timescale of the changes investigated is for 100 kyr variations in P and V. To the best of our knowledge, there are as of

yet no observations available with sufficient temporal resolution to compare model results to. Type of data that would be needed are a series of terrace or lake deposits that are well dated and contain paleo denudation rate information. While temporal variations in denudation rates are document in different places around the world (e.g. Marshall et al. 2015; Schaller et al. 2001; Schaller et al. 2016) few of these document vegetation, precipitation, and denudation rates over the timescales investigated here.
To address this comment: We have added text similar to the above and the previous references to section 5.5. We also highlight that this study provides a testable hypothesis for future observational studies to consider testing.

Important limitation to realize is that vegetation is spatially uniform, and it does not have seasonality in response to radiation and weather. Rainfall is also seems to be steady state although it was not mentioned in the paper. Such variability, if included, can effectively lead to crossing of erosional thresholds with certain frequency. The reason I'm bringing this up is that the model shows a muted response to Vegetation oscillations for V=70%. While this is high veg cover and the variability may be less in the erosional response, however absence of seasonal veg dynamics and stochastic rainfall and vegetation loss due to scour might play a strong role in stabilizing the land. If these above mentioned processes were used, with steeper equilibrium slopes under V=70%, the model would have responded in some episodic fashion.

11. This is an excellent point - and we partially agree. To address how seasonality influences denudation rates would be a separate study on it's own. You are right that we decided to use a steady-state rainfall approach without internal seasonality. We chose this as our starting point for looking at P and V interactions, but clearly future work on seasonality and stochastic precipitation effects need to be considered as well.
To address this comment: We will highlight the effects of seasonality and stochastic precipitation effects more in the paper as limitation of our approach and caveats that need addressing in the future. We agree that an event-driven climate within the model would introduce variability in the results but due to a lack of good data-proxies in this region to derive a high-resolution timeseries of climatic events would not lead to a gain in information from the paper. We acknowledge that your idea is to have a complete theoretical conceptual model paper and then this approach would be feasible but we would stay with our idea of using the Earthshape areas as proxys for climate and vegetation data within the model.

Line 69: Yetemen et al., 2015a, b are also exceptions to this statement as these papers presented daily water balance, runoff, distributed energy balance and evapotranspiration and transient vegetation growth.

12. Thanks for bringing that to our attention, we will modify the manuscript accordingly

Lines 160-162, a citation would be great here on the use of 100 kyr cycles.

13. We added a citation to build up on the 100 kyr cycle concept.

Lines 163 – 169: Were the results vegetation dynamics model results with cyclic climate not used.? Perhaps the actual data used to construct Fig 5 may be shown. I also noticed that in Fig 5 while Veg cover oscillates following what looks like a sinusoidal curve, the Precip data oscillate differently. Does lines 167-168 explain the reasons for this, which I was not sure if I understood correctly. For each 10% in veg cover you change the % in precip. But was this assumption concluded from Fig1,which would give us % V change for %P change within the ranges of P used in the model ? Given precip drives vegetation shouldn't logically Precip be following the sin curve rather than Veg, unless there is a better explanation and rational telling the reader why the curves were plotted differently. I could not find where 10% constant vegetation and near zero precip was mentioned. Some more details on the vegetation cover and the erosional history of the region would be good to include. For example are there any studies that quantified the erosion rates in the Holocene in this region.

14. The +/-10% vegetation cover change was determined by preliminary results from a dynamic vegetation model presented in the companion paper (Part I, Werner et al), but the direct output of the model results were not used in this paper because we wanted to reduce complexity. Actually Fig. 5 shows the actual data from Modis/Worldclim. We will highlight this more in the paper. Again we decided to let P follow V because we wanted the paper to focus on the effect of vegetation change and not changes in precipitation with changes in vegetation as a by-product. We also added citations which we think help with interpreting the complex erosion-rate results of the paper.

Equation 1: please change slope to curvature in the hillslope diffusion term. In the text below the equation kdS is correctly defined as flux but the equation does not use flux it uses the divergence of this flux that leads to change in elevation [L/T] and therefore curvature instead of slope is used. Also if you use curvature in this form the sign in front of the hillslope diffusion component of elevation change should be positive as slope would need another negative sign when represented by elevation change..

15. Thanks for bringing this to our attention, we will change the manuscript accordingly.

Equation 5, Is the E threshold used in the model experiments?

16. Yes, the erosional threshold was used. We added text to the manuscript to make this clearer

Equation 8 was given incomplete. It's missing the shear stress partitioning part that gives the drop of effective shear stress with V, which is correctly plotted in Fig 6.

17. Thanks again for noticing this. We changed the manuscript accordingly.

See equations 10 and 11 in the cited paper in this section. In this model how is rainfall incorporated.. The rate and duration of rainfall would influence the selection of the threshold to make the model results realistic.

18. As mentioned, we used a steady-state mean annual precipitation approach with no stochastic distribution. We will add some text to the method section to make this clearer.

Line 220: please tell what this steepness index was and provide citation. Did you extract the channel network to calculate this?

19. Yes – we assumed a threshold area for channelized conditions and extracted the channel network from our topographies with this. The steepness index was the normalized channel steepness index as for example reported in Wobus et al. 2006

Line 221: Fig 2 captions says 90 m DEM used.

20. Thanks for noticing this. We will change the figure caption.

Fig 7 d, e, f.. Lines 259.. In calculating channel steepness how were channels extracted? Here the authors make comparison between model predictions and the actual landscape.. Earlier the reader was informed that the model was not specifically calibrated for this region and the purpose of the study is to investigate the model sensitivity to V and P and compare the general trends between observations and models. Here the model is compared directly with actual data values from DEMs and the authors point out under and over estimations of the model.. Given that the model was not calibrated these statements undermine the strength of the paper. This model have enough parameters to calibrate with which the Fig 7 d,e,f can look a lot better.. This brings a few questions on model parameter selection. For example how was erodibility selected? I presume m and n exponents are also constant. Manning's coefficient for full vegetation is very low and unrealistic. This value is more smooth concrete channel value. Can the authors elaborate if any calibration at all was attempted? If the authors want to stress on the general patterns predicted by the model rather than a poor direct comparison between model and observations, they can report these figures in a non-dimensional form so that the amplitude of responses are compared with respect to 1 in both model and data. A simple way to non-dimensionalize would be to dive all the values with the mean (slope, relief etc.).

21. We will try to make the parameter selection clearer in the method-section of the paper. While the model was not parameterized to exactly fit the data from the Earthshape focus sites, we used the general behaviour of topographic metrics between the sites as a proxy for how we wanted the model to behave. Parameters where then chosen from published studies for the granitic lithology dominant in the focus sites and combined with vegetation cover data and climate data extracted from the Earthshape sites.

Fig 8 and 9 results make sense..

Fig 13. This figure shows that for the case of denser vegetation cover (V=0.7 or 70%) and larger P erosion increase with P in the similar way when V=10% (and smaller P), but as P gets smaller in the drier phase of the oscillation erosion does not drop as much as the less dense (and drier) simulation. Why the model gives this asymmetric response in E for given P oscillations (for drier and wetter P) for V=0.7 needs to be explained by the authors, as this is a very interesting result. Also why does the simulation with V=0.7 can double its erosion similar to V=0.1.? The mean erosion is higher than the case with V=10%. 10 cycles were plotted, given a total model year of 1M. I wonder why only the negative changes in P was dampened by vegetation in the denser V case but not the wetter cycles? This probably has to do with the way slopes adjusts under the two climate regimes. In both simulations you have a steady-state landscape as initial condition, and when P grows, is the erosion threshold surpassed in all locations resulting in a very similar erosion magnitude?. How long would the landscape need to attain a dynamic equilibrium under cyclic climate?

22. Thanks for thinking about these results and how to better explain them to readers. We will add some text to make this clearer.

Fig 15.V and P > or < V and P is not very informative.

23. We will make some adjustment to the figure in a more informative way.

Response to SC2

Responses in blue

This manuscript has a lot of value in terms of exploring the implications of some of our current best guesses with respect to how vegetation cover and precipitation modulate erosion rates at the Earth surface and influence topographic evolution over long timescales. While the investigation of the individual influences of precipitation and vegetation cover provide insights into the role of each, the dynamic behavior that arises through the combined modeling of both comprises a testable set of results, which could lead the way to improved equations in the case of clear mismatches. People in our community with a focus on details of individual processes and individual types of vegetation cover may find the generalization necessary for such models running over such long timespans to be an oversimplification, but I agree with the approach of first testing whether or not this simple approach can explain first-order observations with respect to erosion rates and landscape morphology.

Along these lines, I think the authors could expand their discussion somewhat to better emphasize how those of us from the field-data side might help to test these results. Testing the results of the model with respect to the morphology of the Andes is useful, but also has some broad limitations. For example, it's difficult to know if the topography that we see today is fully adjusted to forcing conditions such that it reflects the predicted influence of precipitation and vegetation cover, or if there could be a persisting transient response to tectonic activity.

1. Thank you for this productive and thoughtful comment. We agree, that it is hard to quantify if the modern-day topographies which are present in the Andes are in steady-state with the current forcings. We tried to rule out any tectonic transient leftover by picking basins which showed no remnant knickpoints in their river profiles. We agree that ruling out any other transient remnants of changes in vegetation and precipitation is hard, if not impossible to achieve, without additional field data. However we would like to point to publications from Mutz et al. 2018 and especially Schaller et al. 2018, which shows that the climate gradient the Coastal Cordillera has stayed relatively constant for long time periods and therefore we would argue that, if a transient signal of climate change is remaining in these basins, the general trend regarding the N-S gradient should still be comparable.

To address the reviewers comments we will add additional text in section 5.6

Calibrating the model to the conditions in the Andes could be problematic if the landscape is currently in a transient adjustment state. I wouldn't suggest that this issue rules out the possibility of comparing model predictions with topography, but it does point to a reason why there might be mismatches.

2. We fully agree with the reviewer with that the reason for mismatches between model setup and topographies could be due to not fully transient adjustments. As we mentioned in the paper, also a mismatch between the proposed uplift rates and the real uplift rates for the specific areas could be playing into this. Still we also think that its useful to try to reproduce a topographic trend seen in those catchments with our model setup to test for a correct sensitivity of precipitation and vegetation.

Another possibility for comparison would be to consider datasets that have recorded temporal variations in erosion rates in response to changes in precipitation and vegetation cover, particularly over the precipitation and vegetation cover ranges that the authors suggest drive the biggest changes. Field data that support the model results (particularly Figure 17) would be a strong argument in favor of the equations used. Unfortunately, those datasets are somewhat rare; the only ones I am aware of that report both vegetation cover and precipitation changes include Marshall et al. (2015, Science Advances) and Garcin et al., 2017 (EPSL), although Marshall et al. was mostly focused on frost-cracking, which would be less relevant for the model presented here. While I'm a co-author of the second and don't want to insist that the authors consider that study, I think it could be valuable, as we found quite strong variations in 10Be derived erosion rates reflecting the onset and persisting influence of the African Humid Period. An in-depth comparison of your model results to those data would clearly be out of the scope of the manuscript, but a qualitative comparison could be useful.

3. We agree that field data which supports our model results is unfortunately hard to to find. Owen et al. (2010) however discussed CRN-derived soil-production rates in the dry regions of Chile. If the assumption of a constant soil-column thickness holds true these soil production rates can be translated into denudation rates. In this paper in Fig. 12, the authors find that soil production rates for areas with a MAP between 2 mm and 10 mm show a huge variation with lowest values of 0.1 - 0.2 m/Ma which would translate into 0.0001 - 0.0002 mm/yr. They report these values to be situated into either a abiotic zone for 2mm MAP or a transition zone for 10 mm MAP. The maximum denudation rates they report in their dataset increase from 2m/Ma to 4m/Ma which shows that while the mean annual precipitation might increase by a factor 10, the denudation rates show no significant increase, moreover the minimum denudation rates for these climate conditions stay close to zero, which would support our argument that we see a drop to erosion rates of nearly zero despite an increase in precipitation. We argue that this dataset supports our model results by showing that an increase in precipitation from an abiotic regime to a "transitional" regime is not necessarily associated with increasing erosion rates but zero erosion rates could be plausible for this transitional regime.
To address the reviewer's comment we will add the the mentioned citation and and modify the discussion section to incorporate the above reasoning.

One aspect of the modeling results that surprised me is that for the 10% vegetation cover case, increasing precipitation leads to a decrease in erosion rates in the model. I find this somewhat counter-intuitive, and contrasts in some ways with what we see in East Africa, where the onset of wetter conditions leads to a brief spike in erosion rates, however the rates rapidly decreased once denser vegetation cover (trees, compared to a mix of grasses and trees earlier) established itself. So why the difference with your results? I'm guessing there's not much of a time lag in the model between increased precipitation and increased vegetation cover... if there were, perhaps the model would show a response more similar to what we see in our data. I'm unsure whether short short-term responses are important for landscape evolution models running on million-year timespans. But even if it's brief, a spike in erosion rates could have a reasonably good change of being preserved in stratigraphy, so there may be a good chance of recovering such responses with field data.

4. The reviewer is right in assuming that our model does not incorporate a lag-time between vegetation growth and changes in climate. Because of the different reactions of specific plant functional types to climate changes and general shifts in ecosystem inventories it is hard to quantify lag times of vegetation cover to shifts in precipitation. There are however ecological studies that tried to understand the lagtime of specific systems and plant types to changes in precipitation (e.g Fensham et al. 2005) and in general it can be concluded that these changes happen on timescales of 10 - 100 years. While we acknowledge that these adjustments are highly sensitive to the initial conditions of the systems and

that other disturbances like fire frequency etc. also play a role in the time which the ecological systems need to adjust, we argue that the lagtimes will still be in the same order of magnitude than our model timesteps (dt = 100yrs) and will therefore not change the outcome of the results. However we would fully agree that these short lagtimes could cause for a short burst of high erosion rates after onset of precipitation until the vegetation is adjusted, we only can't see it in the model without switching to smaller timesteps, which would be computationally efficient for the questions we hope to answer.

However, because we agree that the manuscript could be improved by linking our results to field data and other 'less theoretical' studies, we added a new subsection: "5.5 Potential Observational Approaches to Test Model Predictions**"**

Along the same lines, I wonder whether or not it would be possible to record (with field data) the high-frequency variations that the model shows during the declining-precipitation phase (again for the 10% veg cover case). Do you think we could we resolve a sudden, brief decrease in erosion rates with 10Be data? Two reasons why it could be tricky is that the thickness of any stratigraphic layer would be limited, and also at lower erosion rates, the erosion rates are integrated over longer timescales.

5. The reviewer brings up an interesting point about the applicability of field-methods to resolve the nearly-zero erosion intervals that our model predicts. We believe that in general it would be possible to resolve these phases with cosmogenic nuclides in a perfect case with a complete set of deposited undisturbed sediment-layers. However even for erosion rates of 0.1mm/yr, the integration time for 10Be would be around 6000yrs, which is longer than the zero-erosion interval our model predicts, so to exactly resolve this case will prove difficult.

This point may be worth discussing. Some additional minor points:

Section 4.1, lines 248, 255, 266: It would be helpful to use the term "spatially variable" in this section rather than "variable" alone, to make it clear that you are not focusing on temporal transients.

Thank you for this input. We believe that using the term "spatially variable" could mislead readers to think that we use a spatially variable vegetation cover within each model domain. However we agree that it could be more clear, so to address this we will change the text to make this point more clear.

Why isn't section 5.4 in the results section (section 4)?

We wanted to set-up the paper so that the coupled simulations that we ran could be thought of as a addition which helps to understand the previously set-up simulations with isolated transients forcings of either vegetation cover or mean annual precipitation. Our presentation of the coupled results in section 5.4 also provides a way of integrating the results, which if we removed it from the discussion section we would likely be asked to address by other reviewers (some of them already asked for more integration). Perhaps this is a difference in writing style, but we prefer to keep the fully coupled simulations in the discussion section to help manuscript integration.

Figure 16 is hard to interpret without Figure 17;it would be easier if the forcing (change in precip) were shown in Figure 16 or at least explained in the caption.

Thanks for this suggestions, we will add a more thorough explanation about the forcings in the figure caption for figure 15, for this figure is the first one that shows the results of the coupled simulations.

Are changes in vegetation cover modeled or prescribed?
I'm embarrassed that I was reading too quickly to discern that detail, but in any case, it would be helpful to have that information more prominent.

Changes in vegetation cover were prescribed and set to 10%. We added text in section 3.2 to make this clearer.

