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Earth Surface Dynamics Discussions

Interactive comment on "The influence of Holocene vegetation changes on topography and erosion rates: A case study at Walnut Gulch Experimental Watershed, Arizona" *by* Jon D. Pelletier et al.

Anonymous Referee #1

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I've corrected this review as I had inserted the wrong reference. Nothing else is changed from the original.

In "The influence of Holocene vegetation changes on topography and erosion rates: A case study at Walnut Gulch Experimental Watershed, Arizona", the authors exploit an extensive modern data set across six catchments extending across an elevation-vegetation gradient. They combine bottom of the watershed sediment data and bare earth lidar observations and derivatives to parameterize a mathematical model that predicts equilibrium drainage density in both grassland and less-vegetated shrubland

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Discussion paper

watersheds. The model results support the hypothesis that drainage density and relief differences can partly be attributed to a late Holocene vegetation change from the protective cover of grasslands and/or forests to lower-elevation shrublands with less vegetative cover and thus more erosion-susceptible bare earth.

The authors are to be commended for applying multiple data-sets from a well-studied watershed to quantify how landscapes respond to climate-driven changes in vegetation in an arid setting. However, I have one large quibble with the study and a host of lesser concerns. I am hopeful that this paper will be published post-revision, once the authors augment the current version by providing critical details and perhaps additional analysis lacking in this version of the manuscript.

The study results derive in a large part from a measured factor of 10 difference in sediment yield between 6 shrub-covered watersheds compared to one grassland watershed. My large quibble is the authors use of a modern 30-year sediment yield data set (erosion rate data) as an input parameter into a model to predict equilibrium drainage density over millennial time-scales without little discussion of the appropriateness of applying a short-term data set to evaluate how differences in erosion rates might control variations in relief, drainage density and predicted differences from divides to valley bottoms. Given that previous work (e.g. Kirchner et al., 2001) point to a discrepancy between decadal-scale measurements of stream fluxes and erosion rates over longer time scales this is a point that needs clarification and discussion in the current study While the Kirchner et al. study was in mountainous terrain, their conclusion that the episodic nature of sediment delivery may limit the use of decadal-scale sediment yield studies in evaluating long-term landscape evolution is extremely pertinent to the Walnut Gulch Experimental Watersheds. I was disappointed in the minimal discussion in the paper of potential episodic events in the watersheds not captured by the data (e.g. extreme rain events, fire) or of the potential for skew in comparing the grassland (n=1) to shrubland (n=5) watershed erosion rate data if episodic events such as fire were non-evenly represented in the watersheds. Additionally, while the mean sediment yield data reflects a 10x difference, the standard deviation for the shrub sites is quite large (2.03 $\pm$ 2.2 t h-1 yr-1) which suggest perhaps exploring potential outcomes across a greater range of reasonable parameters. While MAP is similar between sites, the authors do not discuss differences in rainfall intensity or aspect that may also contribute to different runoff patterns and drainage density. While I am in full agreement with the stated hypothesis regarding a climate-modulated vegetation shift controlling variations in topographic attributes, additional discussion (and perhaps rejection) of alternative controls would strengthen the paper. While the author's do address some of this uncertainty in the discussion section, it seems appropriate to consider alternative scenarios earlier on.

Also I found it puzzling that while Nearing et al. 2005 concluded that the differences in sediment yields between grass and shrub sites was controlled primarily by watershed morphology this study attributes sediment yield differences to the amount of cover. (Though admittedly there may be subsequent studies to the Nearing 2005 paper of which I am unaware).

Other general comments on the paper It would help to clarify if any of the topographic data was smoothed and if so what was the smoothing window for deriving hillslope gradient, curvature, and the stream profiles used for extracting slope-area plots and the associated power-law fits as values as the smoothing radius choices may influence interpretation.

Also, as highlighted below in the line-specific comments, it would be helpful to report the shrubland standard error when comparing values between the grassland and shrubland sites.

