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Interactive comment on "The linkage between hillslope vegetation changes and late-Quaternary fluvial-system aggradation in the Mojave Desert revisited" *by* J. D. Pelletier

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Response to Reviewer 2:

Q1: "One of the important foundations of this paper is the claim that the timing of disappearance, or absence, of juniper from packrat middens represents a loss of woodland cover and that this disappearance occurs progressively from lower to higher elevations through time. The claim is first made in lines 1-3 at the top of p. 185, but there is no citation or discussion to back this up until two pages later - a citation should be included here."

A1: A citation (McDonald et al., 2003) has been added to the revised paper as sug-

gested.

Q2: "A serious concern I have is with the use of this midden data to establish a curve (figure 3), which is then applied in a GIS database to estimate the time of conversion from woodland to desert scrub at different altitudes in the further analysis. First, the stated area includes 3 degrees of latitude and 2.5 degrees of longitude. There may be a significant climatic effect across this area with respect to lower treeline, with the elevation decreasing northward. And from west to east, there are certainly significant modern gradients in atmospheric moisture source and movement. For example, this area includes the southern Sierra Nevada and San Bernardino Mountains, which are today and for much of the Holocene subject to a Mediterranean climate with major winter precipitation, whereas the central Mojave Desert - the main focus of this paper - has essentially equal amounts of winter and summer precipitation. Further, the eastern part of the study area has significant impact from the summer monsoon, which penetrates northward along the Colorado River corridor and extends at least as far as the Providence Mountains. Some discussion regarding these complications should be included - it is not a monolithic area with respect to weather patterns either today or during the late Pleistocene and Holocene."

A2: I shared the reviewer's concern that the elevation of the woodland-to-desert-scrub transition might differ substantially by latitude within the study area. That is precisely why I quantified the variation in vegetation cover versus elevation and latitude within the study area (results presented in Figure 8). As the original paper stated, "The results demonstrate that there is no systematic change in the relationship between modern vegetation cover and elevation with latitude from 34-36°N in the Mojave Desert." I did not present the results for variations in longitude but I can report that there are also no systematic variations (e.g. a steady increase or decrease) in the elevation of the woodland-to-desert-scrub transition with longitude either. The reviewer did not comment on Figure 8, making it difficult for me to know how to improve on that figure or the underlying analysis in order to better address his/her concerns. I am not sure what the basis is for the reviewer's statement that summer monsoon moisture extends at least as far west as the Providence Mountains. As noted in the original manuscript, an analysis of NAM storm tracks shows no influence on California (Dominguez et al., 2009). Eppes et al. (2010) (Geomorphology, 123, 97-108) recently stated unequivocally that the Mojave Desert receives no summer monsoonal precipitation. Certainly all areas of the Mojave Desert (just as all land areas worldwide) experience summertime convective storms of the type that are often associated with monsoons. However, it is important to distinguish between summertime convective storms generally and those sourced by monsoonal moisture specifically. A diurnal cycle of convective precipitation with peak rainfall in the afternoon or early evening occurs over all land areas because the land surface warms daily in the morning and early afternoon, initiating upward motion of air (convection) that can trigger a positive feedback of condensation, release of latent heat, and further convection (e.g. Bechtold, P., Chaboureau, J.-P. Beljaars, A., Betts, A.K., Kohler, M., Miller, M., and Redelsperger, J.-L., 2004. The simulation of the diurnal cycle of convective precipitation over land in a global model, Quarterly Journal of the Royal Meteorological Society, 130, 3119-3137, 10.1256/qj.03.103; Gray, W.M., and Jacobson, Jr., R.W., 1977. Diurnal variation of deep cumulus convection, Monthly Weather Review, 105, 1171–1188, doi: 10.1175/1520-0493(1977)105<1171:DVODCC>2.0;). This feedback is enhanced in monsoon systems due to differential heating of the land and ocean (pulling additional moisture in from the ocean), but the phenomenon is not limited to land areas influenced by monsoons. The presence of summertime convective moisture (which indeed occurs throughout the Mojave Desert) does not prove that that moisture is monsoon-related. To prove that the moisture is monsoon-related, one must track sources of moisture explicitly, as Dominguez et al. (2009) did in demonstrating that the NAM has negligible influence in California. That said, I have modified the language of Section 4 to honor the possible role of the monsoon (now or in the late Pleistocene) in driving erosion in the Mojave. The reviewer's contention that my study area is not "monolithic" cannot be proven or disproven – it is a subjective statement that is hard to address. All geologic study areas have heterogeneity that prevent per-