Effect of changing vegetation and precipitation on denudation (part 2): Predicted ~~L~~landscape response to transient climate and vegetation cover over millennial to million year timescales

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Abstract We present a numerical modeling investigation into the interactions between transient climate and vegetation cover with hillslope and detachment limited fluvial processes. Model simulations were designed to investigate emergent behavior in the effects of climate change and associated changes in surface vegetation cover on topographic basin metrics such as: slope, relief and channel steepness. The Landlab surface process model was modified~~used~~ to evaluate the effects of temporal variations in vegetation cover on hillslope diffusion and f~~detachment limited f~~luvial erosion. A suite of simulations were conducted to represent present-day climatic conditions and satellite-derived vegetation cover at the four EarthShape study areas as well hypothetical ~~transient long term~~ changes over millennial to million year timescales. Two different transient variations in climate and vegetation cover include a step change in climate or vegetation, as well as 100 kyr oscillations over 5 Myr. Results indicate that the coupled influence of surface vegetation cover and mean annual precipitation shifts basin landforms towards a new steady state, with the magnitude of change highly sensitive to the initial vegetation and climate conditions of the basin. Dry, non-vegetated basins show higher magnitudes of adjustment than basins that are situated in wetter conditions with higher vegetation cover. For coupled conditions when surface vegetation cover and mean annual precipitation change simultaneously, the landscape response tends to be weaker. When vegetation cover and mean annual precipitation change independently from each other, higher magnitude shifts in topographic metrics are predicted~~simulated~~. Changes in vegetation cover show a higher impact on topography for low initial surface cover values whereas for areas with high initial surface cover, the effect of changes in precipitation dominate the formation of landscapes. This study demonstrates a sensitivity of catchment characteristics to different transient forcings in vegetation cover and mean annual precipitation, with a crucial role for initial vegetation and climate conditions. ~~Ongoing research is developing fully-coupled landscape evolution and dynamic vegetation model (see companion paper) forced with predicted paleoclimate histories from an atmospheric general circulation model.~~

1. Introduction

Plants cover most of Earth's surface and interact chemically and physically with the atmosphere, lithosphere and hydrosphere. The abundance and distribution of plants throughout Earth's history is a function, amongst other things, of changing climate conditions that can impact the temporal distribution of plant functional types and vegetation cover present in an area (Hughes, 2000; Muhs et al., 2001; Walther et al. 2002). The physical feedbacks of vegetation on the Earth's near surface manifest themselves mainly through an influence of plants on weathering, erosion, transport and the deposition of sediments (Marston, 2010; Amundson et al., 2015). Although the effects of biota on surface processes has been recognized for over a 100 years (e.g., Gilbert, 1877; Langbein & Schumm, 1958), early studies focused mainly on qualitative descriptions of the underlying processes. With the rise of new techniques to quantify mass transport from the plot- to catchment-scale, and the emergence of improved computing techniques and landscape evolution models, research shifted more towards building a quantitative understanding of how biota influence both hillslope and fluvial processes (Stephan and Gutknecht, 2002; Roering et al., 2002; Marston, 2010; Curran and Hession, 2013). The previous studies motivate the companion papers presented here. In part 1 (Werner et al. 2018-this volume) a dynamic vegetation model is used to evaluate the magnitude of past (Last Glacial Maximum to present) vegetation change along the climate and ecological gradient in the Coastal Cordillera of Chile. Part 2 (this study) presents a sensitivity analysis of how transient climate and vegetation impact catchment denudation. This component is evaluated through implementation of transient vegetation effects for hillslopes and detachment limited rivers in a landscape evolution model. ~~Together, these two components provide a conceptual basis for understanding how transient climate and vegetation impact catchment denudation.~~

Previous research in agricultural engineering has focused on plot-scale models to predict total soil loss in response to land-use change (Zhou et al. 2006; Feng et al. 2010) or general changes in plant surface cover (Gyssels et al. 2005), but do not draw conclusions about large-scale geomorphic feedbacks active over longer (millennial) timescales and larger spatial scales. However, a better understanding of how vegetation influences the large scale topographic features (e.g. relief, hillslope angles, catchment denudation) is crucial to understanding the evolution of modern landscapes. At the catchment scale, observational studies have found a correlation between higher values of mean vegetation cover and basin wide denudation rates or topographic metrics (Jeffery et al., 2014, Sangireddy et al. 2016, Acosta et al. 2015). Parallel to the previous observational studies, numerical modeling experiments of the interactions between landscape erosion and surface vegetation cover have also made progress. For example, Collins et al. (2004) were one of the first who attempted to couple vegetation dynamics with a landscape evolution model and found that the introduction of plants to their model resulted in steeper equilibrium landscapes with a higher variability in magnitude of erosional events. Following this, subsequent modeling studies built upon the previous findings with more sophisticated formulations of vegetation-erosion interactions (Istanbulluoglu and Bras, 2005) including the influence of root strength on hillslopes (Vergani et al., 2017). These studies found that not only is there a positive relationship between vegetation cover and mean catchment slope and elevation but there also exists an inverse relationship between vegetation cover and drainage density, due to the plants ability to hinder fluvial erosion and channel initiation.

The advances of the previous studies are limited mainly by their consideration of static vegetation cover or very simple formulations of dynamic vegetation cover. The exception to this is Istanbulluoglu and Bras (2005) who also considered the lag time for vegetation regrowth on hillslopes after a mass wasting event and Yetemen et al. (2015) which considered more complex hydrology in their models but on a smaller spatial scale. However, numerous studies (Ledru et al., 1997; Allen and Breshears, 1998; Bachelet et al., 2003) document that vegetation cover changes in tandem with climate change over a range of timescales (decadal to million year). Missing from previous landscape evolution studies~~,~~ is consideration of not only how transient vegetation cover ~~influences catchment denudation,~~ but also how coeval changes in precipitation influence denudation. While the effects of climate change over geologic timescales on denudation rates and sediment transport dynamics have been investigated by others (e.g., Schaller et al., 2002; Dosetto et al., 2010; McPhillips et al. 2013), the combined effects of vegetation and climate change on catchment denudation have not. Thus, over longer (geologic) timescales, we are left with a complicated situation of both vegetation and climate changes, and the individual contributions of these changes to catchment scale denudation are difficult to disentangle.

In this study, we ~~compliment~~ previous works by investigating both the temporal and spatial sensitivities of landscapes to the coupled vegetation-climate system. By focusing on simplified transient forcings such as a step change, or 100 kyr oscillations in climate and vegetation cover we present a sensitivity analysis of the landscape response to each of these changes, including a better understanding of the direction, magnitude and rates of landscape change. Our model setup is motivated by four study-areas along the climate and vegetation gradient in Chile (Fig. 1a) and illuminates the transient catchment response to biotic vs. climate changes. These study areas are part of the recently initiated German priority research program *EarthShape: Earth surface shaping by biota* (www.earthshape.net). This region is used to provide a basis for our model setup for covariation in precipitation and vegetation present in a natural setting. While we present results representative of ~~four locations in~~ the Coastal Cordillera, Chile, it is beyond the scope of this study to provide a detailed calibration to this area and our main objective is identifying the sensitivity, and emergent behaviour, of catchment denudation to changing precipitation and vegetation cover over millennial timescales.~~,~~ ~~Furthermore, and save that as a focus of future (ongoing) work~~ in the ~~as numerous new data sets emerge from the Coastal Cordillera as part of the German EarthShape priority program (www.earthshape.net).~~ This study also builds upon results from the companion paper (Werner et al. 2018 - this volume) by imposing temporal variations in vegetation cover identified in that study.

2. Background to model setup

Model setup and the range of initial conditions chosen for models were based upon four study-areas ~~which are~~ located in the Coastal Cordillera of Chile ~~(with a latitudinal range from~~ 26ºS to 38ºS~~)~~. The focus areas shown in Fig. 1a ~~are part of the German EarthShape priority research program (www.earthshape.net) and~~ were chosen because of their similar granitic lithology and geologic and tectonic history (Andriessen and Reutter, 1994; McInnes et al., 1999; Juez-Larré et al., 2010; Maksaev and Zentilli, 1999; Avdievitch et al., 2017), and the large gradient in climate and vegetation cover over the region (Fig.1b,c). These study areas include (from north to south): Parque Nacional Pan de Azúcar; Reserva Santa Gracia; Parque Nacional La Campana, and Parque Nacional Nahuelbuta. Although this study does not explicitly present landscape evolution model results 'calibrated' to these specific areas, we've chosen ~~loosely tuned~~ the model input (e.g. precipitation, initial vegetation cover, rate of tectonic rock uplift) to represent these areas to provide simulation results that ~~would have some relationship to changing vegetation and climate conditions~~ represent the non-linear relationship between precipitation and vegetation cover (e.g. Fig. 1b, c) over a large climate gradient~~observed on Earth~~.

Topographic metrics such as mean basin slope, total basin relief, mean basin channel steepness, and mean surface vegetation cover and mean annual precipitation were extracted for the main catchments and a subset of adjacent catchments (Fig. 1~~a, 1b~~; Fig. 2). Topographic metrics were extracted from 30 m resolution digital elevation model from the NASA shuttle radar topography mission (SRTM), and vegetation related datasets from the moderate resolution imaging spectroradiometer (MODIS) satellite data (https://landcover.usgs.gov/green_veg.php).

2.1 Landscape evolution modeling approach and the applicability of these results

Landscape evolution model studies can be assigned to different general approaches, which were conceptually defined by Dietrich et al. ~~2013~~. Those approaches mostly differ on the degree of underlying details which were used to parameterize the model and the claim of reproducing certain aspects of landscapes on a temporal and spatial scale which heavily depends on the used approach (for details about the different approaches, see Dietrich et al., 2013). For this study we have chosen the approach of essential realism, which acknowledges a system-inherent indeterminacy in the evolving topography but focuses on predicting the first-order trends within a system and the differences between landscapes, based on different external conditions, incorporated in the model (Howard, 1997).

While we do not claim to reproduce the topographic metrics of the four different focus areas in Chile on a realistic level, our approach determines the general first-order effects of millennial timescale changes in precipitation and vegetation cover that can impact topography. Superimposed on the effects documented in this study would be the effects of seasonal changes in precipitation and vegetation cover, subcatchment variations in vegetation cover, transport limited fluvial and vegetation interactions, stochastic variations in precipitation in different climate zones. Consider of the previous, more detailed, aspects of precipitation-vegetation interactions on erosion could be independent studies of their own and can not be covered in a single study. Thus, the modeling approach and results of this study should be considered as documenting the longer (millennial) timescale climate and vegetation forcings on fluvial and surface processes.

3. Methods

3.1. Model Description and governing equations

For this study, we use the open-source model framework Landlab (Hobley et al., 2017).~~, which provides easily accessible methods for building a landscape evolution model. Landlab provides the computational environment to build an experimental set-up to test hypotheses and conduct sensitivity analyses of topography to different surface process parameterizations.~~

~~For our study, w~~We chose a model-domain with an~~representative of each of the four Chilean areas using an~~ area of 100km^2 which is implemented as a rectangular grid, divided into 0.01km^2 ~~spaced~~ grid cells. For simplification in the presentation of results, we present our results for the driest, northern most area (Parque Nacional Pan de Azucar) and for the Parque Nacional La Campana which is situated at 32°S Latitude (Fig.1) and shows the highest values in analyzed basin metrics (Fig. 2), although the general behavior and results presented here are representative of the other two areas not shown. The topographic evolution of the landscape is a result of tectonic uplift and surface processes, incorporating detachment limited fluvial erosion and linear diffusive transport of sediment across hillslopes (Fig.3). These processes are linked to, and vary in their effectiveness due to surface vegetation density. Details of the implementation of these processes into Landlab are explained in the following subsections.

This model setup is simplified in regards to hydrological parameters such as~~like, for example,~~ soil moisture and groundwater and unsaturated zone flow. Also, the erosion and transport of material due to mass-wasting processes such as rockfalls and landslides are not considered. We argue that those processes do not play a major role in the basins we used for model-calibration and that the processes acting continuously along hillslopes and channels have the largest impact on shaping our reference landscapes. Additional caveats and limitations of the modeling approach used are discussed in Section 5.4. Main model parameters used in the model (and described below) are provided in Table 1.

3.2. Boundary and Initial Conditions, and Model Free Parameters

In an effort to keep simulations ~~for the different Earthshape areas~~ comparable, we minimized the differences in parameters between simulations. The exceptions to this include the surface vegetation cover and mean annual precipitation, which were varied between simulations. One of the main controls on topography is the rock uplift rate. We kept the rock uplift rate temporally and spatially uniform across the domain ~~and~~ at 0.2 mm/yr (Table 1). Studies of the exhumation and rock uplift history of the Coastal Cordillera, Chile, are sparse at the latitudes investigated here, but existing and in progress studies further to the north are broadly consistent with the rock uplift rate used here (Juez-Larré et al., 2010; Avdievitch et al., 2017). ~~Furthermore, the thermochronometer cooling ages in the northern Coastal Cordillera suggest constant Cenozoic exhumation over >50 Ma at this rate. Thus, despite being located on an active plate boundary, existing observations suggest relatively slow, and temporally constant rock uplift of this region.~~

~~Due to the assumption that t~~The EarthShape focus sites are situated in similar granitic lithologies (Oeser et al., 2018), thereby allowing the assumption that the same critical shear-stress, baseline diffusivity, and fluvial erodibility can be used.

Vegetation cover was chosen to be spatially uniform across model domains. While vegetation can change in high-relief catchments due to precipitation and temperature changes with elevation, this simplifying assumption was made based on the low to moderate relief (500-1500m, mean ~750m) of the Coastal Cordillera areas investigated, and minimal field and MODIS observed changes in type and cover with elevation. The exception to this La Campana study area (~1,500 m relief) which has an observed change in vegetation type and cover in the upper 500 m of the catchment. Furthermore, dynamic vegetation modeling results presented in the companion paper to this (Fig. 5b in Werner et al., 2018 - this volume) indicate that although elevation gradients in plant functional types occur in the region since the last glacial maximum, the elevation range of the catchments in the Coastal Cordillera (<1500 m) exhibits only minor changes with elevation. Vegetation cover near trunk streams within catchments is observed in the field to increase, most likely due to local scale hydrology and more abundant water in these areas. However, these regions are often restricted to with 10's of meters of the trunk stream, well below the 100m grid resolution of the model, and therefore difficult to accurately resolve within the simulations presented.

~~While this certainly is not representative of a realistic setting, we choose this scenario because most simple vegetation models assume an simple decrease of vegetation cover by fluvial shear stress and a constant lapse-rate of temperature with elevation. While the second assumption is not verified for the focus areas, the removal of plants by stream-flow could not be observed in the field. In the vicinity of the main stream trunks of the catchment only a shift in the plant inventory was apparent, but no direct removal of plants.~~

The initial topography used in our simulations was a random white-noise topography with <1 m relief. To avoid unwanted transients related to the formation of this initial topography we conduct ~~equilibrium~~ simulations to produce an equilibrium topography for each set of the different climate and vegetation scenarios (see below). These equilibrium topographies were produced by ~~, and run~~ running the~~eachthe~~ model for 15 Myr until a topographic steady-state is reached. The equilibrium topography after 15 Myr was used as the input topography for subsequent experiments that impose transient forcings in climate, vegetation, or both (Fig. 4). The model simulation time shown in subsequent plots is the time since completion of this initial 15 Myr steady-state topography development. In the results section, we present these results starting with differences in the initial steady-state topographies (prior to imposing transient forcings) and then add different levels of complexity by imposing either: (1) a single transient step-change for the vegetation cover (Fig. 4b); (2) a step change in the mean annual precipitation (Fig. 4d); (3) 100 kyr oscillations in the vegetation cover (Fig. 4a); (4) 100 kyr oscillations in the mean annual precipitation (Fig. 4c); or (5) 100 kyr oscillation in both the vegetation cover and mean annual precipitation (both Fig. 4a and 4c). This approach was used to produce a stepwise increase in model complexity for evaluating the individual, and then combined, effects of fluvial and hillslope processes to different forcings.

The magnitude of induced rainfall transient forcings ~~where~~ based upon the present-day conditions along the Coastal Cordillera study areas (Fig. 1b, c). The step change and oscillations in vegetation cover and mean annual precipitation imposed on the experiments were designed to investigate vegetation and precipitation change effects on topography over the last ~0.9 Ma, the period during which a 100 kyr orbital forcing is dominant in Earth's climate (Broecker & van Donk, 1970; Muller and MacDonald, 1997)~~-~~. Given this timescale of interest, we impose a 10% magnitude change in the step-increase or decrease, or the amplitude change in oscillations for the vegetation cover. This magnitude of vegetation cover change is supported by dynamic vegetation modeling of vegetation changes over glacial-interglacial cycles in Chile (see companion paper by Werner et al. 2018-this journal) and to some degree elsewhere in the world (Allen et al, 2010, Prentice et al. 2011, Huntley et al. 2013), however for the sake of simplicity we use a fixed forcing of +/-10% for all simulations and not a spatially variable forcing which would be dependent on ecosystem behaviour for each separate area. We assume that the present-day conditions of combined vegetation cover and mean annual precipitation along the north-south gradient of the coastal cordillera are directly linked (Fig. 1b, c), and therefore follow an empirical approach based on the present day mean annual precipitation which directly links to the present day vegetation cover in Chile (Fig. 5). We do this by associating each 10% change in vegetation cover (dV) with a corresponding change of mean annual precipitation (dP, Fig. 5) present in the study areas considered. This approach, with a predefined, fixed change in vegetation cover and precipitation was chosen because the emphasis of this study lies on the effect of changing vegetation cover on topographic metrics. Thus, the changes in precipitation and vegetation imposed in this study are empirically based on observations from the climate and ecological gradient in the Coastal Cordillera.

The boundary conditions used in the model were the same for all simulations explained above (Fig. 3). One boundary was held at a fixed elevation and open to flow outside the domain. The other three were allowed to increase in elevation and had a zero-flux condition. This design for boundary conditions is similar to previous landscape evolution modeling studies (Istanbulluoglu and Bras, 2005) and provides a means for analyzing the effects of different vegetation cover and precipitation forcings on the individual catchment and subcatchment scale.

