We wish to thank the reviewer 1 for his/her insightful comments, which will lead to a significantly improved revision.

Q: "*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 ± 2.2 t h-1 yr-1) which suggest perhaps exploring potential outcomes across a greater range of reasonable parameters.*"

A: Sediment yield cannot be negative, so the measured yields should not be reported as 2.03 ± 2.2 t h^{-1} yr^{-1}. The sediment yield is a function of drainage area, hence some of the variation in measured yields reflects a systematic increase in sediment yield with drainage area at WGEW, a dependence we accounted for. In our analysis we used the sediment yield data to quantify sediment transport coefficients that account for the drainage area dependence. For the shrubland sites the sediment transport coefficient is reported on lines 424-426 as follows: "$k_{Qss} = 2\text{x}10^{-6}$ $m^{1.56}$ yr^{-1} (with a range of values including one standard error from $2\text{x}10^{-7}$ to $2\text{x}10^{-5}$), $p = 1.44 \pm 0.2$, and $R^2 = 0.93$." The value of k_{Qss} one standard error below the mean is still more than 3 times larger than the value of k_{Qsg}, which is $6\text{x}10^{-8}$ $m^{1.56}$ yr^{-1}. We would certainly be willing to include additional potential outcomes (which would simply show a range of possible drainage densities depending on the ratio of k_{Qss} to k_{Qsg}), but we believe that our fundamental conclusion that sediment transport coefficients are ~10x greater in shrublands relative to grasslands is not undermined by the variability in the data.

We appreciate that sediment yields can differ among time scales. However, we believe that our decadal-scale sediment yields are an appropriate estimate (and certainly the best-available estimate given the likely impossibility of ^{10}Be cosmogenic erosion-rate determination in WGEW due to the Pleistocene age of the bedrock) for millennial-scale yields based on three lines of argument:

1) Published analyses have shown that fluvial sediment transport in WGEW, while highly episodic, is not dominated by a very small number of extreme events. The effective discharge (i.e. the discharge above which half of the total load is transported) occurs many times within a 30-year record. In a study of sediment transport in the 1995-2005 period, for example, Nearing et al. (2007) addressed this issue as follows: "For six of the seven watersheds, between 6 and 10 events produced 50% of the total sediment yields over the 11-year period." That is, the effective discharge has a recurrence interval of between approximately 1 and 2 years. This is consistent with many other studies

demonstrating that that the effective discharge in low-gradient alluvial channels not subject to debris flows has a recurrence interval of approximately 1 to 2 years (e.g., Wolman and Miller, 1960; Andrews, 1980; Andrews and Nankervis, 1995).

References:

Andrews, E. (1980), Effective and bankfull discharge in the Yampa River Basin, Colorado and Wyoming, J. Hydrol., 46, 311-330.

Andrews, E. D., and, J. M. Nankervis (1995), Effective discharge and the design of channel maintenance flows for gravel-bed rivers, Natural and Anthropogenic Influences in Fluvial Geomorphology, AGU Monograph Series 89, Washington, D.C., 151-164.

Wolman, M. G., and, J. P. Miller (1960), Magnitude and frequency of forces in geomorphic processes, J. Geol., 68, 54-74.

2) Sediment yields calculated from 1995-2005 by Nearing et al. (2007) closely match yields measured over approximately 50 years using ^{137}Cs (Nearing et al., 2005), as we noted on lines 205-208. The 50 year time scale includes significant droughts at WGEW. We are not suggesting that this proves that sediment yields are invariant out to time scales of millennia, but it does establish constancy of rates out to “short” time scales that are significantly longer than most studies of short-term erosion rates, including Kirchner et al. (2001).

