

Interactive  
Comment

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

**J. Pelletier**

jdpellet@email.arizona.edu

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Response to Reviewer 3: (Kyle House)

Q1: “The author has done a good job demonstrating a high probability of a correspondence between diachronous elevation-dependent vegetation change and corresponding piedmont/alluvial fan aggradation. He has also provided a very nice demonstration of a regional example of a complex-response process resulting in two aggradation pulses. It is surprising that this latter fact is not touted a little more emphatically and including citation(s) to the work of Schumm and others about this important concept.”

A1: Schumm is cited in the revised paper.

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Q2: “The paper is well-written. The only exception would be a mildly awkward passage explaining the section that evaluates sites from work not in the primary geographic area of interest.”

A2: Awkward section removed.

Q3: “Title The author could consider using the term ‘elevation’ in the title.”

A3: Title has been changed to “The linkage between hillslope vegetation changes, elevation, and the timing of late-Quaternary fluvial-system aggradation in the Mojave Desert revisited”

Q4: “Work Cited The treatment of relevant literature is good aside from a glaring omission of citations relative to complex response models. I have a mild concern that the manuscript takes focused aim on the work of Antinao and McDonald to an extent that is possibly weakly antagonizing. In particular, interpretations and discussion of storm-type influences is not substantiated in this paper to an extent that allows much criticism of their ideas about those factors.”

A4: Schumm is cited in the revised paper. I have great respect for the contributions of Antinao and McDonald and do not wish to be antagonizing. There is nothing personal in my critique of their published arguments – simply a professional difference of opinion.

Q5: “Modeling Details I am not in a position to adequately evaluate the details of modeling, but am familiar with the concepts as presented and believe that the approach is a logical one. I am confident that the author’s modeling efforts are sound. I appreciate the notion of an elevation-dependent diachronous response of hill slope vegetation to climate change and suspect that all would agree that it must have some effect possibly far more significant than previously considered (I wonder what the effect of considering aspect and orientation of watersheds might be). Thus, the approach to quantifying the influence of elevation and comparing against chronostratigraphic evidence is an important step that also highlights important avenues for further research and geologic

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mapping efforts in this region.”

A5: Thank you for this statement of confidence!

Q6: “Monsoon vs. Tropical Storm Influence I have a general concern that the influence of monsoon-type convective storms may be being over-emphasized at the expense of tropical storms and even rare winter storms in the discussion about changing frequencies of extreme storms. Historically, dissipating tropical storms have had been shown to be significant in their extent and penetration into the Mojave and Sonoran deserts. They have also been shown to be sensitive to ENSO and affected by SSTs in the eastern Pacific and the Gulf of California. The Gulf of California, though proximate to the region is not the sole influence on the influxes of moisture into this region. Moreover, evaluations of regional systematic flood records and reports indicates that the dissipating tropical storm footprint is such that large numbers of small basins are impacted in single instances of storm incursion and that basins larger than about 1000 sq. km generally generate larger floods from this type of storm.”

A6: It is difficult to reconcile this comment with that of reviewer 2, who argued the opposite point, i.e. that my paper did not adequately honor the likely important role of changes in monsoon storms in driving fluvial-system aggradation in the SW US.

Q7: “Historically, the incursion of tropical moisture into the region has resulted in very significant episodes of regional flooding. Some examples include: Sept. 1939, Aug. 1951, Sept. 1970, Oct. 1972, Sept (?) 1976, October 1983, and Oct. 1997. Recommend the author familiarize himself with these storms. Some were enhanced by transit up or over the Gulf of California, but others were steered in to the region from the Pacific Ocean. These types of storms can deliver copious and intense ppt. Their dissipating remnants can spawn extremely intense convective storms under the right conditions, thus developing potential conflation with ‘typical’ monsoon conditions.”

A7: I am well aware of the importance of tropical storms in the SW US. Indeed, I have modeled the inundation from these storms in collaboration with the reviewer (e.g.

