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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 #2

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The paper is well-written and aims to advance the understanding ecosystem controls on landscape patterns. The only issue I would like to raise is in the last paragraph of this review. First I summarized the paper, practically for my understanding and kept it in the review below.

The paper investigates an interesting and relatively unique problem in the intersection between ecosystems and geomorphology at the Walnut Gulch Experimental Watershed (WGEW). The paper first introduces the paleo-ecologic change at the WGEW and resulting differences in erosion rates. In summary, areas higher than 1430 m ASL

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have been grassland and woodlands while elevations lower than 1430 m changed from grassland and woodland to shurblands during the approximately last 2K-4K years. This led to decadal time scale erosion rates ten times higher in shrublands than grassland sites based on Nearing and coworkers' data. Drainage densities are found approximately three times higher, and relief three times lower than elevations above 1430 m where vegetation remained grassland/woodland.

The paper first uses 1m-scale DEMs to examine and show topographic differences in grassland (Kendall) and shrubland (Luck Hills) sites at the WGEW. Authors attribute the observed topographic differences to Holocene vegetation change.

The paper investigates the emergence of the above mentioned patterns using an equilibrium analytical model that predicts drainage density given different erosion rates representing shrub and grass conditions based on the equilibrium model of Tarboton et al. (1992). Decadal sediment flux data from Nearing and coworkers was used to characterize sediment flux in grass/woodland and shrubland watersheds. Channel initiation, and thus drainage density is related to the distance from the hilltop to a location where erosion by fluvial processes exceed diffusive infilling. Watershed topographic data was also used to relate contributing area to distance to outlet for different vegetation types, which is then used in the model. Model predictions were found consistent with observed topographic patters in shrub and grass/woodland vegetated sites.

The contribution of this paper is that it provides a methodology to incorporate watershed-scale sediment flux measurements to the equilibrium model that predicts drainage density. As I understand it, this model can be used where there is differential erosion measurements. Here the application site appears to be locations where vegetation may be responsible for the differences in sediment yields.. Therefore the study considers ecosystem processes implicitly and is not designed to improve ecogeomorphic modeling theory per-se.

The data analysis section of the paper is great and clearly shows associations be-

tween vegetation and morphology. The main issue I have is that an equilibrium model does not seem to be the right tool to test the hypothesis that “late Holocene vegetation changes can modulate drainage density, hillslope-scale relief, and watershed-scale erosion rates..” According to this paper all these changes might have happened approximately in the last 3K years. This hypothesis requires a transient model which would examine if the observed decadal erosion rates, when used with a conservation law, can modify landscapes such that an initially identical topography erodes faster, reducing relief and developing increased drainages that can be recognized on an evolved topography, which would have similar patterns to observed landscapes. I wonder why the authors specifically used an equilibrium model instead of using some of the existing models the lead author uses in his research. Exploring the hypothesis posed in the paper using a transient 2D or 3D model, perhaps in addition to the analytical model presented in the paper, would make this paper a lot stronger and more complete. I strongly recommend the authors to consider revising their papers with this in mind. I also list a few papers below which are relevant to the subject studied in this paper and present similar vegetation controls on landscape morphology, which the authors may want to use for comparison with their findings.

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