

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

Anonymous Referee #2

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This paper brings a new perspective to the long-term debate over what aspect of climate change effects on the landscape triggers sediment availability, transport, and deposition on alluvial fan systems in arid and semiarid environments. The long-held view, propounded originally by William Bull, is that the most recent glacial-interglacial change from late Pleistocene to Holocene caused a two-phase pulse of sediment due to a decrease in vegetation cover in the upper parts of drainage basins, which led to aggradation in the middle reaches of the basins, and the aggraded sediments were then later incised and redeposited in the lower reaches of basins as sediment yield decreased. Recent alternative explanations suggest that the timing of aggradation does not well fit the timing of vegetation change in the Mojave Desert and other nearby

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sites, and invoke increases in precipitation due to enhanced monsoon effects (changes in seasonality of storms) or to an increase in the frequency and strength of El Niño events. This paper points out that previous studies did not account for the dependence of the timing of vegetation change with altitude, and suggests that if this effect is taken into consideration, the timing of the two aggradation pulses better fits the first model of Bull. The paper is a reasonable, thoughtful contribution to this conundrum.

Scientific comments:

There are changes that, if made, can significantly improve the paper. 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. 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 three degrees of latitude and $2\frac{1}{2}$ 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

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during the late Pleistocene and Holocene.

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.

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.

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 en-

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hanced 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.

Minor technical comments:

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”.

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

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