3.3. Vegetation Cover Dependent Geomorphic Transport Laws

The governing equation used for simulating topographic change in our experiments follows the continuity of mass. Changes in elevation at different points of the model domain over time dz(x,y,t) depend on

$$\frac{\delta z(x,y)}{\delta t} = U - \frac{\delta z}{\delta t}\Big|_{hillslope} - \frac{\delta z}{\delta t}\Big|_{fluvial} \quad (1)$$

where z is elevation, x, y are lateral distance, t is time, U is the rock uplift rate~~,~~ $\frac{\delta z}{\delta t}\big|_{hillslope}$ is the change in elevation due to hillslope processes, $\frac{\delta z}{\delta t}\big|_{fluvial}$ is the change in elevation due to fluvial processes~~, $k_d\nabla z$ the linear diffusive flux of sediment along hillslopes, and D_c the detachment capacity of channels~~ (Tucker et al., 2001a). ~~Our implementation of vegetation cover effects for the last two parameters in the Landlab model are described in more detail below.~~

3.3.1 Vegetation Cover Influenced Diffusive Hillslope Transport

The change in topography~~of topographic elevation~~ in a landscape over time ~~which is directly~~ caused by hillslope-dependent diffusion can be characterized as:

$$\frac{\delta z}{\delta t}\Big|_{hillslope} = -\nabla q_{sd} \tag{2}$$

Landscape evolution models characterize the flux of sediment q_s either as a linear or non-linear function of surface slope S (Culling, 1960; Fernandez and Dietrich, 1997). In order to keep the number of free parameters for the simulation to a minimum, we used the linear description of hillslope diffusion:

$$q_{sd} = K_d S \tag{3}$$

Following the approach of (~~Istanbulluoglu and Bras, 2005~~; Alberts et al., 1995; Dunne, 1996; Istanbulluoglu and Bras, 2005; Dunne et al., 2010), we assign the linear diffusion coefficient K_d as a function of surface vegetation density V, an exponential coefficient α, and a baseline diffusivity K_b, such that:

$$K_d = K_b e^{-(\alpha V)} \tag{4}$$

3.3.2 Vegetation Cover Influence on Overland Flow and Fluvial Erosion

Fluvial detachment-limited erosion of material due to water is calculated in this study by the widely-used stream-power-equation (Howard and Kerby, 1983; Howard et al., 1994 ;Whipple and Tucker, 1999; Braun and Willet, 2013):

$$\frac{\delta z}{\delta t}\Big|_{fluvial} = k_e(\tau - \tau_c)^p \; for \; \tau > \tau_c \tag{5}$$

In this equation k_e represents the erodibility of the bed, τ is the bed shear stress which acts on the surface at each node, τ_c is the critical shear stress which needs to be overcome to erode the bed-material and p is a constant.

By following the approach of Istanbulluoglu and Bras (2005) and Istanbulluoglu et al. (2004), we reformulate the standard equation of shear-stress $\tau_b = \rho_w gRS$, where ρ_w is the density of water, g is the acceleration of gravity, R is the hydraulic radius and S is the local slope, to a form which incorporates Manning's roughness to quantify the effect of vegetation cover on bed shear stress (Willgoose et al. , 1991, Istanbulluoglu et al., 2004):

$$\tau_v = \rho_w g (n_s + n_v)^{\frac{6}{10}} q^m S^n F_t \tag{6}$$

Here n_s and n_v represent Manning's numbers for bare soil and vegetated ground, q is the water-discharge per node which is approximated with the steady-state uniform precipitation P and the surface area per node A (q = A * P) and S is the local slope per node, m and n are constants. n_v for each node is calculated as a function of the local surface vegetation cover

$$n_v = n_{vr}\left(\frac{V}{V_r}\right)^w \tag{7}$$

with n_{vr} being the Manning's number for a defined reference vegetation cover, V and V_r being the vegetation cover at each node and the reference vegetation cover and w is an empirical scaling parameter.

The last variable in equation 6 represents the shear-stress partitioning ratio F_T (after Foster 1982; Istanbulluoglu and Bras, 2005), which is used to scale the shear-stress at each node to the vegetation-cover present.

$$F_t = \left(\frac{n_s}{n_s + n_v}\right)^{3/2} \tag{8}$$

By combining the formulation for shear stress out of equation 6 with the general stream-power equation 5 we formulate a new factor K_v which represent the bed erodibility per node as a function of surface vegetation cover, which leads to a new expression of fluvial erosion

$$K_v = k_e \rho_w g (n_s + n_v)^{\frac{6}{10}} F_t \qquad (9)$$

$$\frac{\delta z}{\delta t}\Big|_{fluvial} = K_v\, q^m S^n \qquad (10)$$

3.4 Model Evaluation

Model performance was evaluated using the above equations and different initial vegetation covers and mean annual precipitation~~ based on the steady-state predicted topography~~. Our focus in this study is on the general surface process response to different transient vegetation and climate conditions~~, rather than a calibrated modeling study of the Chilean study areas~~. Given this~~Nevertheless~~, topographic metrics of relief, mean slope, and normalized steepness index (K_{sn}) were computed from the model results and compared to observed values from the 30 m SRTM DEM for each of the four areas (Fig. 2). This was done to evaluate if our implementation of the governing equations in Section 3.4 produced topographies within reason of present day topographies in the four Chilean areas. A more detailed model calibration is beyond the scope of this study, and not meaningful without additional observational constraints on key parameters such latitudinal variations in the rock uplift rate and erosivity. Our aim is not to reproduce the present day topography of the Coastal Cordillera study areas but rather identify the sensitivity and emergent behaviour of vegetation-dependent surface ~~processes gradient~~processes gradient of vegetation cover and precipitation in Chile.

4. Results

Our presentation of results is structured around three groups of simulations. These include: 1. steady-state simulations where equilibrium topographies are calculated for different magnitudes of vegetation cover and identical precipitation forcing. A second set of steady-state simulations with the same magnitudes vegetation cover as 1. but with different precipitation forcings corresponding to each vegetation cover (Fig. 5, Section 4.1). 2. Simulations with a transient step-change in either surface vegetation density or precipitation (Section 4.2) that is initiated after the landscape has reached steady state. and 3. simulations with a transient 100 kyr oscillating time series of changing vegetation or precipitation that occurs after the landscape has reached steady state (Section 4.3). For each group of transient simulations, we show the topographic evolution with help of standard topographic metrics and the corresponding erosion rates after the induced change.

4.1 Equilibrium Topographic Metrics

Topographic metrics from each of the four Chilean focus areas (Fig. 1a) were extracted for comparison to equilibrium topographies predicted after 15 Myr of model simulation time. This comparison was done to document the model response to changing vegetation cover (with climate held constant) and changing vegetation cover and precipitation, and also to demonstrate the modeling approach employed throughout the rest of this study captures the general characteristics of different topographic metrics along the Chilean Coastal Cordillera. ~~We refrain from conducting a more detailed model-observation comparison for reasons previously mentioned.~~

Analysis of the digital elevation model for each of our four Chilean focus areas illustrates observed changes in catchment relief, slope, and channel steepness (K_{sn}) in relation to the surface vegetation (Fig. 7, red points) and latitude (Fig. 2). The general trend in the observed metrics shows a non-linear increase in each metric until a maximum is reached for regions with 70% vegetation cover. Following this, all observed metrics show a decline towards the area with 100% vegetation cover.

The model predicted equilibrium topographies (Fig. 7a,b,c) from four different steady-state simulations with ~~individual uniform but between simulations~~ ~~variable~~ vegetation cover in each simulation and~~but~~ a constant mean annual precipitation (900 mm/yr) show a nearly linear increase in all observed basin metrics with increasing vegetation cover and therefore do not reflect the overall trend observed ~~in the DEM~~ from the study areas (red line/symbols). For example, basin relief and slope are both under predicted for simulations with V < 100% (Fig.7a,b), and only the predicted maximum relief for a fully-vegetated simulation resembles the DEM maximum value. For the normalized channel steepness, only two observed mean values (for V = 10% and 70%) lie within the range of mean to maximum predicted Ksn values (Fig.7c).

The resulting equilibrium topographies from simulations with different~~both, for individual simulations, variable~~ mean annual precipitation and vegetation cover in each simulation (Fig.7d,e,f) show an improved representation of the general trend of the DEM data. The vegetation cover and precipitation values used in these simulations come from the ~~values observed in the~~ Chilean study areas (Fig. 1b, c; Fig. 5). In these simulations, the maximum in the observed basin metrics is situated at values of V = 30% with a following slight decrease in the metric for V = 30% to V = 70%, followed by a steeper decrease in metrics from V = 70% to V = 100%. Generally the model-based results tend to underestimate the basin relief and overestimate the basin channel steepness (Fig.7d,f). Variations in basin slope are captured for all but the non-vegetated state (Fig.7e).

Although the above comparison between the models and observations demonstrates a range of misfits between the two, there are several key points worth noting. First, the model results shown are ~~highly~~ simplified in their setup (e.g. assuming similar rock uplift rate, identical lithology and constants), and assume the present day topography is in steady state for the comparison. Second, despite the previous simplifying assumptions, the degree of misfit between the observations and model are surprisingly small when both variable vegetation and variable precipitation~~,~~ are considered (Fig. 7d,e,f). Finally ~~(third)~~, the general 'humped' shape curve observed in the Chilean areas is captured in the model predictions (Fig. 7d,e,f), with the notable exception that the maximum in observed values occurs at a higher vegetation cover (V = 70%) than the model predictions (V = 30%). Explanations for the possible source of these differences are revisited in the discussion section. ~~Without additional observations from the Chilean areas, reduction of the misfit between the observations and models is not tractable.~~

4.2 Transient Topography From a~~–~~ Step Change in Vegetation or Precipitation

The evolution of topographic metrics ~~our model topographies~~ after a induced instantaneous disturbance (Fig. 4) of either only the surface vegetation cover (Fig. 8, green lines) or ~~a step change in~~ only the mean annual precipitation (Fig. 8, blue lines) is analyzed for changes in topographic metrics for either a positive disturbance (Fig. 8a,b,c) or a negative disturbance (Fig.8d,e,f). This scenario ~~with changes in only vegetation or only precipitation~~ was chosen to ~~analyze sensitivity of drainage basins to changes in vegetation cover and precipitation and~~ isolate the effects of these specific transient forcings, and are useful for understanding more complex changes in vegetation and precipitation presented later. ~~Decoupling of vegetation and climate changes can occur via sudden disturbances, such as wildfires and lagged vegetation responses to climatic changes.~~ Mean catchment erosion rates are also analyzed for their evolution after the disturbance (Fig.9). For simplicity in presentation, results are shown for only two of the four Chilean studay areas with~~of the~~ initial vegetation (V) and precipitation (P) values for vegetation covers of 10 and 70%, and precipitation rates that correspond to these vegetation covers (i.e. P(V=10%) or P(V=70%)) ~~in the Chilean areas~~ (Fig. 5). The results described below show a general positive correlation between all observed topographic metrics and surface vegetation cover and a negative correlation between observed topographic metrics and mean annual precipitation~~ and therefore supports the data from our equilibrium topography simulations. The adjustment time until the system again reaches a new steady state varies between different simulations~~.

4.2.1 Positive Step Change in Vegetation Cover or Precipitation

Topographic Analysis

A positive step change in vegetation cover (V) from V = 10% to V = 20% (solid green line Fig. 8a,b,c) leads to a factor of 1.9, 1.42, and 2.1 change ~~n~~ change~~increase~~ in~~of~~ mean basin relief (from 270m to 520m), mean basin slope (11~~from 11~~.2° to 15.9°), and mean basin channel steepness (~~from~~ 108m$^{-0.9}$ to 222m$^{-0.9}$~~,~~), respectively. ~~which corresponds to a factor 1.9, 1.42, and 2.1 change, respectively.~~ The adjustment time until a new steady state in each metric is reached is 3.1Ma. The corresponding positive change in mean annual precipitation (solid blue lines, Fig. 8a,b,c) leads to a decrease of mean basin relief to 176m, mean basin slope to 8.6° and mean basin channel steepness to 67m$^{-0.9}$. This corresponds to a decrease by factors of 1.5, 1.2 and 1.6, respectively. The adjustment time to new steady state conditions in this case are shorter and 1.1Ma (Fig.8a,b,c). A second feature of these results is the brief increase and then decrease in basin average slope angles following the step change (Fig. 8b).

For simulations with V = 70% initial surface vegetation cover, a positive increase to V = 80% leads to an increase of mean basin relief from 418m to 474m, mean basin slope from 15.5° to 16.8° and mean basin channel steepness from 172m$^{-0.9}$ to 199m$^{-0.9}$. This causes an increase in each metric by factors of 1.1, 1.1 and 1.2, respectively. The adjustment time to steady-state conditions is 1.9Ma (dotted green lines, Fig 8a,b,c). The corresponding positive change in mean annual precipitation leads to a decrease of relief to 268m, decrease in slope to 11.9° and decrease of channel steepness to 105m$^{-0.9}$. This resembles a decrease by factors 1.5, 1.3, 1.6, respectively. Adjustment time in this case is 1.7Ma (dotted blue lines, Fig. 8a,b,c). The basin slope data shows similar behaviour as the V_{ini} = 10% simulations with an initial decrease and then increase for a vegetation cover step change and an initial increase and then decrease for a step change in mean annual precipitation. Comparison of the change in the topographic metrics for the low (V=10%) and high (V=70%) initial vegetation covers, the magnitude of change in each metric is larger when the step change occurs on a low, rather than higher, initial vegetation cover~~ topography~~.

Erosion Rate Changes

T~~Erosion rates show~~ an ~~instantaneous reactions to positive disturbances in vegetation or precipitation. Generally, t~~he model results show a negative relationship between increases in vegetation cover and erosion ~~rates~~ and a positive relationship between increases in precipitation and erosion~~ rates~~ (Fig. 9). Although the response~~reaction~~ between the disturbances and changes in erosion rates are instantaneous, the ~~specific~~ maximum or minimum in the change is reached after some lag time and the magnitude and duration of non-equilibrium erosion rates varies between different simulation setups.

For initial vegetation cover of V = 10%, a change in vegetation cover (dV) of +10% leads to a decrease in erosion rates from 0.2 to 0.03mm/yr (factor of 5.7 decrease, Fig. 9a green line). The minimum erosion rate is reached 43.5kyrs after the step change occurs. Following this minimum in erosion rates, the rates increase until the steady-state erosion rate is reached after the adjustment time. An increase in mean annual precipitation corresponding to a vegetation cover of 10% (i.e. P(V=10%) to P(V=20%); Fig. 5) leads to an increase in erosion rates to a maximum of 0.44mm/yr after 74.8kyrs (factor of 2.2 increase, Fig.9a, blue line). For initial vegetation of V = 70% a vegetation increase of dV = +10% results in minimum erosion rates of 0.14mm/yr after 117.7kyrs (factor of 1.4 decrease, Fig. 9b, green line). A corresponding increase in precipitation for these same vegetation conditions leads to maximum erosion rates of 0.44mm/yr after 107.5kyrs which is an increase by a factor of 2.2 (Fig.9b, blue line). The previous results for a positive step change in vegetation or precipitation demonstrate that the magnitude of change in erosion rates is larger for changes in precipitation rate than for vegetation cover changes, and in low initial vegetation cover settings (V=10%) the~~that~~ magnitude of change in erosion rates for changing vegetation is larger (compare green lines Fig. 9a with 9b).

4.2.2 Negative Step Change in Vegetation Cover or Precipitation

Topographic Analysis

For negative step-changes in vegetation (green curves, Fig. 8d,e,f), the results show a sharp decrease in topographic metrics associated with shorter adjustment times compared to the positive step change experiments (compare Fig. 8d,e,f with a,b,c). For step changes in precipitation (blue curves, Fig. 8d,e,f), the increase of topographic metrics happens slower and therefore with longer adjustment times. A negative step change in vegetation cover from V = 10% by dV = -10% leads to a decrease of mean basin relief from 269m to 35m, mean basin slope from 11.2° to 2.3° and mean basin channel steepness from 108m$^{-0.9}$ to 11m$^{-0.9}$ which resembles decreases by factors of 7.8, 3.8 and 9.6, respectively. The adjustment time until a new steady-state is reached is 0.26Ma (solid green lines, Fig. 8d,e,f). The corresponding negative change in precipitation leads to an increase in mean basin relief to 512m, mean basin slope to 15.8° and mean basin channel steepness to 223m$^{-0.9}$. These increases reflect changes by factors of 1.9, 1.4 and 2.1 with an adjustment time of 4.9Ma (dotted green lines, Fig.8d,e,f). Mean basin slope results (Fig. 8e) for a step change in vegetation illustrate a pulse-like feature of initially increasing slope values, followed by a decrease to lower slope values. In contrast, a negative step change in precipitation induce an initial decrease in slope, followed by a gradual increase in slope to a value higher than was initially observed before the change.

Simulations with initial vegetation cover V = 70% and dV = -10% show a decrease in mean basin relief from 418m to 356m, mean basin slope from 15.5° to 13.6° and mean basin channel steepness from 172m$^{-0.9}$ to 144m$^{-0.9}$ which resembles changes by factors of 1.2, 1.1 and 1.2 and an adjustment time of 2.1Ma (dotted green lines, Fig.8d,e,f). Corresponding negative changes in precipitation lead to increase of basin relief of 465m, basin slope to 16.4° and channel steepness to 195m$^{-0.9}$ which resembles changes by factors of 1.1 for all three values. Adjustment time in this case is 2.2Ma (dotted blue lines, Fig.8d,e,f). Behavior of mean basin slope after the step-change follows the V = 10% simulations but shows lower amplitudes of basin slope for both step-changes in vegetation and precipitation.

Erosion Rates

The positive step-change results (Fig. 9a, b) indicated that erosion rates reach their minimum or maximum with~~after~~ a lag time~~ after the change~~, and show significant differences in the magnitude and duration of non-equilibrium conditions depending ~~on if~~ vegetation or precipitation were changing. Simulations with a decrease from V = 10% to V = 0% (Fig. 9c) show a sudden increase in erosion rates to a maximum value of 3.5mm/yr which is an increase from steady state conditions by a factor of 17.7 which is reached after 19.5kyrs (green line, Fig.9c). A step decrease in precipitation for this corresponding vegetation difference (i.e. P(V=10%) to P(V=0%) leads to a smaller, and protracted (longer adjustment time) decrease in erosion rates to 0.03mm/yr after 50.1kyrs. These conditions cause a factor of 5.6 decrease (blue line,

Fig. 9c). Simulations of V = 70% with a vegetation change of dV = -10% show an increase in erosion rates to 0.27mm/yr which is a factor of 1.4 increase after 126.3kyrs (Fig. 9d). For the corresponding decrease in precipitation the data show a decrease in erosion rates to 0.15mm/yr after 124.5kyrs. This resembles change by factor of 1.2 (blue line, Fig. 9d).

