Earth Surf. Dynam. Discuss., 2, C102–C108, 2014
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Earth Surface Dynamics Discussions Open Access

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2, C102–C108, 2014

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

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Received and published: 27 May 2014

Response to Reviewer 1:

Q1: "1. As someone not strongly familiar with the study region (I have briefly visited the study region so have some feeling for the landscape and its complexity) the paper relies on the reader having some considerable familiarity with the sites and associated literature. The paper attempts to summarise current ideas on landscape change with relevant literature cited, however, there is no attempt to place the site and region in a global environmental change context. What is the significance of the site and the information obtained in the global context? The reader is left with the impression that any knowledge gained is site specific? Further, as the paper is examining past climate, vegetation and landscape change, can some comment be made about future landscape trajectory? As ESurf is an international journal, the above information will make the paper much more accessible and significant for those readers outside North America. It would be relatively easy to do this both in the Introduction and Conclusion."

A1: See response A1 to AE. Also, I have included a shaded relief base map as Fig. 4A in the revised manuscript (pending) so that the physiography of the study area is clear to the general reader.

Q2: "2. The Introduction is very complex to someone not familiar with the study site literature. The PVCH hypothesis is outlined as well as the complex aggradation/depositional processes and vegetation change at the study sites. However, upon reaching the end of the 2nd last paragraph of the Introduction the work of Pierson et al is introduced where the PVCH is discussed. Here the reader becomes a little confused as it appears that the author is discounting (or seriously questioning) the PVCH hypothesis based on this work. Yet, the last paragraph states that the PVCH is the correct hypothesis/model? An overall comment is that the Introduction is trying to cover a lot of previous studies, theories and introduce the authors ideas yet the detailed intent of the paper is quite ambiguous. This is also reinforced by the readers lack of detailed knowledge of the study site (as will apply to countless others) and the work of the others cited. The Introduction really needs a better focus."

A2: I tried hard to make the original Introduction understandable to someone not familiar with the primary literature of the study site. For example, I went to the unusual step of quoting key passages directly from the two previous most relevant works (Bull, 1991 and Antinao and McDonald, 2013) so that readers would not have to know the key points of those papers prior to reading my paper. I am not sure how to make the Introduction clearer. Towards the end of the Introduction I do question the mechanism (not the timing) of the PVCH as proposed by Bull (1991) because changes in drainage density with vegetation cover are needed to match observations and such changes

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were not part of the Bull (1991) model That I am proposing a modification of the PVCH should come as no surprise to the reader since I stated in the Abstract that I would be proposing changes to the PVCH model, i.e. “I also present an alternative process model for PVCH that is more consistent with available data and produces sediment pulses primarily via an increase in drainage density (i.e. a transformation of hillslopes into low-order channels) rather than solely via an increase in sediment yield from hillslopes.” The last sentence of the Introduction again stated that changes to the PVCH are necessary, i.e. “I also present an improved process model of how the PVCH works that includes transient changes in drainage density associated with semiarid-to-arid climate transitions.” Therefore, I think I have made it as clear as possible that my analysis fully supports the timing of the PVCH but that I am proposing an alternative process model for how the PVCH works.

Q3: “3. Section 2, page 188 and onwards. This is really the basis of the paper. The intent is of this section is clear. However, from line 6 on p188, it is really difficult for me to follow what has been technically done. It is really not all that clear what the role MFDs plays as opposed to Dinfinity of even D8. On such a landscape scale and it may be argued that drainage direction method is irrelevant. Please explain in more detail why this is explained. Section 2. Page 187 (Lines 1-5) and Page 189 (lines 1-15). This is difficult to decipher clearly from the literature review but can the reasons for the different timing be briefly discussed? It is not really clear how you did the GIS analysis and how and why and relevance of the various thresholds used. The paragraph starting line 8 page 189 then leads into a very complex discussion of time lags relating to aggradation. Overall, I have tried to read this section many times and every time, I have come away a little bewildered and not really certain what has been done and how.”

A3: I have rewritten the description of the GIS analysis in an attempt to be clearer. Here is the revised text: “The GIS analysis that predicts the timing of the onset of primary aggradation and the timing of channel-incision/onset-of-secondary-aggradation begins

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by associating each pixel in the source catchments with the age of the woodland-to-desert scrub transition using the solid curve in Figure 3. Then, for each pixel in the study site, the percent of the upland source catchment that has undergone a change from commonness/abundance to rarity/absence of Juniperus is computed in 1 ka intervals starting at 15 ka cal BP. For example, at 15 ka only the lowest elevations (i.e. those less than or equal to 750 m a.s.l.) of source catchments have undergone a change from commonness/abundance to rarity/absence of Juniperus, hence pixels downstream of such basins will record relatively low percentages for the percentage of the upstream source catchment basin that has undergone such a change. As time passes and woodlands retreat to higher elevations, the GIS analysis computes a progressively larger percentage of each source catchment that has transitioned from commonness/abundance to rarity/absence of Juniperus. The GIS analysis requires a map of the source catchment domain for each downstream pixel. The mapping of contributing area is performed in this study using the multiple-flow-direction (MFD) algorithm of Freeman (1991). I used the MFD method of Freeman (1991) to delineate source areas because it more faithfully represents flow routing pathways in distributary channel systems (which are common in the study area) compared with alternatives such as the $D\infty$ method (Pelletier, 2008). When 5% of the source catchment area has undergone a change from commonness/abundance to rarity/absence of Juniperus, primary aggradation is assumed to be initiated. Similarly, when 50% of the source catchment area has undergone a change from commonness/abundance to rarity/absence of Juniperus, channel-incision/secondary-aggradation is assumed to be initiated. The 5% and 50% threshold values are not unique, but the results are not highly sensitive to these values within reasonable ranges, i.e. the predicted ages of aggradation and incision differ by 2 ka or less for 96.5% and 98.6% of the study area where aggradation is predicted to occur as these values are varied over reasonable ranges from 5% to 15% and 40% to 60%, respectively. The systematic uncertainty of the GIS analysis is taken to be approximately 2 ka (Table 1) based on the uncertainty associated with the 5% and 50% thresholds as well as the uncertainty of the timing of the woodland-to-desert scrub transition at each elevation (i.e. the solid curve in Fig. 3).” I am not sure why the reviewer would claim that the drainage direction method is irrelevant – there are significant differences in the computation of drainage basin area based on flow routing method, as stated in the original paper. The sensitivity of contributing area to flow routing method has been documented by many studies (e.g. Wilson, J. P., Lam, C. S. and Deng, Y. (2007), Comparison of the performance of flow-routing algorithms used in GIS-based hydrologic analysis. Hydrol. Process., 21: 1026–1044. doi: 10.1002/hyp.6277; Pelletier, J.D., Quantitative Modeling of Earth Surface Processes, Cambridge University Press, 2008, and references therein).