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Pelletier, J.D., L. Mayer, P.A. Pearthree, P.K. House, J. Klawon, K. Demsey, and K.R. Vincent, An integrated approach to alluvial-fan flood hazard assessment with numerical modeling, field mapping, and remote sensing, Geological Society of America Bulletin, 117, 1167-1180, (2005). Recognizing that these storms have triggered widespread flooding in the modern era is still many steps removed from the conclusion of Antinao and McDonald (2013) that changes in the frequency and/or magnitude of these storms is the primary mechanism responsible for late Quaternary fan aggradation. That said, in the revision I have removed or modified a number of phrases that the reviewer (in his annotated pdf) found to be lacking substantiation (most related to the possible role of extreme storms in driving fan aggradation in the SW US).

Q8: “One can’t help but wonder if periodic episodes of increasing activity of this sort could accompany a long-term transition in vegetation to generate the alluvial record described in this study. This is a topic worthy of further research and I suggest that the author acknowledge that fact and avoid committing to over-simplified statements about causative storm types.”

A8: Nowhere in the paper do I state that changes in the frequency and/or intensity of tropical storms did not have any effect on fluvial system aggradation and incision. In fact, I state the opposite, i.e. “It is likely that changes in extreme storms played a role in changing Mojave Desert landscapes in the late Quaternary.” I am merely arguing that the available evidence does not show that extreme storms are the primary driver of fan aggradation.

Q9: “Watershed Aspect and Elevation In my fieldwork throughout Arizona and Nevada, I have observed areas with impressively different amounts and types of modern vegetation and modern/relict colluvial deposits and soils on south vs. north facing slopes. Is the predominant aspect really not a key variable modulating the rate and extent of vegetation change over millennial time scales? Is there any evidence, say in the spatial distribution of key deposits, for this effect?”

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A9: The potential importance of aspect was noted on line 25 of page 190 in the original manuscript. I have published multiple papers on the importance of aspect. In short, I have found that aspect controls on vegetation are significant but not dominant compared with elevation differences in mountain ranges of the SW US. In the Santa Catalina Mountains, for example, the vegetation difference between relatively steep north- and south-facing slopes is equivalent to approximately 300 m in elevation change (Pelletier, J.D., Barron-Gafford, G.A., Breshears, D.D., Brooks, P.D., Chorover, J., Durcik, M., Harman, C.J., Huxman, T.E., Lohse, K.A., Lybrand, R., Meixner, T., McIntosh, J.C., Papuga, S.A., Rasmussen, C., Schaap, M., Swetnam, T.L., and Troch, P.A., Coevolution of nonlinear trends in vegetation, soils, and topography with elevation and slope aspect: A case study in the sky islands of southern Arizona, *Journal of Geophysical Research – Earth Surface*, 118, doi:10.1029/2012JF002569, 2013). 300 m is significant but not dominant considering that the retreat of vegetation over the P-H transition was more than 1 km (i.e. species found today at 1.8 km a.s.l. were at or below 0.8 km at LGM). I would have liked to have incorporated aspect into the quantitative model but unfortunately the available databases (e.g. North American Midden Database) do not encode aspect data.

Q10: “The Model Maps An aesthetic concern: The maps showing model results use a rather gloomy color scheme that is not intuitive at all to me. The lack of any kind of base map beneath further complicates my ability to interpret them. I suggest something easier to look at and understand.”

A10: The maps I made for the original paper used the Stern special color scheme. This is a popular color scheme that aficionados of visualization like because it is very effective at resolving fine distinctions within datasets that have a large range of values. Simpler two-color or three-color schemes are just not as good at making fine distinctions in data values within a range. However, I have modified the maps to a simpler red-white-blue color scheme in hopes that the reviewer finds this new scheme more pleasing (submission of revised paper pending invitation).

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Q11: In his annotated manuscript, the reviewer also asked about the potential importance of lightning-caused fires in driving erosion during the vegetation retreat to higher elevations.

A11: I appreciate this comment, since I work on the geomorphic effects of wildfires and appreciate their potential importance (e.g. Pelletier, J.D., and C.A. Orem, How do sediment yields from post-wildfire debris-laden flows depend on terrain slope, soil burn severity class, and drainage basin area? Insights from airborne-lidar change detection, Earth Surface Processes and Landforms, in press, doi:10.1002/esp.3570, 2014). However, I think that distinguishing wildfire-related from non-wildfire-related erosion from the late Pleistocene to middle Holocene is beyond the scope of this paper.

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

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