Some additional justification on the use diffusive equations for colluvial sediment transport in non-soil mantled shrubland (bare earth) settings seems necessary as geomorphic transport laws do not yet exist for erosion driven by overland flow nor would I expect the hillslope erosion to be smooth but rather episodic and patchy.

Line-specific comments L387-389 What is the SE and number of slopes evaluated for the reported slope values of 0.17 and 0.19?

L259-263 and 390-393 (Figure 6) The relief results depicted in Figure 6 could use additional explanation regarding the 'plateaus and spikes' in the data. While overall relief does increase above $\sim$ 1450 m, there are also rapid reversals expressed in the data.

L404-40 Why does so much of the terrain exhibit positive curvature values? This seems curious unless this corresponds to relief values and if so this is worthy of discussion. Also the deviation between grassland and shrubland curvature is correctly described in the figure caption as from $\sim$ 10 to 300 m2 rather than the 30 m2 value reported in the text.

L407-417 (Figure 9) The use of the slope-area plots seem a bit ad hoc with the exception of the identification of the colluvial region of the data sets. What part of the slope area plots were used to generate the exponents of 0.15 and 0.18 for grasslands and shrublands respectively? Is there a reason for presenting the concavity index values as they are not discussed at all in the document except to note that they somewhat different? I'm also a bit flummoxed by the use of the shrubland mean rather than plotting individual watersheds as there is no way to tell if the watersheds are similar enough to warrant combining. And finally it is common to log-bin the data to reduce noise – I am unsure how the authors generated the concavity index values. Presumably using just the fluvial portion of the plots (> 50 m2 for shrublands and > 100 m2 for the grasslands) and perhaps log-binned? Also it is difficult to distinguish between the slope area plots since they both use a solid black line of the same weight. It appears as though the shrubland plot has depositional areas or knickpoints or something else leading to a non-smooth slope area relationship.

L428-431 Why rely on a diffusivity value from scarp degradation studies (which presumably are faster than hillslope diffusion) rather than the smaller diffusivity values from the western US? This is an area where the authors could explore the parameter space using a range of values in eqns 11 and 12.

L432-435 Nice model results!

L462-468 This analysis is only valid if the decadal erosion rates apply to longer term erosion rates. The authors have not convinced me that this is true.

L523-538 Holmgren (2003) is from a region far to the south of the study area. I seem to recall that more local paleoclimate reconstructions suggest a much later transition from grasslands to shrublands. While the new perspective afforded by the more recent paleo vegetation data is understandably exciting, more context in necessary to demonstrate why this new(ish) data is applicable to a region to the north of the midden data.

L571-578 How does the reduction in vegetative influence on soil stability translate to channels – as an increase in sediment supplied to channels could potentially armor the beds? Especially given that the channels are described as transport-limited.

Section 4.4 The global compilations suggesting little correlation between MAP and erosion rates are increasingly becoming less surprising as increasing numbers of studies and conceptual models are starting to converge on periglacial temperatures and associated processes, rather than MAP, influencing erosion rates in regions mid-latitude terrain (e.g. Herman et al., 2013; Marshall et al., 2015; Savi et al., 2015; Schaller et al., 2002; Tucker et al., 2011). Consider modifying this section to constrain the weak linkages between MAP and erosion rates to unglaciated areas outside the influence of periglacial processes.

L601-605 Perhaps I missed it but I did not see evidence in the manuscript for erosion rates during an arid to humid transition – so while this discussion item is worth considering in general when evaluating the role of the magnitude of transient erosion states, I'm unsure if the study results can be applied to this outstanding argument.

L607-609 This statement on vegetation as an erosion agent due to bioturbation seems to directly contradict the hypothesis stated in lines 571-578. And while trees are considered both soil dilators and mechanisms for bedrock detachment and transport, I am unaware of any studies that suggest grasses or shrubs are effective mechanisms for increasing erosion.

Technical corrections L391 Should be Figure 6 not 5.

L696-698 Holmgren 2005 should be 2003

Figure 1 Missing U/D identifiers for the fault

Figure 3 Missing identifiers of the bottom two graphics (E and F). 3E is very washed out though this may be attributed to the lower resolution.

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