4.3 Transient Topography – Oscillating

~~In addition to simulations where a transient step-change in either surface vegetation density or mean annual precipitation was conducted, we set up two distinct sets of simulations with an oscillating transient signal with a period of 100kyrs. This period resembles the eccentricity driven part of the Milankovitch cycle that is dominant in Earth's climate over the last 0.9Ma.~~

4.3.1 Oscillating Surface Vegetation Cover, Constant Precipitation

Topographic Analysis

The topographic evolution in simulations with a constant precipitation (10 and 360mm/yr for V=10%, and V=70%, respectively) and oscillating vegetation cover show a different response than the previous step change experiments. The differences depend on the initial steady-state vegetation cover prior to the onset of 100kyr oscillations. All observed basin metrics (Fig. 10) show an initial oscillating decrease in values until a new dynamic steady-state is reached where the amplitude in oscillations is less than in the preceding initial adjustment period. Simulations with V = 10% (Fig. 10a) show a factor of 2.5 decline in the basin relief (from 269m to 107m)~~ which resembles a decrease of the mean elevation of factor 2.5~~. ~~The positive amplitude of oscillation is 9m, the negative amplitude is 8.3m but time intervals of negative amplitudes are longer compared to positive amplitudes.~~ For simulations with V = 70% the reaction and adjustment to the new dynamic steady-state is less pronounced with a factor of 1.01 decline in relief (from 410m to 407m) ~~(Factor 1.01)~~ with positive and negative amplitudes in dynamic steady-state of 1.6m. Analysis of mean basin slope for the model topographies with low (V=10%) vegetation shows a similar behavior with a factor of 1.6 decrease of the mean slope (from 11.2° ~~(~~prior to the onset of oscillations,~~)~~ to 6.0°~~ (Factor 1.6~~, Fig. 10b). However, before this new equilibrium is reached, the slopes show an increase in mean slope for the first two periods of vegetation oscillation which then declines towards the new long-term stable dynamic equilibrium which is reached after approximately 500kyrs. Local maxima of mean basin slope coincide with local minima in basin relief. For the V = 70% simulations, the reaction is significantly smaller with no change in mean slope for the new dynamic equilibrium and amplitudes of both positive and negative of 0.16°. Mean basin channel steepness (Fig. 10c) reflects the behavior of mean basin elevation. For V = 10% simulations the mean channel steepness decreases by a factor of 2.7 (from ~~values of~~ 108m$^{-0.9}$ to 40m$^{-0.9}$~~ (factor 2.7 change~~) with a positive amplitude of 3.7m$^{-0.9}$ and a negative amplitude of 6.1m$^{-0.9}$. For V = 70% simulations the response is again only minor, compared to the lower initial vegetation cover simulations with a change of mean channel steepness from 186m$^{-0.9}$ to 167m$^{-0.9}$ and positive amplitudes of 1.1m$^{-0.9}$ and negative amplitudes of 0.9m$^{-0.9}$. Like the elevation data, the steepness data shows a distinct oscillating pattern with a slow increase~~s~~ to local maxima and rapid decreases to local minima which coincide with maxima/minima of elevation data. Taken together, the previous observations demonstrate a larger change in topography for oscillations in poorly vegetated areas compared to those with higher vegetation cover. Furthermore, the magnitude of topographic change that oscillations in vegetation impose on topography are largest in the first ~500 kyr after the onset of an oscillation, and diminish thereafter.

Erosion Rates

The erosion history for simulations with oscillating vegetation cover (Fig. 11) demonstrate large variations in the erosion rate that depend on the average vegetation cover of the oscillation. Furthermore, pronounced differences in the amplitude of erosion occur if the vegetation cover is above or below the mean of the oscillation (Fig. 4a). More specifically, simulations with V = 10% show a pattern of a small decrease in erosion rates (from 0.2 to 0.03mm/yr) when vegetation cover increases above the mean cover, in contrast to a large increase in erosion rates (up to 3.3mm/yr) when vegetation cover decreases below the mean of the oscillation (Fig. 2, Fig. 11). Maximum erosion rates decline over multiple periods of oscillation until they reach a dynamic steady-state with maximum rates (~~of~~ 1.2mm/yr) at 760kyrs after the onset. Time periods of higher erosion rates (>0.2mm/yr) have a mean duration of 28kyrs, whereas periods of lower erosion rates (<0.2mm/yr) have a mean duration 72kyrs. For simulations with high vegetation cover (V = 70%) the maximum and minimum erosion rates are 0.28 and 0.15mm/yr, respectively. The magnitude of maximum and minimum erosion rate are not significantly time-dependent and are reached at each local vegetation cover minimum. The mean duration of periods with higher erosion rates (> 0.2mm/yr) is 55kyrs whereas the duration for periods with lower rates (< 0.2mm/yr) is 45kyrs. These results demonstrate that areas with low vegetation cover experience not only larger amplitudes of change in erosion rates, but also an asymmetric change whereby decreases in erosion rates are lower magnitude than the increases in erosion rates.

4.3.2 Oscillating precipitation, Constant Vegetation

Topographic Analysis

The evolution of topographic parameters for simulations with oscillating mean annual precipitation and two different constant surface vegetation covers (V=10 or 70%, Fig. 12) show a less extreme and smaller temporal change in erosion rate to variations in precipitation compared to the previous discussed effects of oscillating vegetation cover (Fig. 11). In Fig. 12a, the mean basin relief results for V = 10% and oscillating precipitation show small variations (+4.9m to -3.8m) in relief around a mean of 269m, which is similar to the mean relief prior to the onset of oscillations at 5,000kyrs. For simulations with V = 70% the change in relief is slightly more pronounced with factor of 1.1 adjustment to a new mean (~~of~~ 380m from 418m) in steady-state conditions. ~~This change in mean relief equates to factor of 1.1 change.~~ The evolution of topographic slope (Fig. 12b) for V = 10% simulations shows a factor of 1.05~~n~~ adjustment to a new dynamic equilibrium (from 11.2° to 10.6° ~~(Factor 1.05)~~ ~~with a negative amplitude of 1.2° and positive amplitude of 0.9°~~. For V = 70% the mean slope values do not significantly change from steady-state to transient conditions ~~and the amplitudes of oscillation are 0.6° (positive) and 0.7° (negative)~~. Mean channel steepness (Fig. 12c) for V=10% shows a factor of 1.01 ~~n~~ adjustment (from 108m$^{-0.9}$ to 1[illegible]0m$^{-0.9}$ ~~(Factor 1.01~~). The amplitude of oscillation is 4 m$^{-0.9}$ for both negative and positive amplitudes. For V_{ini} = 70% simulations a factor 1.1 change in channel steepness occurs~~the topography adjusts~~ (from 171m$^{-0.9}$ to 152m$^{-0.9}$ ~~(Factor 1.1~~) with amplitudes of 4.5m$^{-0.9}$ for both positive and negative changes. Thus, although figure 12 illustrates changes in topographic metrics that result from oscillations in precipitation occurring around vegetation covers of 10 and 70%, these changes are significantly smaller than those predicted for constant precipitation, but oscillating vegetation conditions (Fig. 10).

Erosion Rates

Predicted erosion rates from simulations with constant surface vegetation cover and oscillating mean annual precipitation indicate different amplitudes of change around the mean erosion rate depending on the vegetation cover. For simulations with V = 10% (Fig. 13, blue solid line) erosion rates oscillate symmetrically around the steady-state erosion rate (~~of~~ 0.2mm/yr). The maximum and minimum erosion rates of 0.42 and 0.01mm/yr, respectively, result in no change in the mean rate. In contrast, predicted rates with a higher vegetation cover of V = 70% (Fig. 13, blue dotted line) demonstrate an asymmetric oscillation in rates around the mean, whereby the maximum ~~in~~ rate~~s~~ (0.43mm/yr) has a larger difference above the mean rate, tha~~e~~n do the minimums in the oscillation (0.12mm/yr). For this higher vegetation cover scenario, ~~a gradual increase in~~ the mean erosion rate ~~from 0.2 to 0.25 mm/yr as with time progresses~~ occurs~~is evident~~. Furthermore, the maximum and minimum erosion rates decline over several oscillation periods~~ to values of 0.38 and 0.15mm/yr, respectively~~. Taken together, these results indicate that oscillations in precipitation impact erosion with different magnitude depending on the amount of vegetation cover. Areas with low vegetation cover demonstrate the highest and symmetric oscillation of erosion rates due to changes in precipitation whereas in areas with high vegetation cover the effect of negative changes in precipitation is dampened by vegetation.

5. Discussion

The previous results highlight ~~predicted topographies with~~ different sensitivities to changes in either surface vegetation cover or mean annual precipitation. ~~The previous sSimulations, were conducted to isolate the magnitude of effect each parameter has on topography and erosion.~~ In the following, we synthesize the previous results and then build upon them to discuss the~~, over longer time-scales, more common scenario~~ of synchronous variation in both precipitation and vegetation cover.

5.1 Interpretation of Steady-State Simulations

Landscapes in a topographic steady-state show distinct features in topographic metrics that are widely used to estimate catchment-averaged erosion rates and therefore the leading processes of erosion within a landscape (DiBiase et al., 2010). ~~In most studies, focusing on comparing *in-situ* measured ^{10}Be erosion rates with topographic metrics, this is done in catchments with low variations in precipitation to focus on distinct topographic controls on soil erosion and transport processes. By conducting simulations with equal soil properties and assuming that the basic processes of sediment erosion and transport do not change between different climate settings we~~ The steady-state simulations presented can reproduce (Fig. 7) variations in topographic metrics over different climate and vegetation states~~s~~ seen in other studies (Langbein & Schumm, 1958, Walling and Webb 1983)~~ in steady-state landscapes with homogeneous erosion rates~~. Comparison of simulations with homogeneous precipitation and changing values of vegetation cover (Fig. 7a,b,c) to simulations with both changing precipitation and vegetation cover (Fig. 7d,e,f) indicates~~we see that~~ we can~~are only able to~~ only reproduce a similar trend with a distinct peak in topographic metrics when both variable precipitation and vegetation cover are considered. From this, we conclude that modern model-based landscape evolution studies that aim to compare areas with different climates should incorporate vegetation dynamics in their simulations. Misfits between the predicted and Chilean observed topographic metrics (Fig. 7d,e,f) present when the vegetation and precipitation both vary likely stem from the simplicity of the model setup used and the likelihood of differences of the rock uplift rate and l~~ithology's~~ present in these areas.

5.2 Interpretation of Step-Change Experiments

Our analysis shows that changes in vegetation-cover typically have a higher magnitude of impact on topographies for lower values of initial vegetation cover, compared to simulations with high initial vegetation cover (Fig. 8, 9). In those settings the influence of vegetation cover outweighs the influence of precipitation in cases of negative and positive directions of the step change. The reason~~s~~ for this is~~areis~~ due to a higher impact of changes in vegetation on erosivity and diffusivity (parameter K_v, K_d; equation 4, 10~~8~~) tha~~e~~n changes in precipitation~~ and therefore changes in runoff have on overall erosion values~~.

Furthermore, a negative step change in vegetation cover impacts the topographic metrics a factor of two more than do positive step change changes (Fig. 8d,e,f). This response is interpreted to be due to the non-linear reaction of diffusivity and fluvial erodibility to changes in vegetation cover (See Fig.6). Negative changes in vegetation cover lead to a higher overall change in diffusivity and erodibility that cause~~which leads to a higher sensitivity of equations 4 and 8 to negative step-changes~~ compared to positive step-changes.

Model results for the topographic metrics and erosion rates also indicate a difference in the adjustment times of the system until a new steady state is reached when either precipitation or vegetation cover changes (Figs. 8, 9). For simulations with positive step-changes (Fig. 8a,b,c) the adjustment time for changes in vegetation cover to reach a new equilibrium in topographic metrics or erosion rates is three times ~~higher~~ than the adjustment time for changes in precipitation. Simulations with a negative step-changes in vegetation cover show an adjustment time which is l~~ower~~ by a factor of 18 compared to negative changes in precipitation. This difference in adjustment time again is a result of the non-linear behavior of erosion parameters K_d and K_v which influence how effective a signal of increasing or decreasing erosion can travel through a river basin (Perron et al., 2012). High values of K_d and K_v are associated with lower adjustment times and are a result of negative changes in vegetation cover. The influence of changing precipitation on adjustment time behaves in a more linear fashion and therefore mostly depends on the overall magnitude of change. Therefore, positive step-changes in vegetation cover decrease K_d and K_v which leads to higher adjustment times than the corresponding changes in precipitation.

An increase and then decrease, or decrease and then increase, in predicted slope and erosion rates is observed for both the positive and negative step change~~s~~ experiments (Fig. 8b,e; and Fig. 9). This non-linear response in both positive and negative step changes in precipitation and vegetation cover is also manifested in the subsequent oscillation experiments, but most clearly identifiable in the step change experiments. The explanation for this behavior is as follows. A positive step change in vegetation cover (Fig. 8b) leads to a decrease in fluvial capacity because increased vegetation cover increases the Manning's roughness (parameter n_v, equation 8). The effect of changing the Manning's roughness varies with the location in the catchment and influences which processes (fluvial or hillslope) most strongly influence slopes and erosion rates. In the upper part of catchments where contributing areas (and discharge) are low, this increase in Manning's roughness causes many areas to be below threshold conditions such that fluvial erosion is less efficient, and hillslope diffusion increases in importance and reduces~~lowers~~ slopes. In the lower part of catchments, where contributing area and discharge are higher, changes in the Manning's roughness are not large enough to impact fluvial erosion because these areas remain at, or above, threshold conditions for erosion. With time, the lower regions of the catchments that are at or above threshold conditions propagate a wave of erosion up to the higher regions that are below threshold conditions. The propagating wave of erosion eventually leads to increase in slope angles, essential due to the response time of the fluvial network to adjust to new Manning's roughness conditions.

In contrast, a positive step change in mean annual precipitation leads to an initial increase in fluvial shear stress which initially causes headward incision of river channels and leads to wave of erosion that propagates upstream and increases channel slope values (Fig. 8b, see also e.g. Bonnet and Crave, 2003). The increase in channel slopes leads to an increase in the hillslope diffusive flux adjacent to the channels that then propagates upslope. Eventually, this increase in hillslope flux leads to a decrease in hillslope angles, and an overall reduction~~s~~ in mean catchment slopes after the systems reaches equilibrium.

Negative step-changes in vegetation cover or precipitation (Fig. 8e, green curves) shows the opposite behavior of the previous positive step change description. A negative step change in vegetation cover leads to an initial increase of fluvial erosion everywhere in the catchment because the Manning's roughness decreases everywhere. This catchment wide decrease in Manning's roughness leads to fluvial incision everywhere in the catchment and an increase in mean slope. However, eventually hillslope processes catch up with increased slopes near the channels and with time an overall reduction of slope occurs. Negative changes in precipitation (Fig. 8e, blue curves) lead to an initial decrease in fluvial erosion thereby leading~~which leads~~ to an increase in the significance of hillslope processes such that slope angles between channel and ridge decrease as hillslope processes fill in channels. With time, the fluvial network equilibrates to lower precipitation conditions by increasing slopes to maintain equilibrium between erosion and rock uplift rates.

Thus, the contrasting behavior of either initially increasing or decreasing slopes and erosion rates, followed by a change in the opposite direction of this initial change highlight a complicated vegetation-climate induced response to changes in either parameter. This non-linear behavior, and the timescales over which these changes occur, suggest that modern-systems that experienced past changes in climate and vegetation will likely be in a state of transience and the concept of a dynamic equilibrium in hillslope angles and erosion rates may be difficult to achieve in these natural systems.

Previous studies have inferred relationships between mean catchment erosion rates derived from cosmogenic radionuclides and topographic metrics (e.g., DiBiase 2010; DiBiase and Whipple 2011). However, the previous discussion of how topographic metrics change in response to variable precipitation and vegetation suggest that empirical relationships between erosion rates and topographic metrics contain a signal of climate and vegetation cover in the catchment. We illustrate the effect of step changes in climate and vegetation on the new steady-state of topographic metrics in figure 14. In this example, the new steady state conditions in basin relief and mean slope after a modest (+/-10%) change in vegetation or precipitation (triangles) differ from the initial steady-state condition (circles). These changes in topographic metrics when the new steady-state is achieved occur despite the rock uplift rate remaining constant. ~~Thus, differences in mean slope and relief can occur solely due to changes in climate or vegetation and are not necessarily linked to variations in erosion rate. The change in relief or slope is most pronounced for catchments with initially low (e.g. 10%) vegetation cover.~~

5.3 Interpretation of Oscillation Experiments

The results from the 100kyr oscillating vegetation and precipitation conditions shows that oscillating vegetation cover without the corresponding oscillations in precipitation leads to adjustments of topographic features, to a new dynamic equilibrium after approximately 1.5Ma (Figs. 10, 11). ~~The results indicate that the magnitude of adjustment depends on the initial vegetation cover, whereby simulations with 10% initial vegetation cover (solid lines, Fig. 10) show the largest changes from the initial (pre-oscillation) conditions to the new dynamic steady-state. Simulations with 70% initial vegetation cover (dashed lines, Fig. 10) show only minor adjustment to a new dynamic steady-state and lower amplitudes of oscillation. This is also represented by the mean basin erosion rates which show a significant peak for the first period~~

~~of oscillating vegetation cover with erosion rates being 16 times higher than steady-state erosion rates for simulations with 10% initial vegetation cover whereas the peak erosion rate for 70% vegetation cover simulations is only higher by a factor of 1.4 in the first period of oscillation.~~ The previously described response of topographic metrics and erosion rates to oscillating vegetation ~~(see results section)~~ are due to processes described in the previous step change experiments. For example, the asymmetric oscillations in topographic metrics for V=10% (Fig. 10) are due to the superposition of positive, then negative changes described in section 5.2. Variations in the imposed Manning's roughness, and relative strengths of fluvial vs hillslope processes in different parts of the catchments at different times causes the topographic metrics and erosion rates to have a variable amplitude and shape of response from the symmetric oscillations imposed on the topography (Fig. 4a).

