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Interactive comment on "Effect of changing vegetation on denudation (part 2): Landscape response to transient climate and vegetation cover" *by* Manuel Schmid et al.

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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. 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. My main concern is that the results presented here for constant and transient changes individually P and V and combined, is only one potential out come 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.

What was the basis of using 10% and 70% V in the model simulations. I might have missed it. It sound like for a given mean annual P, you need a mean annual V for the sensitivity analysis of the model.

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..

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. 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.

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. 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?

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.

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

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

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.

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.. BTW equation 2 is correct.

Equation 5, Is the Ethreshold used in the model experiments?

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. 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.

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

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

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 simpe way to non-dimensionalize would be to dive all the values with the mean (slope, relief etc.).

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

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Discussion paper

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?

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

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