Review of revised manuscript for Earth Surface Dynamics, esurf 2016-62: Mouslopoulou et al., "Distinct phases of eustatism and tectonics control the late Quaternary landscape evolution at the southern coastline of Crete"

Reviewer: Mark Brandon, Yale University June 6, 2017

Overview

I have read through all of the available reviews, short comments, and replies, and the current revised manuscript and associated summary about the revisions. My conclusion is that this manuscript makes a useful contribution about the stratigraphy and age of alluvial units in an interesting locality in SW Crete. The data are original, well documented, and very nicely illustrated, and they will be useful for future geomorphic and tectonic studies. Crete is also widely recognized as an important active tectonic setting. Thus, I strongly recommend that the manuscript be "accepted", which I guess means moving it from discussion to publication status.

A shortcoming, however, is that the manuscript, in its revised form, fails to establish an intellectual framework for the work. Why should the reader be interested in the paper? What are the specific competing ideas or hypotheses? The introduction focuses on eustasy versus tectonics, but there are no explanation about why this issue is important. In their reply to comments, the authors indicate that they are mainly focused on tectonic not geomorphic issues. But if the focus is on tectonics, then I would expect an introduction that summarized competing tectonic ideas, and a discussion that showed how the new data tested those ideas. At present, the main contribution is that the rock-uplift rate is about 2.5 mm/a in a small location in Crete. There are a couple of vague sentences about steady versus unsteady uplift. One would think that Crete, which lies above an active subduction zone, would be an ideal setting for testing tectonic interpretations.

I follow here with some specific feedback, which the authors might find useful for sharpening their presentation.

Specific Comments

1) The core contribution of this manuscript is strongly challenged by the short comments from Gallen and Wegmann. Their most significant challenge is to suggest that the Domana fan units are not 20 to 30 ka, as proposed by the authors, but rather <10 ka. The authors need to directly address this issue in the manuscript. It is important to remember that the short comments and reviews will be published along with the paper, so they can be directly addressed and cited in the paper.

2) Based on my reading, I side with the authors for their interpretation of the age of the Domata fan units. Gallen and Wegmann argue for a Holocene age based on relationships relative to a "Holocene" (?365 AD) sealevel notch. The authors clearly show that the notch is probably not equivalent to the unconformity

beneath the lower fan unit. I am also convinced by the IRSL Kspar ages. These ages are messy, but each of the five ages has a well-defined minimum-age component. The clustering of a minimum-age component in each sample suggests a similar bleaching history for all of these grains. That suggests to me full bleaching for this component. In addition, the IRSL ages appear to be consistent with the stratigraphic ordering of the samples. (It would help to organize the data plots in Figure 7 in stratigraphic order, and to discuss this relationship in the text.)

3) Rex Galbraith would blow a gasket if he read this citation to the Galbraith and Roberts (2012) paper (p. 8, line 30), in that citation is used to justify using the mode in the density plot (as determined by the KDE method) as an estimate of the age. Galbraith has spent his career lecturing against the use of probability density plots. In fact, the Galbraith and Roberts review recommends estimating "minimum ages" for mixed OSL grain-age distributions. If the authors want to use the mode of the probability density plot as an estimate for the age, then they should remove the Galbraith and Roberts' citation. Alternatively, they could calculate minimum ages, and, in that case, the Galbraith and Roberts citation would be appropriate. My recommendation is to use the minimum age estimate. That approach provides a clean way to deal with mixed grain ages.

4) The manuscript would be stronger if it had a more critically developed interpretation (see section 4). As presently written, the interpretation read as a "just-so" story (i.e. a self-consistent narrative). The reader is left to wonder if there might be other interpretations that fit the data. In addition, I would expect that if the issue of steady versus unsteady uplift is important, then the interpretation should address this issue as well.