Simulations with oscillating precipitation and constant vegetation cover however show a less pronounced shift to new equilibrium conditions and ~~in general~~ lower amplitudes of oscillation in both topographic metrics and erosion ~~rate~~ (Figs. 12, 13). This difference in the response of the topographic metrics and erosion rates shown in figures 12 and 13, compared to the oscillating vegetation cover experiments (Figs 10, 11), is due to a ~~generally~~ higher impact of changes in vegetation cover on parameters which guide erosion rates and therefore adjustment to topographic metrics~~,~~ compared to the \~~calibrated,~~ corresponding changes in precipitation in our model domains (Fig. 5). Especially for simulations with low initial vegetation cover the effect of changing vegetation has~~shows~~ a larger magnitude effect~~s~~ because of the non-linear response of diffusivity and fluvial erodibility to changes in vegetation cover compared to the linear response to changes in precipitation.

5.4 Coupled Oscillations in Both Vegetation and Precipitation

The previous sections present a sensitivity analysis of how step changes or oscillations in either vegetation cover or precipitation influence topography. Here we present a step-wise increase towards reality by investigating the topographic response to changes in both precipitation and climate at the same time. The amplitude of change prescribed for both precipitation and vegetation is based upon the present empirical relationship observed in the Chilean study areas for initial vegetation covers of 10 and 70%, and mean annual precipitations for 10 and 360mm/yr (Fig. 5). As with the previous experiments, oscillations in parameters were imposed upon steady-state topography that developed with the previous values, and a rock uplift rate of 0.2mm/yr.

Figure 15 shows the evolution of topographic metrics for simulations with combined oscillations in precipitation and vegetation. The variation in topographic metrics resembles those described for simulations with constant vegetation cover and oscillating climate by showing little to no significant adjustment towards new dynamic steady-state conditions. The amplitudes of oscillation are dampened from those of previous results because of the opposing effects of changes in precipitation and vegetation cover (e.g. compare blue and green curves in Figs. 8 and 9).

However, inspection of the predicted erosion rates (Fig. 16) for the combined oscillations indicates a significant (~0.1; -~0.15mm/yr), and highly non-linear response. The response between the 70% and 10% vegetation cover scenarios are very different such that for heavily vegetated areas (P(V=70%)) erosion rates typically increase during an oscillation, whereas for the low vegetation cover conditions (P(V=10%)) erosion rates initially show a decrease, and then an increase and decrease at a higher frequency.

To better understand this contrast in the response to combined precipitation and vegetation changes, the first 100 kyr cycle ~~of the imposed oscillation~~ is shown in figure 17. After an oscillation starts, the 10% initial vegetation cover simulations show a decline in erosion rates with the minimum erosion rate correlated with highest values of both vegetation cover and mean annual precipitation (compare top and bottom panels). This part of the response is interpreted as resulting from the hindering effect of increased vegetation on erosion rates outweighing the impact of higher values of precipitation on erosion rates (Fig. 17) due to vegetation increasing~~increases in~~ bed stability and shielding of the surface against rainfall. After values of vegetation cover and precipitation start to decline, erosion rates show a very rapid increase to values of ~0.3mm/yr. This increase in erosion rates is due to an increase in both K_v and K_d (Fig. 3b, equations 4, 5) which outcompetes the effect of precipitation decrease.

Following this, a sudden drop in erosion rates to 0mm/yr occurs and lasts for 3kyrs due to the onset of hyper arid conditions at minimum precipitation. After this low in erosion rates, they increase again (to 0.3mm/yr) as precipitation and vegetation cover increase while the effect of increased precipitation outweighs the effect of the non-linear decrease in K_v and K_d (Fig. 3b, c; equations 4, 5). Finally, at the end of this complex cycle a decrease in erosion rates occurs (Fig. 17b) while vegetation and precipitation are increasing (upper panel) because the effect of vegetation increases K_v/K_d and outweighs the effect of increasing precipitation.

Lastly, a clearly different behavior in erosion rates occurs for settings with higher vegetation cover (e.g. P(V=70%), Fig. 17) compared to the previous lower vegetation cover scenarios. As the vegetation cover and precipitation increase (Fig. 17A) in the first half of the 100kyr cycle, the erosion rates increase ~~to values~~ (to~~of approximately~~ 0.35mm/yr). This is due to the increase in precipitation which outcompetes the decline in vegetation influenced erosivity/diffusivity parameters K_d and K_v. Following this, when vegetation cover and precipitation decrease in the second half of the cycle, little to no change occurs in the erosion rates. This near static behavior in erosion rates while precipitation and vegetation cover decrease is due to an equilibrium between the negative effect on erosion rates for decreasing precipitation and the positive effect on erosion rates for decreasing vegetation cover.

In summary, the non-linear shape of the vegetation dependent erosivity (K_v) and hillslope diffusivity (K_d) in combination with linear effects of mean annual precipitation on erosion rates, exert a primary control on the direction and magnitude of change in catchment average erosion rates. Despite a simple oscillating behaviour in precipitation and vegetation cover, a complex and non-linear response in erosion rates occurs. In Fig. 18 we depicted the conceptual end-members of landscape behaviour for the different scenarios of increasing or decreasing vegetation cover and mean annual precipitation for different initial landscapes. The implications of this are large for observational studies of catchment average erosion rates and suggest that the direction and magnitude of response observed in a setting is highly dependent on the mean vegetation and precipitation conditions of the catchment, as well as what time the observations are made within the cycle of the varying vegetation and precipitation. Furthermore, these results highlight the need for future modeling studies (and motivation for our ongoing work), to investigate the response of catchment topography and erosion rates to more realistic climate and vegetation change scenarios, as well as a broader range of initial vegetation covers and precipitation rates than those explored here such that the threshold in behaviour between the two curves shown in figure 17b can be understood.

5.5 Potential Observational Approaches to Test Model Predictions

The behaviour discussed in the previous section matches field-data reported by Owen et al. (2010) who analysed soil production rates from bedrock in different climate regime. This data, under the assumption of steady-state soil thickness, can be translated into denudation rates. They show that for low values of mean annual precipitation, soil production rates vary between 0 m/Ma and 2 m/Ma due to abiotic processes controlling soil production rates. These observations resemble the effect of our simulations with 10% initial vegetation cover, which shows the same variations in erosion rates with intervals of zero erosion rate for hyper-arid conditions (Fig. 17). Areas with higher values of mean annual precipitation show higher values in the soil production rate. These data points were not corrected for different uplift rates in the sample areas so it is not possible to isolate the effect of vegetation/precipitation and tectonic uplift. In general the observations show no clear isolated trend but more of a cluster of soil production rates among a common mean, situated in a zone controlled by biotic conditions. Compared to our model data for simulations with 70% initial vegetation cover, this resembles the non-intuitive behaviour of an increase in erosion rate for increasing values of vegetation cover and precipitation compared to a constant erosion rate for decreasing values of vegetation cover and precipitation.

Schaller et al. (2018) and Oeser et al. (2018) present millennial timescale (cosmogenic radionuclide derived) hillslope denudation, and soil production, rates from the Chilean (EarthShape) study areas (Fig. 1A) considered in this study. They find the lowest hillslope denudation rates in the arid and poorly vegetated north. Moving south towards higher precipitation and vegetation cover the denudation rates increase until the southernmost location with highest rainfall and vegetation cover where denudation rates decrease again. This non-linear relationship of hillslope denudation rates with vegetation cover and precipitation is not directly comparable to the results presented here, but is consistent with a) the notion emphasized here that interactions between precipitation and vegetation cover on denudation are non-linear, and b) that the study areas considered here, although tectonically quiescent for tens of millions of years, have varying denudation rates that suggest either variable rock uplift rates, and/or a persistent state of transience in hillslope denudation induced by millennial timescale oscillations in climate and vegetation.

Beyond the previous studies, limited observations are available for comparison to the predictions shown here. The millenial to million year time scales investigated here can best be evaluated from observations of catchment wide denudation over similar timescales. Cosmogenic radionuclide measurements from modern river sediments offer one means to evaluate these results. Work by Acosta et al. (2015) in east Africa and Olen et al., (2016) in the Himalaya, are also consistent with the results presented here for the range of vegetation cover available in each of these areas. However, the integration time scales that these studies are sensitive to are shorter than what is presented here and prohibit a detailed comparison. A final approach that future studies could pursue is to calculate paleo denudation rate for catchments from a time series of sediments deposits preserved in either lakes (e.g. Marshall et al. 2015) or fluvial river terraces (e.g. Schaller and Ehlers, 2006; Schaller et al., 2016). However, to be most effective, these studies need to target multiple study areas with terrace or lake deposits that span a range of vegetation covers in the upstream catchments.

~~In summary, the non-linear shape of the vegetation dependent erosivity (K_v) and hillslope diffusivity (K_d) in combination with linear effects of mean annual precipitation on erosion rates, exert a primary control on the direction and magnitude of change in catchment average erosion rates. Despite a simple oscillating behavior in precipitation and vegetation cover, a complex and non-linear response in erosion rates occurs. The implications of this are large for observational studies of catchment average erosion rates and suggest that the direction and magnitude of response observed in a setting is highly dependent on the mean vegetation and precipitation conditions of the catchment, as well as what time the observations are made within the cycle of the varying vegetation and precipitation. Furthermore, these results highlight the need for future modeling studies (and motivation for our ongoing work), to investigate the response of catchment topography and erosion rates to more realistic climate and vegetation change scenarios, as well as a broader range of initial vegetation covers and precipitation rates than those explored here such that the threshold in behavior between the two curves shown in figure 17b can be understood.~~

~~This could be achieved by using simulation results from state-of-art dynamic vegetation models (e.g Smith et al. 2014, see also companion paper by Werner et al. 2018) as inputs into the landscape evolution model or by full coupling between~~

~~both model types. Vegetation simulations could, e.g. benefit from simulated changes in soil depth, which can crucially determine plant water stress, provided by the landscape evolution model. Coupling between Landlab and the dynamic regional to global vegetation model LPJ-GUESS (Smith et al. 2014, Werner et al. 2018) is envisioned.~~

5.6~~5~~ Model Restrictions and Caveats

Similar to ~~Like other~~ previous work on this topic (Collins et al. 2004, Istanbulluoglu and Bras 2005) ~~the~~ model setup used in this study was ~~intentionally~~ simplified to document how different vegetation and climate related factors impact topography over long (geologically relevant) timescales. We acknowledge that future model-studies should address some of the restrictions imposed by our approach to evaluate their significance for the results presented here. Future work should consider a transport-limited fluvial model or a fully-coupled alluvial sedimentation and transport model. The a~~A~~ddition of this could bring new understanding in to how vegetation not only influences detachment limited systems, but also influences sedimentation and entrainment of material. This added level of complexity could however limit (due to computational concerns) the temporal scales ~~over the investigation can be conducted.~~ Future studies could improve upon this work by considering a more in-depth parametrization~~parameterization~~ of how vegetation related processes (e.g. root depth and density, plant functional type) influence topographic metrics and erosion rates. Due to the long-timescales considered here, mean annual precipitation rather than a stochastic distribution of precipitation were~~was~~ implemented. Future work should evaluate how stochastic distributions in precipitation and extreme events in arid, poorly vegetation settings, impact these results, however the long timescale forcings in precipitation and vegetation imposed in this study will likely persit as the background template upon which high-frequency changes are active.

Regarding the vegetation and water-budget, a more sophisticated model of evapotranspiration and infiltration as a function ~~to~~ surface plant cover and plant functional traits such as rooting depth would improve model predictions and is a priority for our future work~~goal of future research within this project~~. Improvements will come from planned coupling of surface process model with the dynamic vegetation model LPJ-GUESS (Werner et al. 2018, this issue).

Our assumption that an increase in surface vegetation cover directly translates to an increase in Mannings roughness is an additional simplification. The real value of Mannings roughness of a surface will be a function of the fractional densities of different plant communities per model-patch. We argue that this simplification is however necessary because it is not possible to know the composition of the plant community for specific areas for our modeled timesca

We also acknowledge ~~here~~ that the transient forcings we have chosen for driving our model are simplistic and could be improved by a higher-fidelity time-series of climate over the last millennia. We choose a 100kyr, eccentricity driven, periodicity because it is widely recognized that the eccentricity cycles are a main control in driving Earth's glacial cycle over the last 0.9Ma. While this approach is reasonable for a sensitivity analysis such as we've conducted, it prohibits a detailed comparison to observations in specific study areas without additional refinement. Our results suggest that a shorter periodicity, which would resemble other periodicities in the Milankovitch cycle (e.g., 41kyrs, 23kyrs) or shorter time scale climate variations, such as Heinrich events (see Huntley et al. 2013) would lead to smaller magnitudes of adjustment to new dynamic equilibria, because of short time spans in high-/low-erosive climate conditions within one period. Regarding the long time-periods considered, we chose to have a steady-state climate driver in the model without frequency driven modulation of rainfall events. We argue that over large time scales the occurrence of these events can be integrated into a meaningful mean value but acknowledge that the incorporation of those events could alter the results on a short cycle-basis. However because there is no meaningful way to test these frequency distributions against past-climates, this would add additional unknowns and assumptions into our model parameterization.

6. Conclusions

The results from our ~~model-based~~ experiments ~~in comparison to observations from topographic analysis from four different areas in the Coastal Cordillera in Chile~~ show that the interactions of vegetation cover and mean annual precipitation on the evolution of landscapes is a complex system with competing effects. ~~Main conclusion~~ which emerge from this study are:

(I) vegetation cover ~~in general~~ has a hindering effect on hillslope and fluvial erosion~~eroding surface processes~~ but the magnitude on which changes in vegetation cover affect these processes is a function of the initial state of the system. Changes in systems with higher initial values of vegetation cover have a less pronounced effect than changes in systems with lower initial vegetation cover.

(II) In comparison to the Coastal Cordilleras of Chile, the relationship between precipitation and surface vegetation cover shows a distinct shape: For a 10% increase in surface vegetation cover, the corresponding increase in mean annual precipitation is smaller in areas of lower vegetation cover and increases for areas with higher vegetation cover. This has an effect on transient topographies by shifting the equilibrium of vegetation and precipitation effects on erosion rates.

(III) ~~Following~~ our step-change simulations, ~~our model results~~ show different behaviours for changes in vegetation-cover and mean annual precipitation. While increases in mean annual precipitation have an increasing effect on erosion rates and therefore a long-term negative effect on topographic metrics, an increase in vegetation cover hinders erosion, and leads to higher topographic metrics. The magnitude of these changes is again~~highly~~ dependent on the initial vegetation cover and precipitation before the step-change.

(IV) Simulations with either oscillating vegetation cover or oscillating precipitation show adjustments to new dynamic mean values around which the basin metrics oscillate. The magnitude of adjustment is highly sensitive to initial vegetation cover, where simulations with 10% initial cover show higher magnitudes than simulations with 70% cover, for oscillating vegetation. Oscillating precipitation leads to lower-/no adjustments but an oscillation of basin metrics around the initial mean values with generally lower amplitudes compared to simulations with oscillating vegetation cover.

(V) Simulations with coupled oscillations of both vegetation cover and precipitation show only small magnitudes of adjustments in~~of~~ topography metrics to new dynamic equilibrium~~s~~ similar to simulations with a oscillation in only precipitation. However corresponding erosion rates show a complex pattern of rapid increases and decreases which results from a interplay of competing effects of hindering of erosion by vegetation and aiding of erosion by precipitation.

Taken together, the above findings from this study highlight a ~~non-linear and~~ highly variable behavior in how variations in vegetation cover impact erosion and topographic properties. The complexity in how vegetation cover and precipitation changes influence topography demonstrates~~highlights~~ the need for future work to consider both of these factors in tandem, rather than singling out either parameter (vegetation cover or precipitation) to understand potential transients in topography.

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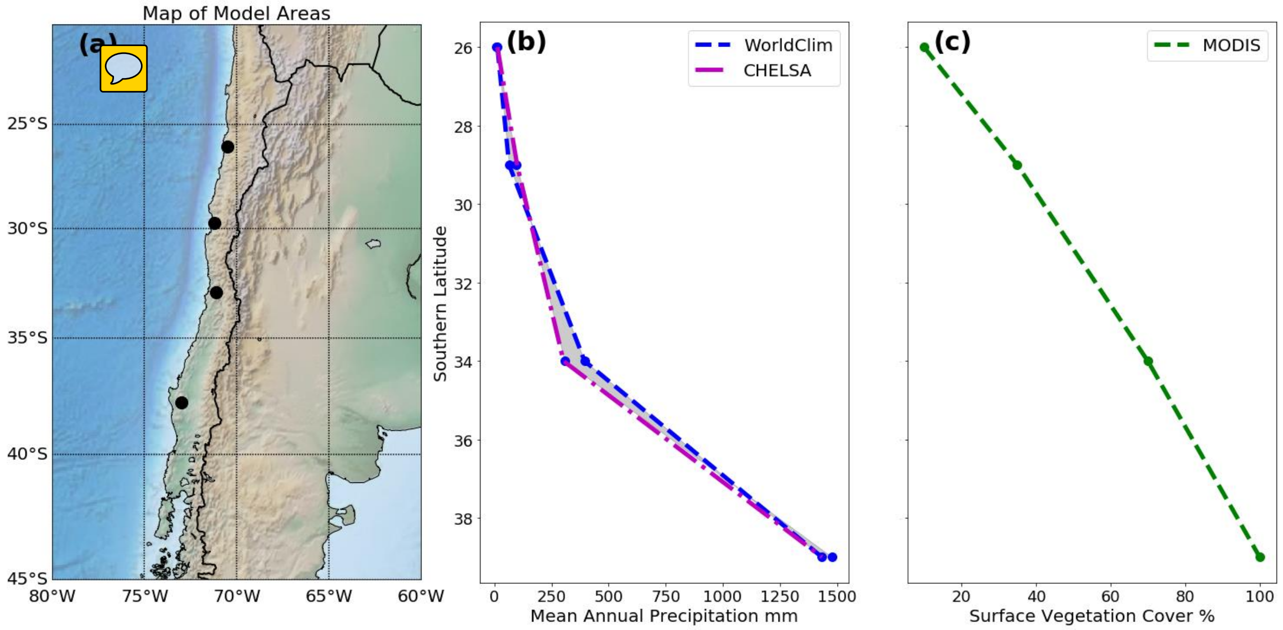


Figure 1 Overview of the geographic location, precipitation, and vegetation cover of the Coastal Cordillera, Chile studies areas used for model setup up. A) Digital topography of the areas considered and corresponding to the EarthShape (www.earthshape.net) focus areas where ongoing related research is located. B) Observed present day mean annual precipitation from the WorldClim and CHELSA datasets used as model input. B) Present day maximum surface vegetation cover from MODIS data.

