

## ***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 #1**

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Review of ‘The linkage between hillslope vegetation changes and late-Quaternary fluvial-system aggradation in the Mojave Desert revisited’ by J. D. Pelletier

This paper examines the complex issues of how landscapes respond to climate change. This knowledge is vitally important not just from a scientific viewpoint but also for how landscapes may respond under different future climate scenarios. Unravelling the complexities of such systems is a difficult yet necessary task. This paper examines landscape and vegetation change in the Mojave Desert in the USA. It investigates different theories behind landscape change using existing data sets examined in a GIS framework. However, there are a number of areas where the paper could be improved, largely to better enhance readability and access to those not familiar with the study

C54

site. This is described below. 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.

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.

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

C55

as opposed to D-infinity 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.

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.

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.

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.

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