5) The manuscript contains many cases where an argument is advanced on the basis of what might be "likely" or "unlikely" to have occurred (see below for partial list). My sense is that these terms are used to indicate what the authors view as reasonable and unreasonable aspects of their interpretations. Unfortunately, this phrasing tends to make the argumentation sound weak. I would recommend stating the interpretation in direct terms and to avoid personal assessments of whether the ideas are likely or unlikely. That judgment is probably best left to the reader.
    p. 10, line 20: The initiation of deposition of the upper-fan (Fig. 10a) is likely to have occurred post ~50 kyr BP and prior to 45 kyr
    p. 10, line 23: We argue that upper-fan deposition is unlikely to have started as early
    p. 11, line 14: "Marine trimming of the lower-fan surface and deposits is unlikely to have occurred
    p. 11, line 16: "The Holocene high sea-level stand is the most likely candidate period

6) The term casual “marine bench” is widely used throughout the paper. The term “marine terrace” or “marine abrasional surface” is probably more precise. Also, it would be useful to know if there is direct evidence that these “flats” were cut in the surf zone, as opposed to being fluvial straths. I suspect that this distinction was made by considering the context of the flat relative to the coast and adjacent river channels. Nonetheless, it would probably help to know if the marine versus fluvial origin of these flats included evidence based on fossils or deposits.

7) The title includes the noun “eustatism” as a modifier. I would suggest using the adjective, “eustatic”. For example, “Distinct phases of eustatic and tectonics forcing for late Quaternary landscape evolution in southwest Crete”.

Recommendations for Figures

Caption for Figure 1: “Numbered arrows show geodetically-derived convergence rates between the African and Eurasian plates and their azimuths at selected sites (after Reilinger et al., 2010).” >>> “Labeled arrows show geodetically-derived site velocities (mm/a) relative to a fixed Nubia plate (Reilinger et al., 2010).”

Figures 3, 5, 6: Change “upper fan surface” and “lower fan surface” to “upper fan tread” and “lower fan tread”. Surface is vague. Tread clearly indicates the uppermost limit of an inset geomorphic unit.

Figure 4:

1) Change labels as such: profiles > profile (Fig. 5 shows only one profile for
2) The label “marine cliff profile” should refer to Fig. 5b (not 5a).

Figure 6a:

1) My understanding is that the “upper fan gravels” are inset into the “lower fan gravels”. It might help to use a different contact line to indicate an inset relationship. Perhaps something like TTTTTTTTTT (like a normal fault contact) with the barbs pointing to the inset unit.
2) In the lower panel, the “marine bench” is shown as grading into the “upper fan gravels”. This relationship is inconsistent with the description in the text. The line work needs to be modified (or the text rewritten) to resolve this conflict.
3) The term “marine bench” is a bit confusing. Does it refer to the wave-cut unconformity (marine erosion) or does it refer to a depositional unit below the unconformity? The position of the label suggests that it is depositional unit. Is there evidence that this unit was a marine deposit?

Figure 6b: This figure is a bit confusing and incomplete.

1) Unit labeled “rockfall debris post-dating upper fan deposition” lies within the dashed line that encircles the upper and lower fan deposits. I recommend

placing the dashed line below this "rockfall debris" unit, to make it clear that it is not part of the two fan units.

2) I recommend showing here the "Older Quaternary deposits" unit in the lower left side of this image (see Figure 6c).
3) The unconformity at the base of the fan units is not highlighted, and the "beach bench" unit is not labeled.

Figure 6c: The "Older Quaternary deposits" are not dated, so it is best to call them "older deposits" or "older sediments".

Figure 7

1) What are the numbers on the right axis of these plots? I suspect that they show the cumulative distribution in terms of "Cumulative Grains". This right axis need a label.
2) Left axis is labeled "kernel density estimate" but the units are not specified. Of course, density plots do not require any units since they are only used to indicate a relative sense where the location of the probability mass of the dated grains. If scaling is needed, then I recommend, for simplicity, to leave this axis blank. If the authors do insist on labeling this axis, then it should be called "probability density" and the units should be shown (dP/dTau where P is probability and tau is age; could use density units of grains/ka or %/ka).

Table 1.

1) The columns showing locations should be labeled "Longitude" and "Latitude". Easting and northing refer distances, as used for UTM coordinates.
2) The term "mode" (in the sense of mode of the probability density distribution) should be used instead of "KDE max".