fect comparisons/replication. As noted, Figure 8 represents my best attempt to test for systematic spatial variations in vegetation cover within my study area. The fact that no systematic variations in vegetation cover exist as a function of latitude or longitude is evidence in favor of the appropriateness of treating the study area as a study unit for the purposes of this analysis.

Q3: "Second, it appears that the line between presence and absence of juniper is not constrained between about 10-3 ka and 1100-1800 masl. Other lines could be drawn where data is missing. For example, the lower portion of the line with lower slope can permissibly be extended out to about 3 ka and then turn straight up to an inflection point at about 1800 masl. Or the line could inflect the other direction, with altitude essentially increasing in a step change at about 10 ka. Such a line would have a significant influence on the modeled time of onset of hillslope instability and aggradation."

A3: I agree with the reviewer that other elevation-age curves could be drawn in the age range of 10-3 ka for elevations between 1100 and 1800 m a.s.l. I doubt that the treeline underwent a step function of the sort that the reviewer proposes (because the temperature change was not a step function), but it is theoretically possible. In the revised paper I have added additional text on this point: "The elevation-age relationship (i.e. solid curve in Fig. 3) for the lower limit of Juniperus is well constrained in the 15-10 ka cal BP interval but significantly less well constrained in the 10-3 ka cal BP interval. However, this uncertainty has little practical effect on the comparison of the model predictions to data because the predicted age of initiation of primary aggradation is between 15 and 10 ka cal BP for all of the sites except two (Johnson Valley and Grassy Valley) (Table 1)."

Q4: "Another issue is the claim that the predictions of the modeled PVCH "are consistent with 8 out of 9 sites of aggradation and incision in the Mojave Desert" with sufficient age control (lines 5-9, p. 193). This claim seems exaggerated. "Consistent with" would imply that the predicted timing plus estimated error should at least overlap with the range of dates. This is certainly not the case for Chambless, as stated, but also to some extent for the three highest sites. In Table 1, the predicted vs. actual aggradation times match but not the predicted vs. actual time of incision. At Johnson Valley, predicted vs. actual do slightly overlap, and at Grassy Valley they do not overlap at all granted, the actual time of aggradation is only a maximum age so it could have been later in time (see fig. 7). However, these sites also fall within the time and altitude range in which the lower juniper occurrence line is unconstrained (see comment above) and this could well explain the discrepancy."

A4: I have changed the quote to "are consistent with eight out of nine sites of aggradation in the Mojave Desert." I have also included a paragraph in the Discussion regarding the fact that the model does worse at predicting the age of incision at the two sites where incision ages are available: "In the two sites where the timing of incision is constrained (southern Death Valley and Sheep Creek), the model underpredicts the age of incision by approximately 3 ka. It is difficult to draw conclusions from a sample size of two, but the discrepancy between the predicted and measured incision ages could be due to the relatively large uncertainty of the timing of paleo-vegetation changes within the 10-3 ka interval and/or the relatively large uncertainty associated with ages of incision measured (as done here) using the highest stratigraphic age (which necessarily overestimates the age of incision)."

Q5: "It is surprising that in the review of theory regarding timing of fan aggradation and incision, the recent publication in GSA Bulletin by Enzel, Amit, and others extensively revising the cause and timing of aggradation at Nahal Yael in Israel is not quoted or discussed. Bull's original ideas about this topic (the PVCH) were founded on visiting this study site so it seems that the complete revision of these ideas should be cited and at least briefly discussed in this paper. The author addresses the other proposed hypotheses re: enhanced ENSO and enhanced monsoon as aggradation triggers. On p. 196, the author discusses and dismisses monsoon effects on the basis that the modern monsoon is not important in the Mojave Desert. As discussed above, it is a player in the eastern Mojave today. More to the point here, however, is that Miller et

al. invoke an expanded monsoon during specific times in the past, not as it is today. Also, on p. 197, the comment is made that if an increase in extreme storms were to cause aggradation, that effect should be relatively elevation-independent. It's not clear why this should be so since it is well known that precipitation is well correlated with elevation."