3) Kirchner et al. (2001) showed order-of-magnitude differences in sediment yields/erosion rates between interannual and millennial-scale erosion rates from a forested landscape subject to high-severity (i.e. stand-replacing) forest fires. The recurrence interval of high-severity forest fires in the western U.S. ranges from 150 to 400 yr based on frequency distributions of even-aged stands and fire-related sedimentation studies (Meyer et al., 1995; Veblen, et al., 1994; Kipfmueller and Baker, 2000; Sibold et al., 2006; Margolis et al., 2007; 2011; Fitch and Meyer, 2015). Erosion rates commonly increase by 2 to 4 orders of magnitude for one to several years following high-severity forest fires (e.g. Wagenbrenner and Robichaud, 2014; Orem and Pelletier, 2015). These numbers strongly suggest that, *in forests similar to those studied by Kirchner et al. (2001)*, millennial-scale erosion rates may be dominated by the erosion that occurs shortly following high-severity wildfires. This suggestion is consistent with the work of Fowler (1979). In his remarkable compilation of 90 sediment yield studies in forested areas of the U.S., Fowler (1979) found that approximately 20% of the 90 studies report short-term erosion rates of <1 μm/yr, i.e. 2 orders of magnitude below typical long-term erosion rates (i.e. ~10-100 m/Myr in mid-latitude areas of low to moderate relief). Of the remaining 80% of studies he compiled (i.e. those with short-term erosion rates >1 μm/yr), Fowler (1979) demonstrated that nearly all were associated with the occurrence of wildfire and/or landsliding/debris flows. As such, it is likely that some of the episodicity in erosion rates documented by Kirchner et al. (2001) is the result of high-severity forest fires and/or episodic mass movements. Neither of these processes occurs at WGEW in the late Holocene. The modest slopes of WGEW preclude debris flows. Wildfires are of very limited size and severity in shrublands. Grassland fires in Arizona typically result in modest (if any) increase in runoff and erosion rates (e.g. Stone et al., 2003).

References:

Fitch, E.P., and Meyer, G. (2015), Temporal and spatial climatic controls on Holocene fire-related erosion and sedimentation, Jemez Mountains, New Mexico, Quat. Res., doi:10.1016/j.yqres.2015.11.008.

Fowler, J.M. (1979), The interface of forestry and agriculture as nonpoint sources of suspended sediment: A national modeling approach, Ph.D. dissertation, Iowa State Univ., Ames, Iowa, 244 p.
Kipfmueller, K.F., and Baker, W.L. (2000), A fire history of a subalpine forest in south-eastern Wyoming, USA, J. Biogeogr., 27, 71-85, doi: 10.1046/j.1365-2699.2000.00364.x.
Margolis, E. Q., Swetnam, T. W., and C. D. Allen (2007), A stand-replacing fire history in upper montane forests of the southern Rocky Mountains, Can. J. Forest Res., 37(11), 2227-2241.
Margolis, E. Q., Swetnam, T. W., and C. D. Allen (2011), Historical stand-replacing fire in upper montane forests of the Madrean Sky Islands and Mogollon Plateau, southwestern USA, Fire Ecol., 7(3), 88-107.
Meyer, G.A., Wells, S.G., and Jull, A.J.T. (1995), Fire and alluvial chronology in Yellowstone National Park: Climatic and intrinsic controls on Holocene geomorphic processes, Geol. Soc. Am. Bull., 107(10), 1211-1230, doi: 10.1130/0016-7606(1995)107<1211:FAACIY>2.3.CO;2.
Orem, C., and Pelletier, J.D. (2015), Quantifying the time scale of elevated geomorphic response following wildfires using multi-temporal LiDAR data: An example from the Las Conchas fire, Jemez Mountains, New Mexico, Geomorphology, 232, 224-238, doi: 10.1016/j.geomorph.2015.01.006.
Sibold, J.S., Veblen, T.T., and Gonzalez, M.E. (2006), Spatial and temporal variation in historic fire regimes in subalpine forests across the Colorado Front Range in Rocky Mountain National Park, Colorado, USA, J. Biogegr., 32, 631-647, doi:10.1111/j.1365-2699.2005.01404.x.
Stone, J.J. et al., (2003), Post-wildfire runoff and erosion response on grassland and oak woodlands in southeastern Arizona, available at https://www.firescience.gov/projects/03-2-3-11/project/03-2-3-11_03_2_3_11_Deliverable_03.pdf
Veblen, T.T., Hadley, K.S., Nel, E.M., Kitzberger, T., Reid, M., and Villalba, R. (1994), Disturbance regime and disturbance interactions in a Rocky Mountain subalpine forest, J. Ecol., 82(1), 125-135, doi:10.2307/2261392.
Wagenbrenner, J.W., and Robichaud, P.R. (2014), Post-fire bedload sediment delivery across spatial scales in the interior western United States, Earth Surf. Process. Landf, doi:10.1002/esp.3488.

