

## ***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 AE's comments:

Intro statement: I wish to thank all three reviewers and the AE for their helpful and constructive reviews. I have attempted to revise the manuscript to incorporate all of the reviewer's and AE's suggestions.

Q1: “Three thorough reviews of prof. Pelletier's manuscript have now been received. They speak of a very interesting contribution to debates about landscapes' response to climate change, and one that has been well written and presented. For me, the main points that the reviewers raised, and that I feel that the author should at least include

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in his changes to the document, are the following: 1. A perceived lack of context about the study site. Why was this site chosen? How does it compare to other sites, in terms of its inferred climate-landscape relations? What makes it interesting from a wider perspective? Which caveats must be taken into account when transferring the paper's findings to other worker's study sites? This point must particularly be looked at from the point of view of an international audience that is unfamiliar not only with the study site itself, but also with its American geographical context.”

A1: In the revised manuscript I have expanded on why the site was chosen and what makes it interesting from a wider perspective. The richness of the paleovegetation and geomorphic records in the Mojave Desert make it a rather unique area in terms of its ability to test process-response models for geomorphic response to semiarid-to-arid climatic changes. It's uniqueness is one reason why a paper that focuses on the Mojave Desert should not be considered inappropriate for an international journal such as ESurf. The model may apply to other landscapes that have undergone semi-arid-to-arid transitions outside the U.S. but it is beyond the scope of my expertise to comment on such potential applications. I appreciate the fact that ESurf is an international journal and it is worth noting that some of the discussion in my papers deals with sites in Mexico. Here is the text I have added in response to this concern: “In this paper I focus on the timing and mechanisms of fluvial-system aggradation and incision in the Mojave Desert portion of the southwestern U.S. from the latest Pleistocene to the present. I focus on this study area and this time interval because the constraints on both fluvial-system behavior and its potential driving mechanisms are arguably better constrained than for any other area and any other time interval in the world that has experienced a semiarid-to-arid climatic change. For example, the timing of local paleovegetation changes is unusually well constrained, i.e. 87 dated packrat middens within the central Mojave Desert exist with woodland species (*Juniperus*) clearly present or absent from 17-0 ka). Also, dozens of state-of-the-art stratigraphic and surface-exposure ages have been obtained (e.g. Miller et al., 2010; Antinao and McDonald, 2013 and references therein). Semiarid-to-arid climatic transitions are of particular interest given that

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semiarid landscapes may be particularly sensitive to climatic changes (e.g. Langbein and Schumm, 1958) and because large portions of Earth's surface have the potential to transition from semiarid to arid climates in the future (e.g. Held and Soden, 2006; Lau et al., 2013). More broadly, drainage basin responses to climatic changes are mediated in large part through changes in vegetation cover, and understanding the feedbacks between vegetation cover and landscape evolution has emerged as a "grand challenge" problem in Earth surface science (e.g. Murray et al., 2009; Reinhardt et al., 2010). As such, understanding the late Quaternary record of fluvial-system response to climatic changes in the Mojave Desert has the potential to enhance our conceptual understanding of how vegetation cover and landforms coevolve in other process zones."

Q2: "2. Despite the clear, and clearly appreciated efforts to quantify climate-landscape relations, more attention is due to the effect of choices made in the altitude-climate relation. Reviewers mentioned various potential shortcomings of the assumption that the author makes in this regard. It would be worthwhile to explore the effect of this assumption on the results (i.e. the timing of geomorphic activity across the study site at the end of the LGM)."

A2: See specific responses to each reviewer on this point.

Q3: "3. In addition to the effect of my point 1 on the literature that is cited, I feel that more attention is due to the body of literature surrounding the complexity of landscape response to climate change. Reviewers provide examples for this, with the work of the late Stanley Schumm chief among them. More modern examples would include Murray AB, Lazarus E, Ashton A, Baas A, Coco G, Coulthard T, Fonstad M, Haff P, McNamara D, Paola C, Pelletier J, Reinhardt L (2009): Geomorphology, complexity, and the emerging science of the Earth's surface. *Geomorphology* 103, 496-505, and Reinhardt L, Jerolmack D, Cardinale B, Vanacker V, Wright J (2010): Dynamic interactions of life and its landscape: feedbacks at the interface of geomorphology and ecology. *Earth Surface Processes and Landforms* 35, 78-101."

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A3: In the revised manuscript I have cited Schumm, Murray et al. (2009), and Reinhardt et al. (2010). I agree that not citing Schumm especially was a major oversight on my part.

I have included a pdf of the proposed text of the revised paper. Note that several figures have also been modified (submission pending an invitation to revise) as detailed in the response to reviewers.

Please also note the supplement to this comment:

<http://www.earth-surf-dynam-discuss.net/2/C123/2014/esurfd-2-C123-2014-supplement.pdf>

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