Q4: “4. Section 3. Equation 1 and onwards. It can be seen what is trying to be done here however, the link with the work of Prosser Dietrich in different soils and climate is tenuous as the Prosser study was done on the west coast with different soils and climate. Are the findings actually transferable? What follows in the paragraph starting line 15 on page 192 is not at all clear nor is what Figure 9 is trying to say. The reader is left with the impression that there has been some landscape evolution modelling done or about to be done – but there is only speculation. This needs to be made clearer.”

A4: Section 3 begins with data from the SW US (e.g. Melton, 1953) and makes use of data from the semi-arid-to-arid SW US to the greatest extent possible. The only result from Prosser and Dietrich that I am claiming is transferable to the Mojave Desert is the fact that the critical shear stress for erosion is sensitive to percent bare area. I have added an additional reference (Al-Hamdan et al., 2013) in the revision that provides theoretical/modeling support for the concept that the critical shear stress for erosion is sensitive to percent bare area in landscapes generally. The new text is: “The results of Prosser and Dietrich were obtained in the relatively humid climate of coastal California, but Al-Hamdan et al. (2013) developed a theoretical model that suggests the sensitivity of erosion to bare area is a general phenomenon.” Landscape evolution modeling has indeed been done. As my paper states, “Numerical models (e.g. Tucker and Slingerland (1997)) predict that sediment pulses produced via an increase in drainage density

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are temporary (i.e. they are sediment pulses with both waxing and waning phases) because first-order valley heads eventually stabilize (following a transition from lower to higher drainage density) as they become adjusted to a smaller contributing area associated with the lower vegetation cover and hence lower shear stress threshold for detachment of arid climates. The stabilization of valley heads following drainage network expansion causes a reduction in sediment supply that triggers channel incision, terrace abandonment, and secondary aggradation further downstream (Tucker and Slingerland, 1997)." I have expanded the description of Figure 9 in the revision in an attempt to be clearer: "Figure 9 presents a schematic diagram of an alternative conceptual model for the PVCH that includes elevation and is qualitatively based on the results of numerical modeling studies (e.g. Tucker and Slingerland, 1997). This figure shows variations in sediment yield (bottom panel) resulting from hypothetical changes in mean vegetation cover (top panel) and drainage density (middle panel) for three hypothetical drainage basins with elevations from 800-1000, 900-1100, and 1000-1200 m a.s.l. The lowest drainage basin (800-1000 m a.s.l.) experiences the initiation of aggradation first (ca. 15 ka cal BP) because the loss of Juniperus is time transgressive, proceeding from low to high elevations through time. Numerical models predict that sediment pulses produced via an increase in drainage density are temporary (i.e. they are sediment pulses with both waxing and waning phases) because first-order valley heads eventually stabilize (following a transition from lower to higher drainage density) as they become adjusted to a smaller contributing area associated with the lower vegetation cover and hence lower shear stress threshold for detachment associated with arid climates. As such, the accelerating increase in drainage density that occurs in the 15-12 ka time interval for the lowest drainage basin is accompanied by an increase in sediment yield to a peak value ca. 12 ka (Fig. 9). Numerical models demonstrate that the stabilization of valley heads following drainage network expansion causes a reduction in sediment supply that triggers channel incision, terrace abandonment, and secondary aggradation further downstream (Tucker and Slingerland, 1997) (ca. 12 ka for the lowest drainage basin). The response of the higher-elevation drainage basins is similar except that aggradation is delayed relative to lower-elevation drainage basins and the duration of primary aggradation is predicted to be somewhat shorter as a result of the increase in the rate of the recession of Juniperus at higher elevations (1100-1800 m a.s.l.) relative to lower elevations."

Q5: "5. The figures are well constructed and appropriate for the paper but it is very difficult to interpret Figure 4 and 5 and Figure 10b. The intention of the figures is clear but for someone not intimately familiar with the geography of the study region they are near impossible to interpret."

A5: I have included a shaded-relief base map for Figures 4, 5, and 10B in the revised paper, and I have modified the color scheme of these figures to make them easier to interpret.

Q6: "6. Other minor issues. What is the Landfire data base? It is recommended that a reviewer with detailed knowledge of the study area also review this paper as there are many site specific details that require specialist knowledge."

A6: LANDFIRE is a U.S. Geological Survey mapping program that produces high-resolution geospatial data for vegetation and fire regimes in the U.S. Any reader can easily discern what the LANDFIRE database is by doing an internet search on the word "landfire" (the USGS database is the first page returned). I have added the following sentence to the revised paper: "LANDFIRE is a U.S. Geological Survey mapping program that produces high-resolution geospatial data for vegetation and fire regimes in the U.S."

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