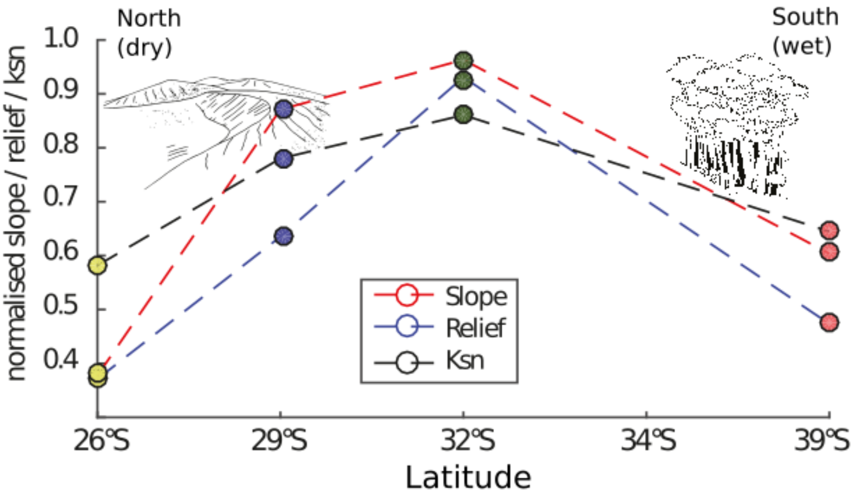
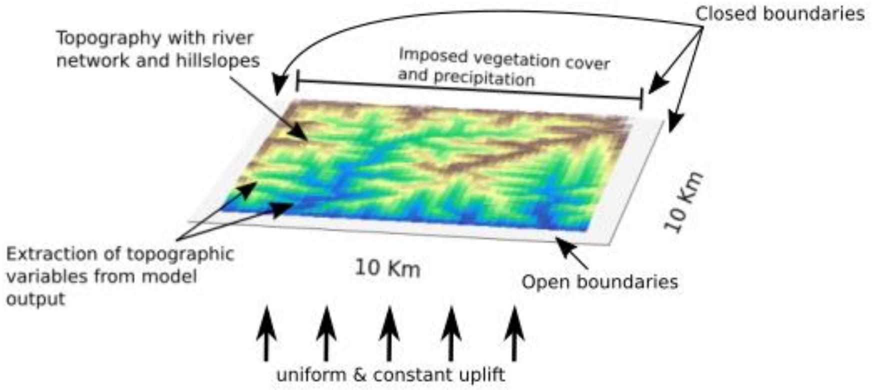


Figure 2 Normalized basin metrics for study-areas derived from 30 m SRTM digital topography from the study areas shown in Figure 1a. Colored d~~D~~ots represent cumulative mean values of normalized slope, relief and channel steepness calculated for all locations using 5-8 representative catchments in each area. Dotted lines represent linear interpolation between values. Note the gradual increase, then decrease in all metrics around study area at ~32°S.



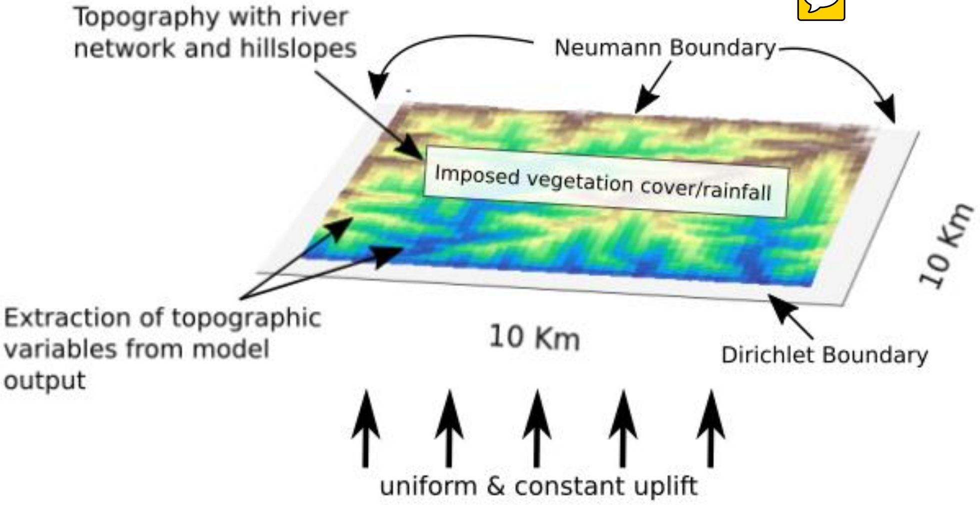


Figure 3 Example model setup used in simulations in this study. Figure shows an example model predicted topography~~example synthetical DEM~~ with a set drainage network, draining to the south. Boundary conditions and parameterizations used in the models are labeled. Blue colors represent low elevations, brown colors represent higher elevations. Additional details of parameters used are specified in Table 1.

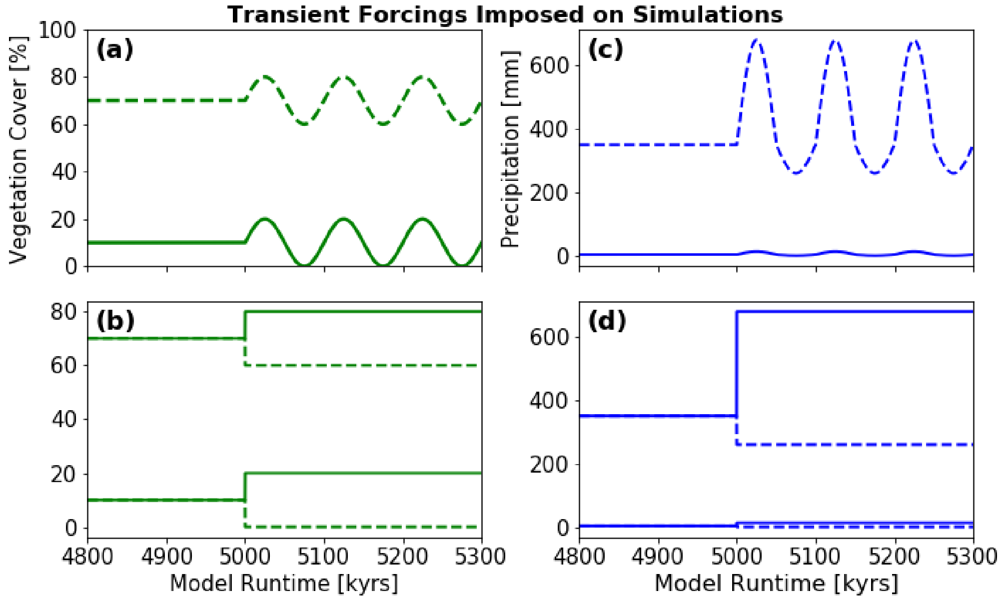


Figure 4 Transient forcings in vegetation and precipitation considered in model experiments. Simulations were run for 15 Myr prior to the runtime shown in the figure. All transients imposed started a runtime of 5 Myr. A) Variations in vegetation cover imposed in the oscillating experiment conditions for initial vegetation cover of 10 and 70%. <u>Oscillations have a 10% amplitude and a 100kyr periodicity.</u> B) <u>10% positive and negative step-changes in vegetation cover imposed on simulations with 10% and 70% initial vegetation cover.</u>~~Positive and negative step change parameterizations for vegetation cover.~~ C) <u>Oscillating mean annual precipitation. Positive and negative amplitudes of oscillation resemble the magnitude of precipitation change extracted from vegetation cover/rainfall relationship from satellite data (Fig. 5)</u>~~Variations in mean oscillating annual precipitation.~~ D) Positive and negative step changes in mean annual precipitation. The initial precipitation <u>was based on values extracted from the worldclim climate dataset for respective focus areas.</u>~~amounts prior to the transient oscillations or step changes correspond to the observed precipitation corresponding to the vegetation cover in each of the observed study areas (Fig. 1). See also Figure 5.~~

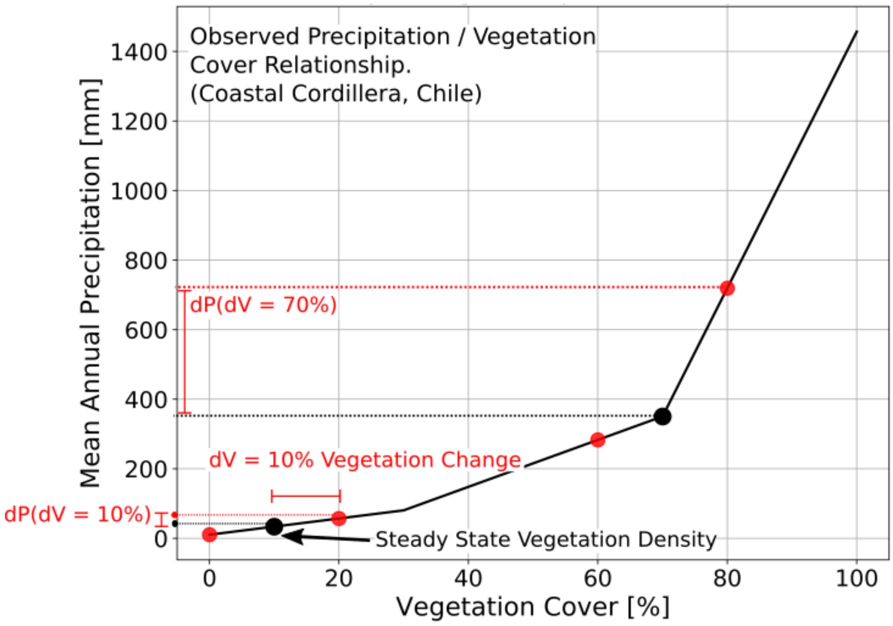


Figure 5 Graphical representation of the observed precipitation – vegetation relationship in the <u>Chilean</u> focus areas (Fig. 1) and how precipitation amounts were selected when perturbations in vegetation cover were imposed. Black dots represent vegetation-precipitation values used in the steady-state model conditions and prior to any transients. Red dots show how

vegetation cover perturbations in +/- 10% in the model simulations were used to select corresponding mean annual precipitation amounts. Note that the observed relationship between observed precipitation and vegetation cover in the Coastal Cordillera of Chile is non-linear, and is a source of the non-linear behavior in model forcing (e.g. Fig. 4) and results (Fig. 17) presented here.

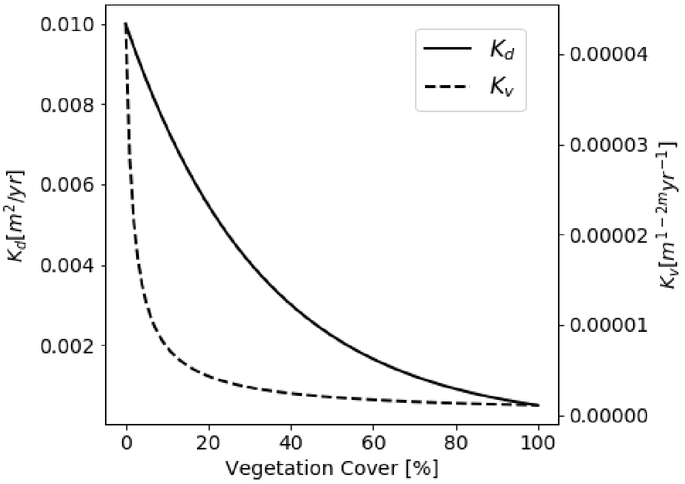


Figure 6 Predicted values of hillslope diffusivity K_d (solid line) and fluvial erodibility K_v (dashed line) as a function of vegetation surface cover. Although absolute values can't be compared due to different units, the shape of the curves representing the different parameters show different sensitivities to changes in vegetation cover, and major source of the non-linearities discussed in the text. Fluvial erodibility shows the highest magnitude of change for vegetation cover values < 25% whereas hillslope diffusivity reacts in a more linearly with highest change below < 65% vegetation cover.

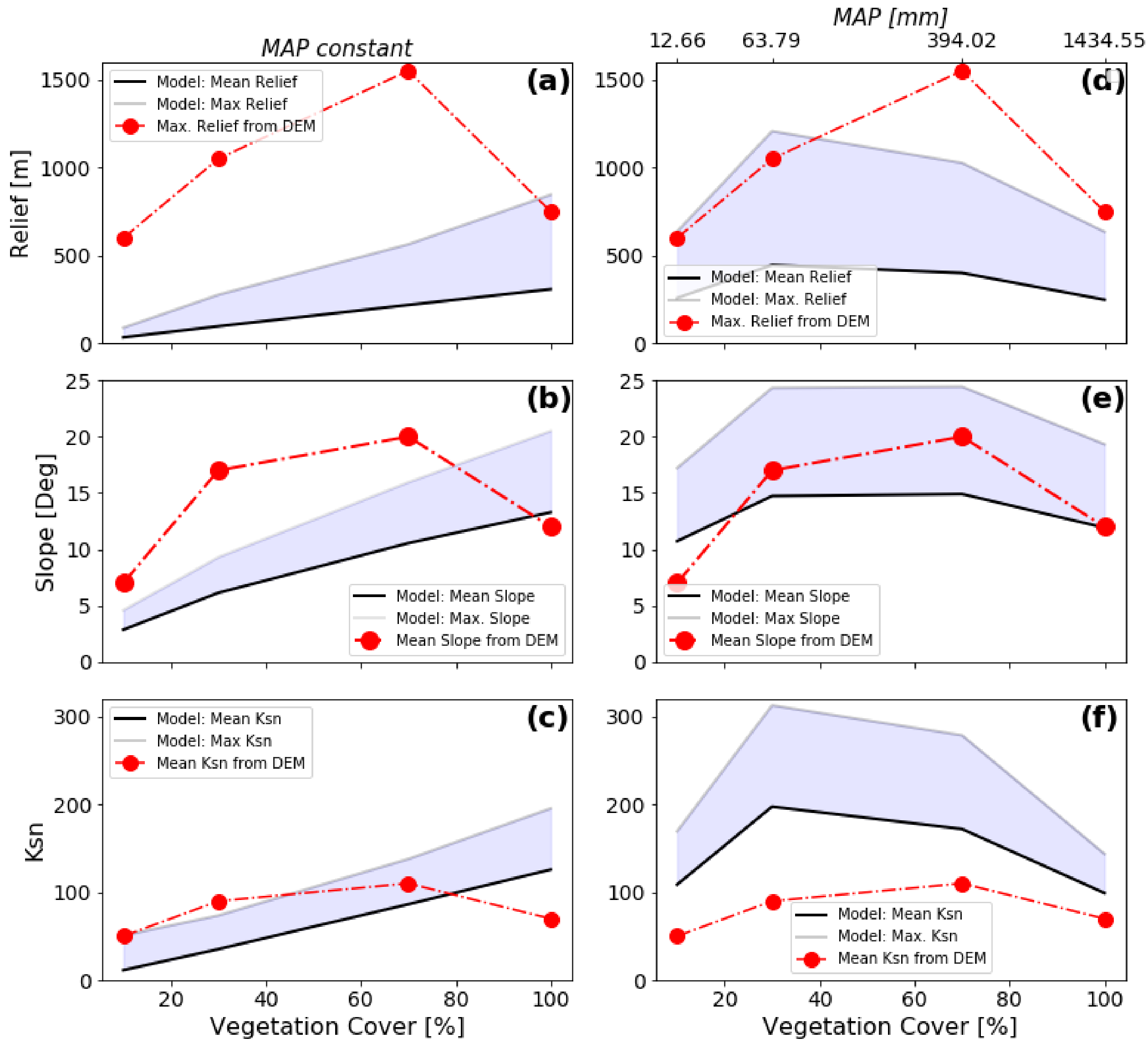


Figure 7 Steady-state model predicted (shaded regions) and observed (red dots) topographic metrics from the study areas shown in Figure 1 for different vegetation cover amounts. Observed topographic metrics were extracted from SRTM 90 m DEM. Model predicted values are shown for the cases of constant mean annual precipitation (a,b,c) or variable precipitation (D,E,F). Variable precipitation rates and vegetation covers were selected for these simulations using the observed values from the focus areas (Fig. 5). Note that for variable precipitation and vegetation cover simulations (d,e,f) the predicted values (similar to the observations) develop a humped shape pattern of an increase and then decrease in each parameter suggesting the changes in both precipitation and vegetation cover are needed to reproduce the general trend seen in observations. The sources of misfit between the predicted and observed values are due to the simplified (and untuned) setup of the simulations and discussed in the text.