Q: "*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.*"

A: Certainly we can include these data, which demonstrate minimal differences between the shrubland and grassland sites. Nearing et al. (2015) (referenced in our paper) demonstrated that rainfall erosivity (which includes intensity at 30 min duration) is approximately 20% lower in Lucky Hills compared to Kendall (their Figure 1b). This is somewhat larger than the difference in MAP we noted on line 145, but still insignificant when compared to the 30-fold difference in erosion rates/sediment transport coefficients. Kendall drains to the west and has roughly equal areas of N- and S-facing hillslopes. The shrubland sites have a range of aspects and exhibit erosion rates approximately 30x higher than the grassland sites across all aspects. These points will be included in the revision.

Q: "*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).*"
A: A key point of our paper is that watershed morphology and vegetation cover are related, so it is not inconsistent to invoke both watershed morphology and vegetation cover as controls, as the two are related. Also, Nearing et al. (2005) invoked vegetation cover as a cause of erosion rate differences. In order to emphasize this fact, we took the unusual step of directly quoting Nearing et al. (2005) on this point in lines 214-220: "Nearing et al. (2005) interpreted the differences in erosion rates between Lucky Hills and Kendall to be primarily a function of vegetation cover, i.e. "hydrologic response differences as a function of vegetation differences are probably largely responsible for the differences in hillslope erosion rates between the two watersheds. If flows are more concentrated and vegetative cover is less, as on the Lucky Hills site, flow shear stresses and stream power will tend to be greater, resulting in a greater hydrologic potential for erosion. Also important is probably the higher litter cover and plant basal area cover on the grassland site that would have a direct protective effect against erosion."

Q: "*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.*"
A: As stated on line 233 an Optimal Weiner Filter was used to smooth the topography, following Pelletier (2013).

Q: "*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.*"
A: For the shrubland sites this value is reported on lines 424-426 as follows: "$k_{Qss} = 2x10^{-6}$ $m^{1.56}$ yr^{-1} (with a range of values including one standard error from $2x10^{-7}$ to $2x10^{-5}$), $p = 1.44 \pm 0.2$, and $R^2 = 0.93$." Note that the standard error is included.

Q: "*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.*"
A: WGEW is soil mantled almost everywhere (outcrops are extremely rare). Hence, we do not think a discussion of non-soil-mantled landscapes is relevant to our study area. It may be that the reviewer has interpreted our reference to "bare soil" on line 182 as referring to an absence of soil. Bare soil simply refers to soil with no vegetation or stone cover.

Q: "*L387-389 What is the SE and number of slopes evaluated for the reported slope values of 0.17 and 0.19?*"

A: The computed slope values are based on an analysis of all hillslope pixels in the DEM (tens of millions of pixels). It is somewhat difficult to place a precise error estimate on this value. The mean value can be estimated very precisely because we have tens of millions of pixels to average and the error of the mean decreases as sqrt(N). There is, however, also a structural error associated with the fact that what is hillslope versus what is valley bottom cannot be defined with absolute precision.