A5: I did not reference the Enzel et al. (2012) paper for the simple reason that the PVCH model as proposed by Bull (1991) deals with semi-arid-to-arid climatic changes, and Enzel et al. (2012) documented changes that occurred in a drainage basin (Nahal Yael) that has been hyperarid throughout the late Pleistocene. Enzel et al. (2012) framed their study as a necessary revision of the Bull (1991) model, but it is more appropriate to consider it as the first model for the geomorphic response of a hyperarid drainage basin subjected to changes in the frequency of extreme storms. As the quotes of Bull (1991) that I provided in my paper make clear, Bull intended his model to apply only to drainage basins subjected to semi-arid-to-arid climatic changes (and the vegetation changes associated with such a transition), not to drainage basins that have been hyperarid throughout the late Quaternary. Of course, Enzel et al. (2012) are correct that the application of the Bull model to the Nahal Yael is incorrect, but that fact says nothing about the applicability of Bull's model to situations in which it was intended to be applied (semi-arid-to-arid transitions) nor does it require a reevaluation of Bull's model. Some of the confusion surrounding the applicability (or lack thereof) of Bull's model to the Nahal Yael may stem from Enzel et al.'s (2012) conflation of arid and semi-arid climates. On p. 705 they refer to the application of Bull's model to "drainage basins in the arid and semiarid (50–250 mm yr–1) southwestern United States." If one assumes that the stated numerical range refers to a mean annual precipitation, the statement is incorrect because semi-arid climates have a MAP of 250-500 mm yr-1 (50–250 mm yr–1 is arid). In any case, I have included a citation of Enzel et al. (2012) in my revision along with several sentences. The added text is: "The PVCH has potential applications to other sites that have experienced a transition from semiarid to arid climates in the late Quaternary. Due to the fact that paleovegetation and the timing of fluvial-system aggradation are rarely present in the same location, however, the PVCH has rarely been tested outside of the deserts of North America (where pack-rats middens are available and have been studied for decades). An exception is the Nahal Yael, a drainage basin in southern Israel where Bull and Schick (1979) applied an early version of the Bull (1991) model. Enzel et al. (2012) recently showed that the Nahal Yael did not experience a semiarid-to-arid climatic transition. As such, the PVCH does not apply to that site. The case of the Nahal Yael underscores the importance of having reliable local paleoclimate/paleovegetation data when attempting to apply or test the PVCH." My reading of Miller et al. (2010) is quite different from that of the reviewer. Their Figure 1 shows NAM storms heading straight for the Mojave Desert under modern conditions. Actual analyses of the NAM storm tracks show no measurable influence in California (Dominguez et al., 2009). There is no text in Miller et al. (2010) that indicates they are invoking an expanded monsoon – all of the text suggests that they are invoking an increase in the intensity of monsoon activity (the word they use is "enhanced"). That said, I have added a sentence in my Discussion section that acknowledges the possibility of an expanded monsoon during late-Pleistocene to Holocene time. I have removed the comment about elevation-independence of the extreme-storm hypothesis. My point was that dissipating tropical storms trigger precipitation in both basins and ranges across the study area. However, the reviewer is correct that it is possible that extreme storms could trigger aggradation in an elevation-dependent manner.

Q6: "The paper is very well written and illustrated, and needs little editing. A few minor things require correction. On p. 197, line 7, should read "the timing of aggradation would be (or should be) relatively elevation-independent." On Fig. 4, "San Bernardino Mountains" Bernardino is misspelled. Line 26 on p. 187 should say "correctly differentiates all but one of 87"

A6: Line 7 on p 197 has been removed as suggested by reviewer 1. The typo of San Bernardino has been fixed in Figure 4. I believe line 26 on p. 187 is correct as originally

written. There is one data point in Figure 3 that has a mean/expected value that is on the wrong side of the solid curve, but to within 2σ uncertainty it is consistent with the curve (a parenthetical note has been added on this point for clarification).

Interactive comment on Earth Surf. Dynam. Discuss., 2, 181, 2014.