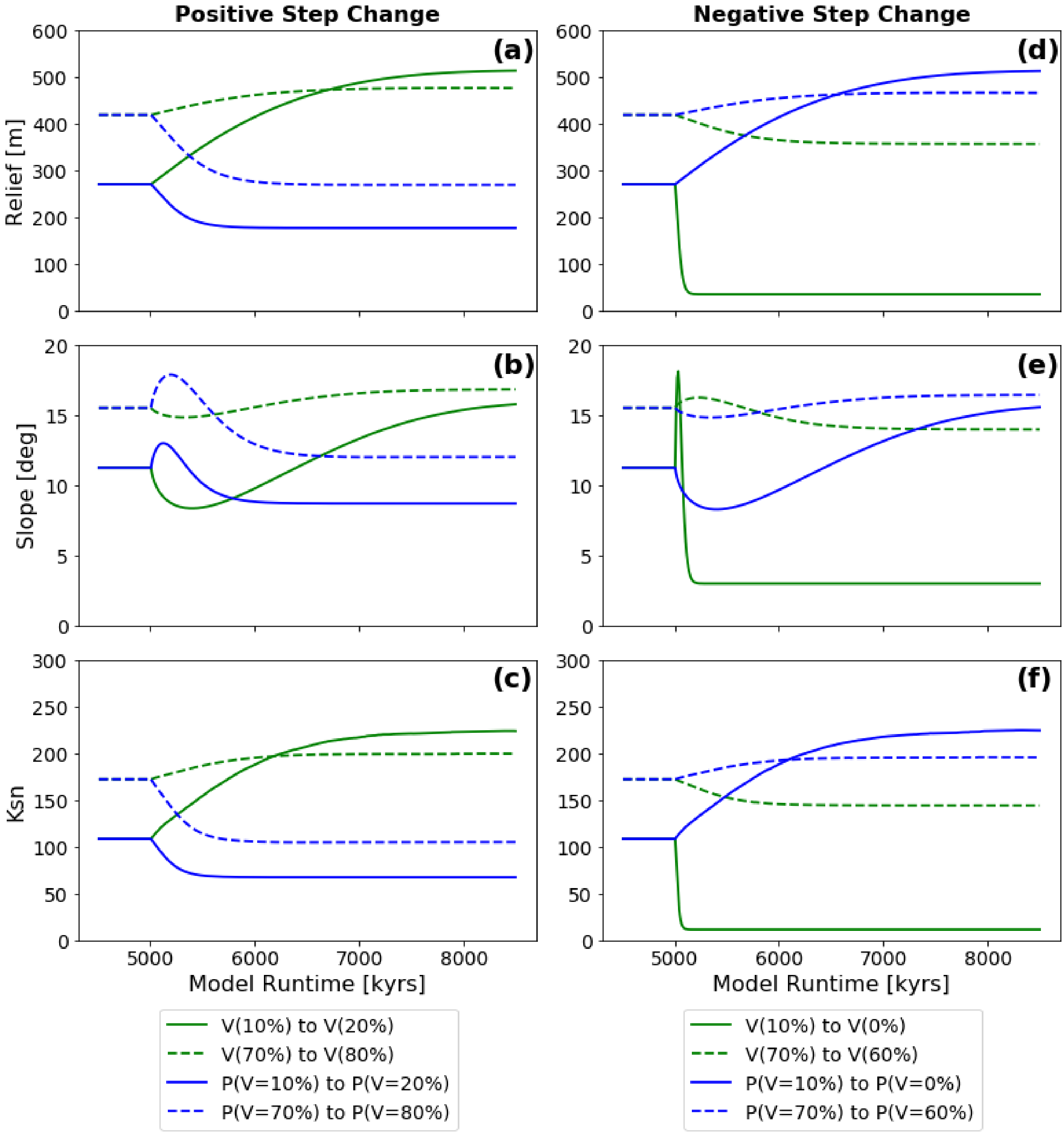


Figure 8 Observed evolution of topographic metrics after a step-change in either vegetation (green lines) or mean annual precipitation (blue lines). Results are shown for two different initial vegetation cover amounts of V=10 and 70%. Imposed mean annual precipitation changes were done by selecting the precipitation amount corresponding to the initial and final vegetation amounts used in the simulations for vegetation cover 'only' change. Panels a,b,c show the reaction of model topographies to positive changes in boundary conditions, panels d,e,f show the reaction to negative changes in boundary conditions.

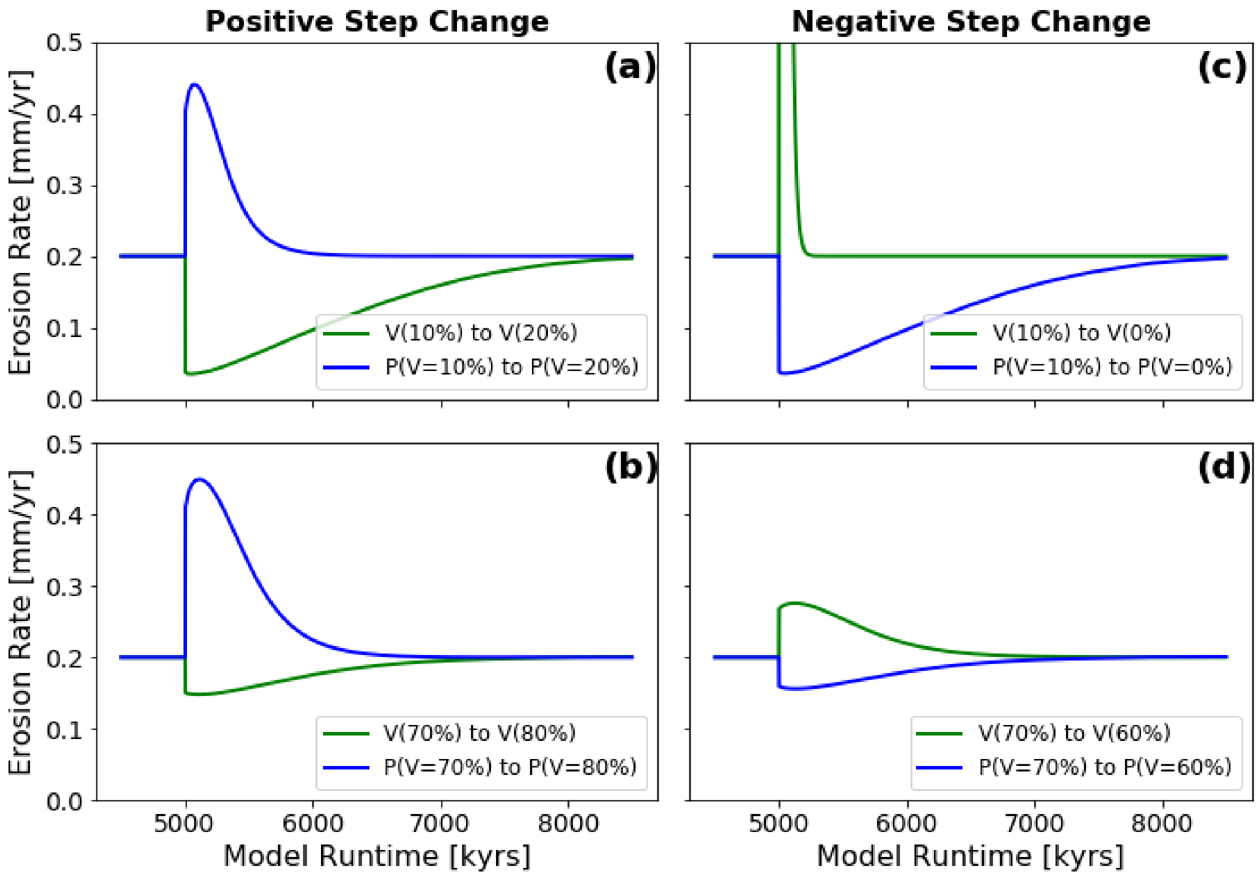


Figure 9 Mean catchment-wide erosion rates after a step-change disturbance in model boundary conditions. Blue lines represent erosion rates for models with changes in only precipitation, green lines represent erosion rates for models with changes in only vegetation cover. Panels a,b show the evolution after positive step-change, panels c,d for models with negative step-change. Note that the direction of change (positive or negative) from the initial state is in the opposite directions for precipitation and vegetation cover changes. This effect is manifested in the subsequent plots.

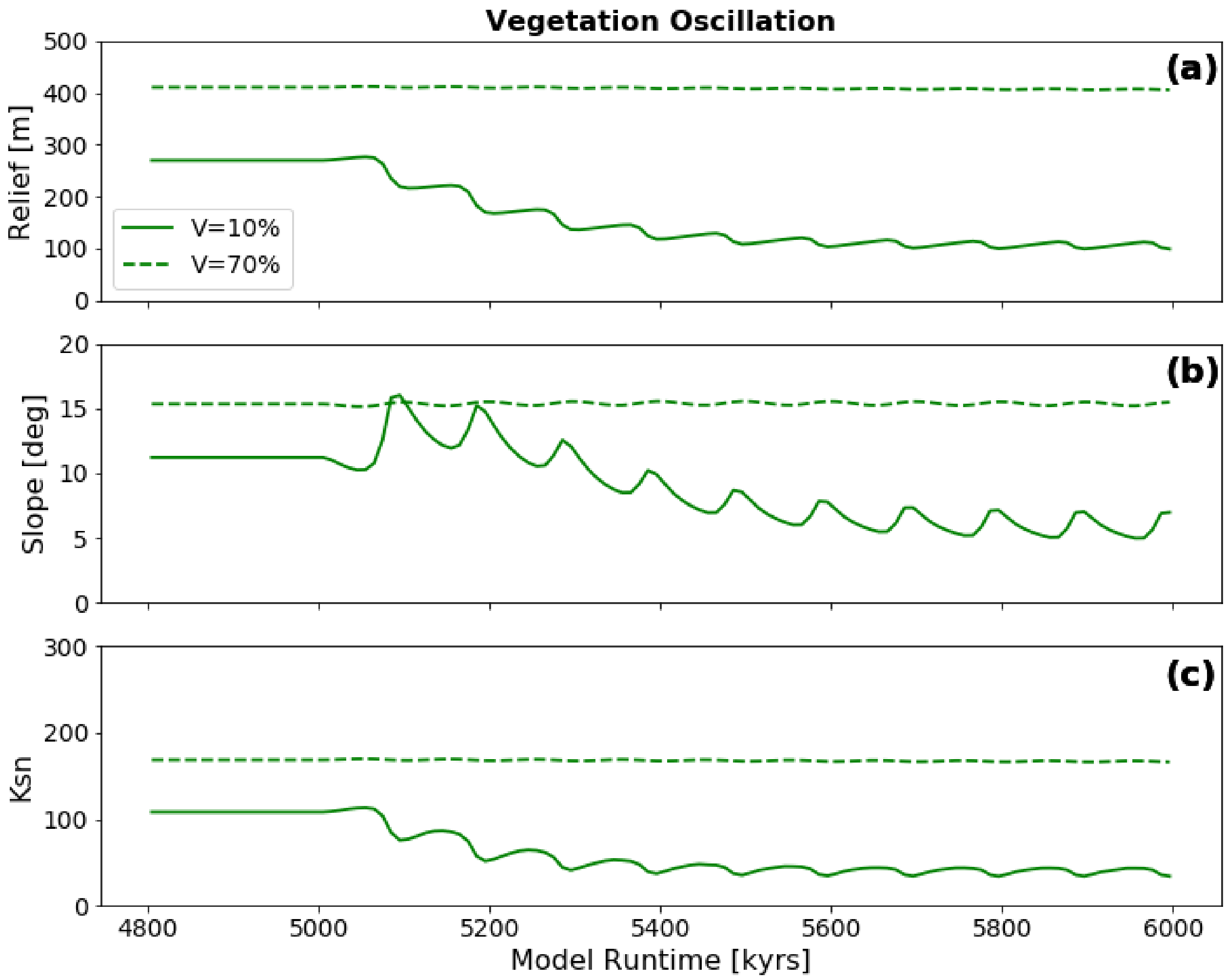


Figure 10 Evolution of topographic metrics for simulations with oscillating surface vegetation cover and constant precipitation corresponding to the initial vegetation cover prior to the transient in vegetation cover. Panels a,b,c show mean basin relief, mean basin slope and mean basin channel steepness (k_{sn}), respectively.

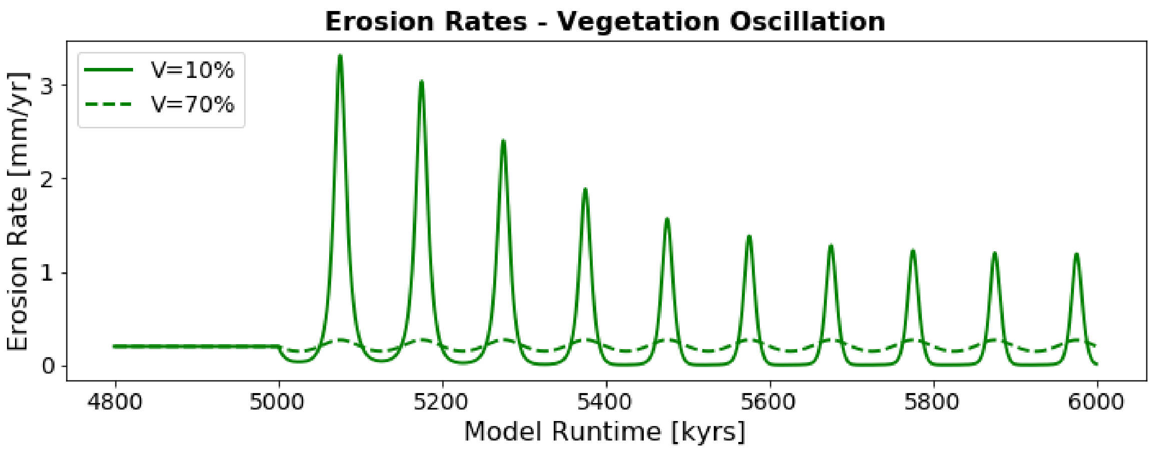


Figure 11 Predicted mean catchment erosion rates for simulations with oscillating surface vegetation cover and constant precipitation. **Note that the magnitude of change in erosion rates for +/- 10% change in vegetation covers differs depending on the initial (or background) vegetation cover. This non-linear response is due in part to the vegetation cover effects on rock erodibility and diffusivity shown in figure 6.**

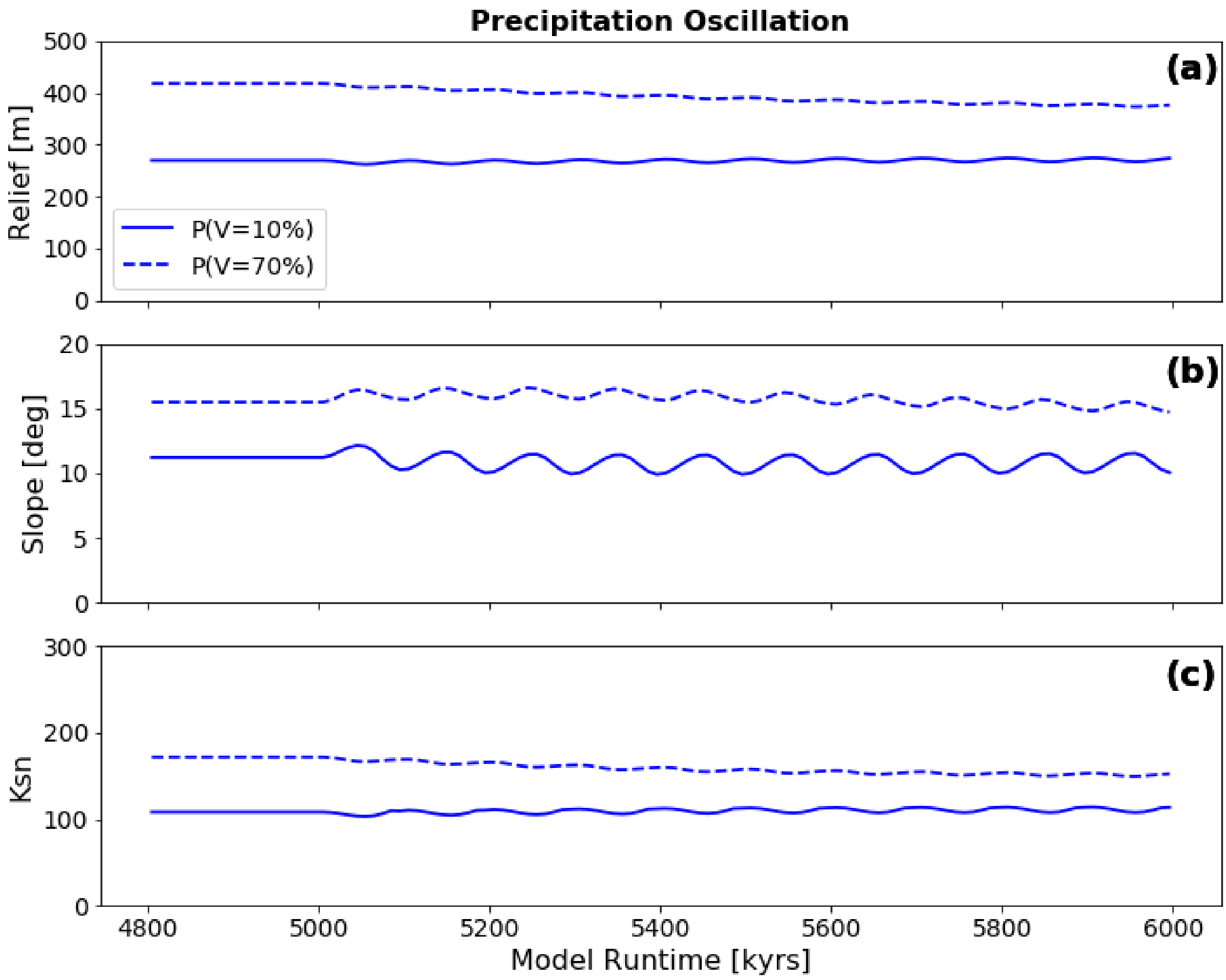


Figure 12 Evolution of topographic metrics for simulations with oscillating mean annual precipitation and constant vegetation cover. The vegetation cover was held constant at the value corresponding to the precipitation rate prior to the onset of the transient at 5000 kyrs. Panels a,b,c show mean basin relief, mean basin slope and mean basin channel steepness (k_{sn}), respectively.