Q: "*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 ~ 10 to 300 m2 rather than the 30 m2 value reported in the text.*"
A: It may be that the community has become used to looking at numerical models of landscapes driven to topographic steady state, and that this has biased our view of the world. In such cases the vast majority of the landscape (i.e. essentially all hillslopes) will have negative curvature. In nature, however, toeslopes commonly have positive curvature and colluvial infilling often results in wide unchannelized valley bottoms with positive curvature. The numerical model results presented by Pelletier (2011) that were motivated by WGEW and do not assume topographic steady state (they were subjected to a pulse of uplift/base-level fall followed by stasis) show that much of the landscape has positive curvature. In the revision we will change the 10 m^2 reported value to 30 m^2 on line 406.

Q: "*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.*"
A: Yes, the data were log-binned. We will take this figure and the associated text out of the revision. We just thought that analysis was useful to show as part of a complete topographic analysis of the study area, but we will remove it since the results are not essential.

Q: "*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.*"
A: Scarp degradation is modeled as hillslope diffusion by Hanks (2000) and all of the papers he cites. As such, we are unsure what the reviewer is referring to when he/she suggests a difference between diffusivity values derived from scarp degradation and those derived from hillslope diffusion. Scarp degradation is hillslope diffusion so there is no difference. The scarp studies

referenced by Hanks (2000) that led to the 1 m^2/kyr value are all from the western U.S. and range between approximately 0.5 and 2 m^2/kyr, hence they have a geometric mean of approximately 1 m^2/kyr.

Q: "*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.*"
A: The study sites of Holmgren et al. (2003) are directly east of WGEW (both are centered on 31.75°N). There is no more local paleovegetation reconstruction that exists.

Q: "*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.*"
A: A difference in bed armoring between the shrubland and grassland sites would not result from a difference in sediment supply but rather from a significant difference in the texture of the bed material. Bed sediment texture is similar in Lucky Hills and Kendall.

Q: "*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 and Braun, 2008; 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.*"
A: We intended this discussion to focus on non-glaciated areas outside the dominant influence of periglacial processes. Sentence will be modified in revision to: "Recent work on the role of vegetation, and its changes through time, can provide a basis for understanding the relatively low correlation between erosion rates and MAP in unglaciated areas outside the dominant influence of periglacial processes and the complex relationship between erosion rates and climate in such areas more generally." That said, while periglacial processes involve temperature cycling near the freezing point of water, they also involve water, hence MAP or other measures of water availability are still relevant to the discussion of peri/paraglacial process rates.

Q: "*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.*"
A: Correct – our study provides no data for the landscape response to arid-to-humid transitions. We will simply remove the sentence "That is, erosion rates can be larger during a humid-to-arid transition than during an arid-to-humid transition, even if the mean climatic conditions (averaged over the transition) are equal" in the revision.

Q: "*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.*"

A: We do not see a contradiction here. Lines 571-578 refer to the concept that less vegetation cover leads to an increase in drainage density, all else being equal. Drainage density is set by a competition between diffusive (colluvial) and advective (fluvial) processes. Less vegetation cover leads to a decrease in colluvial transport rates and an increase in fluvial transport rates, as stated on lines 607-609, hence it increases drainage density by increasing the relative importance of advective processes relative to diffusive processes, as stated on lines 571-578.

Previous studies have shown that more vegetation cover drives higher rates of colluvial (i.e. bioturbative) transport (e.g. Hughes et al., 2009) while less vegetation cover drives higher rates of fluvial erosion (e.g. this paper and many references therein, including Abrahams et al., 1995), all else being equal. We agree there may be no studies in which an increase in grass or shrub density specifically has been shown to increase colluvial transport rates. However, there are certainly a number of studies (e.g. Hughes et al., 2009) that demonstrate that an increase in biomass generally increases sediment transport by bioturbation.

References:

Hughes, M.W., P.C. Almond, and J.J. Roering (2009), Increased sediment transport via bioturbation at the last glacial-interglacial transition, Geology, 37(10), 919-922, doi:10.1130/G30159A.1

The incorrect figure number on line 391 and the incorrect date on Holmgren et al. (2003) in the reference list will be fixed in the revised paper. We will also include labels E and F in Fig. 3. We appreciate the reviewer pointing out these errors.