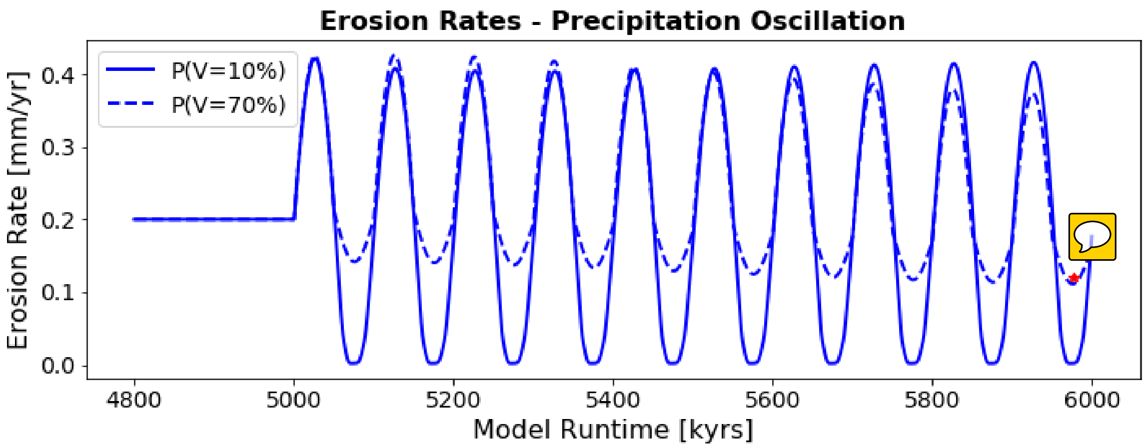


Figure 13 Mean catchment erosion rates for simulations with oscillating mean annual precipitation and constant surface vegetation cover. The amplitude of change in the erosion rates varies with the initial vegetation cover, in part due to the non-linear relationship between precipitation and vegetation cover (Fig. 4, 5).

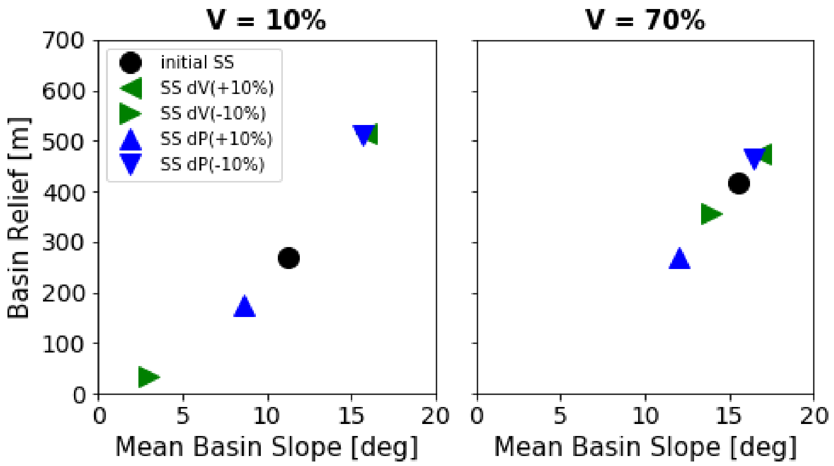


Figure 14 Shifts in mean basin slope/mean basin relief relationship for simulations with positive and negative step-changes in either vegetation cover (green triangles) or mean annual precipitation (blue triangles). Black dots represent initial steady-state conditions prior to any imposed transient in vegetation cover or mean annual precipitation. Note that the sensitivity of topographic relief to perturbations in precipitation or vegetation cover is highest for low-vegetation cover (10%) settings.

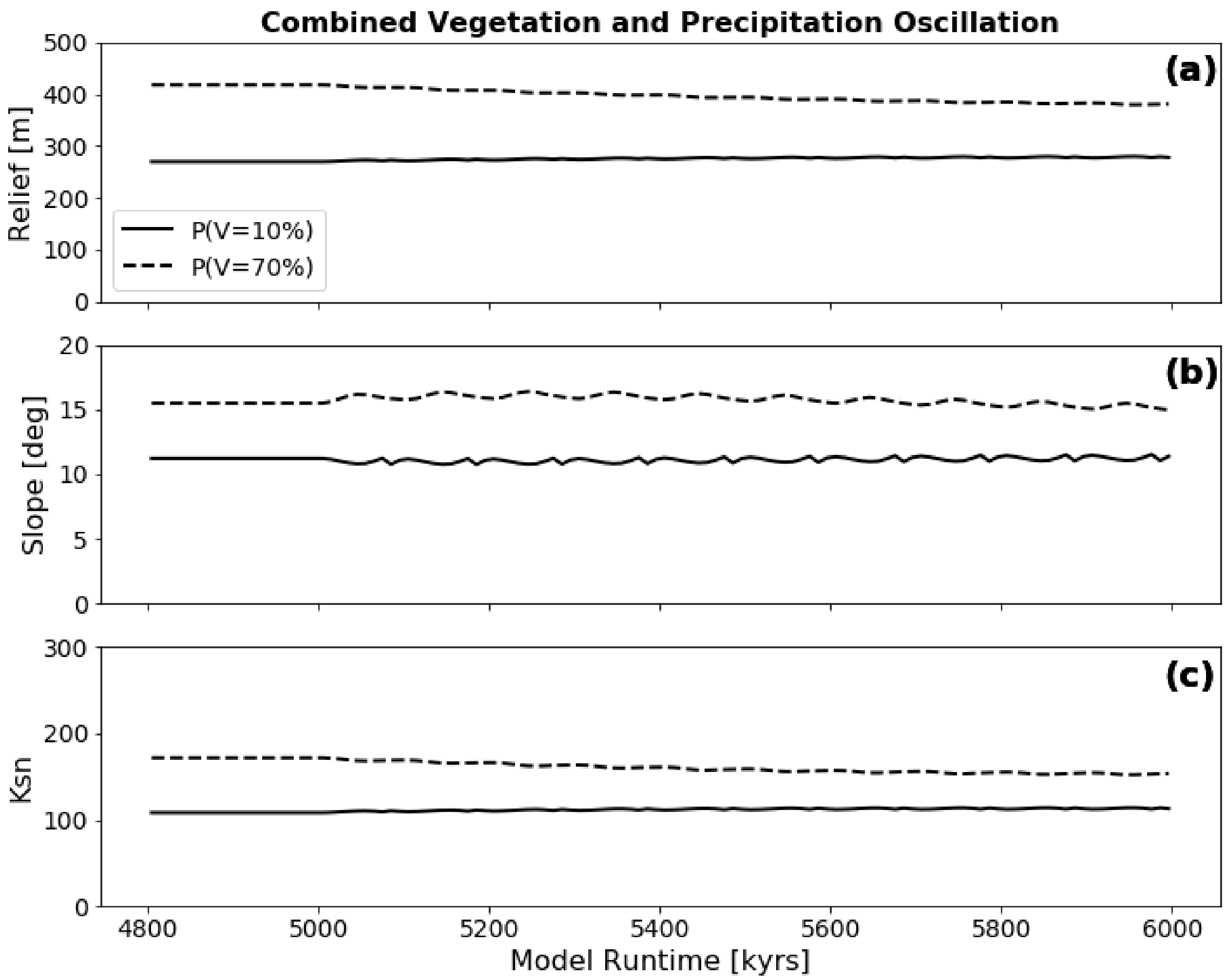


Figure 15 Evolution of topographic metrics for coupled simulations where both changes in surface vegetation cover and a corresponding change (Fig. 5) in mean annual precipitation are simultaneously imposed. The amplitudes and frequency of the forcings that were imposed on the simulations are the same than the ones used for the simulations with isolated transient forcings. Panels a,b,c show evolution of mean basin relief, mean basin slope and mean basin channel steepness (k_{sn}) after start

of oscillation at 5Ma. Note the muted/damped response relative to previous simulations of oscillating vegetation cover or precipitation conditions.

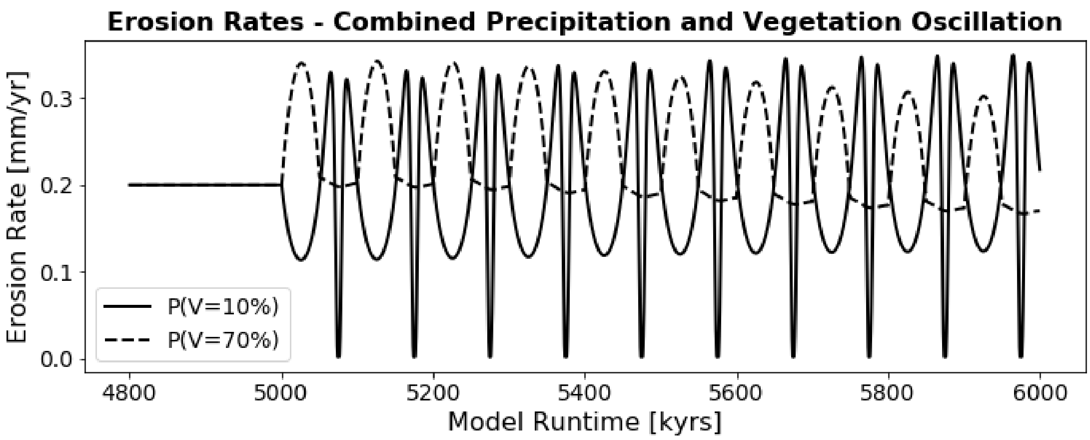


Figure 16 Mean catchment erosion rates for coupled simulations with changes in surface vegetation cover and mean annual precipitation. The first cycle in the time series is expanded in Figure 17. The variable amplitude and non-linear response shown here is due to the combined non-linear forcings in precipitation (Fig. 4, 5) and rock erodibility and diffusivity (Fig. 6) for different initial vegetation cover amounts.

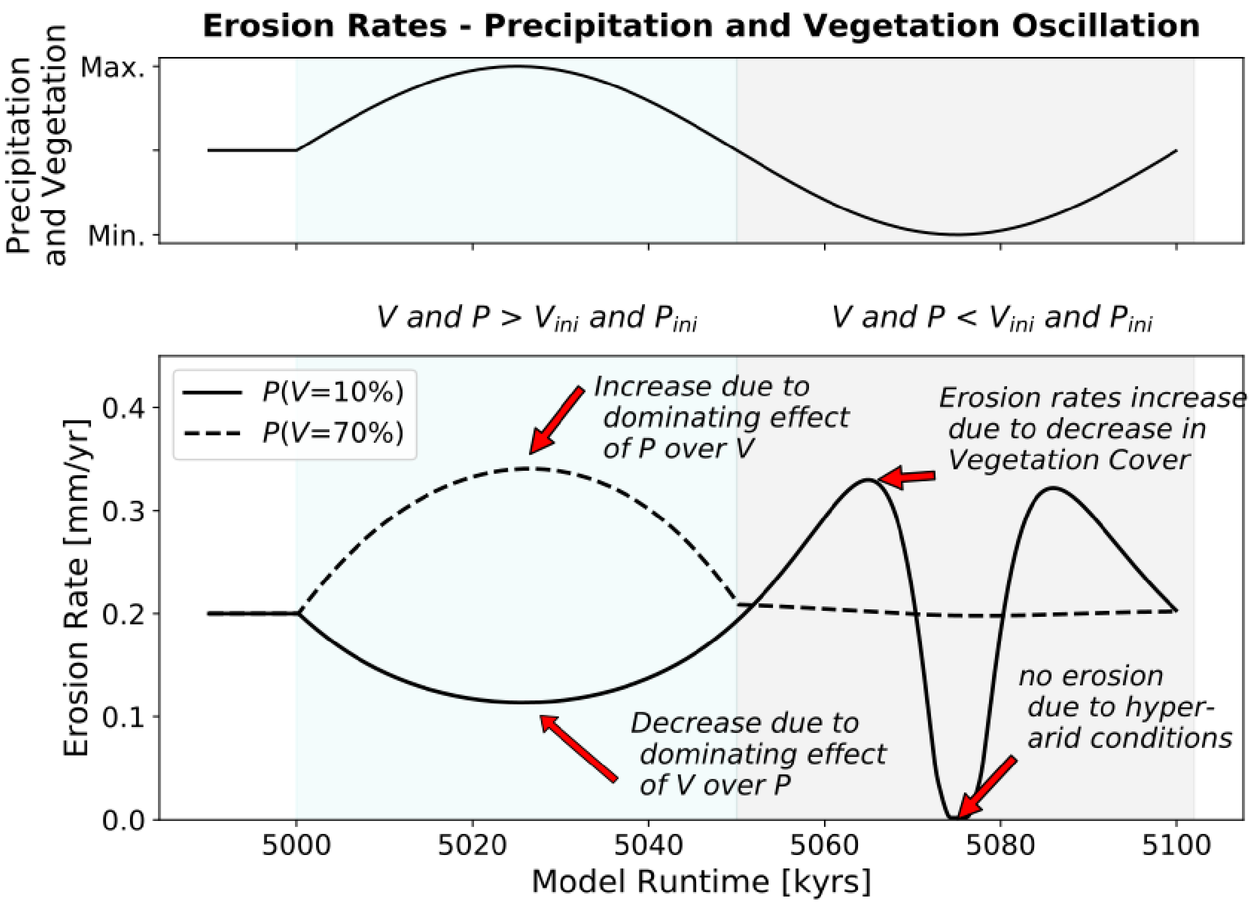


Figure 17 Mean catchment erosion rates for coupled simulations for one period of oscillation after the start of transient conditions (see also Fig. 15). Upper subplot shows conceptualized transient forcing in vegetation cover and mean annual precipitation, lower subplot shows erosion rate for simulations with low (black line) and high (dotted line) initial vegetation cover and precipitation values.

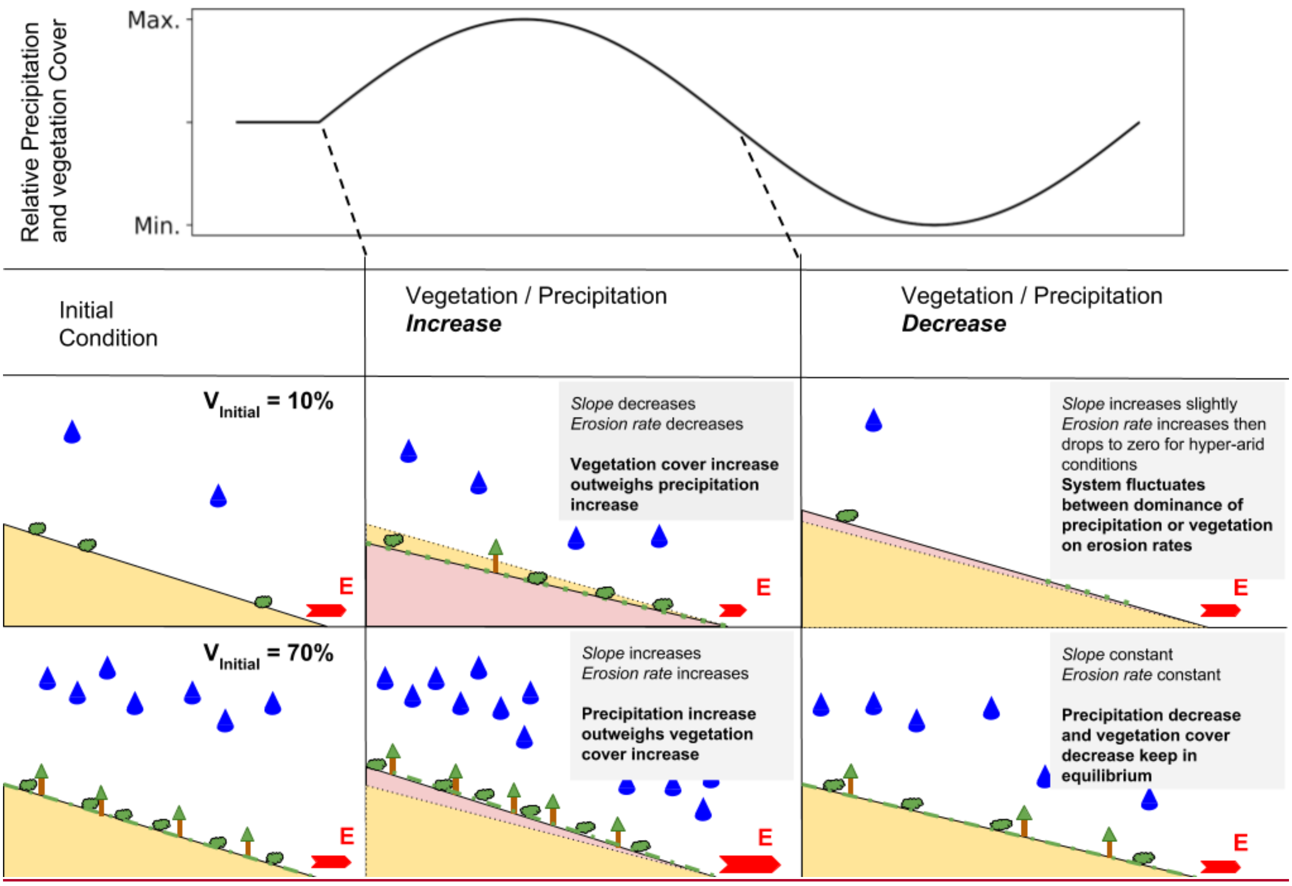


Figure 18 Conceptual figure showing the topographic response from simulations with coupled oscillation of mean annual precipitation and vegetation cover (see Fig. 17 for erosion rates). Upper row illustrates transient forcings, lower two rows shows initial topography (yellow) and resulting transient topography (pink). Changes in topography are not to scale. Vegetation and rainfall amount is shown qualitatively on the hillslopes.

Table 1 Model parameters used for Landlab model setup.

Model Parameter	Unit	Value
Uplift (U)	mm/yr	0.2
Fluvial Erodibility (ke)	m/yr (Kg m1 s-2)-1	7.00E-06
Critical Shear Stress (\tau c)	Pa	58
m, n	-	0.6 / 0.7
Base Diffusivity (Kb)	m2/yr	0.02
Mannings Number (Vegetated, nvr)	-	0.6
Mannings Number (Soil, ns)	-	0.01
Reference Vegetation Cover (Vr)	-	100%
w	-	1
alpha	-	0.3
p	-	1
Transient Vegetation Cover Amplitude (+-dV)	%	10

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