Detailed Response to the Reviewers & Associate Editor

Ms. Ref. No.: esurf-2016-62

Title: Distinct phases of eustatism and tectonics control the late Quaternary landscape evolution at the southern coastline of Crete

Dear Associate Editor, *6 July 2017*

We have now completed the revision of the revised version. The comments of Reviewer-1 were easily addressed and the comments made by Reviewer-2 were straight forward and very useful. The most important change is the calculation of minimum ages, as recommended by Prof. Brandon.

In the following paragraphs, we carefully address the comments and suggestions of:

- *the Associate Editor (Prof.* Vanacker*),*
- *Each of the two reviewers of the revised version of our manuscript (Dr. Tiberti and Prof. Brandon)*

Our responses appear in 'red italics' while the comments of the Reviewers/Associate Editor appear in black.

In the uploaded revised-revised version we have:

- *Included additional text in the introduction, end of section 3.1 and section 5 (all tracked)*
- *Modified Figure 2 (replaced 'marine bench' with 'marine terrace').*
- *Modified Figure 3 (replaced 'upper/lower fan surface' with 'upper/lower fan thread').*
- *Modified Figure 4 (removed 's' from the word profile).*
- *Modified Figure 5a (replaced 'upper/lower fan surface' with 'upper/lower fan thred' and 'sea-level' with 'abrasion surface').*
- *Modified Figure 5b (replaced 'wave cut bench' with 'marine abrasion surface').*
- *Modified Figure 6a (replaced 'marine bench' with 'abrasion surface'. Also modified the white bedding in the lower panel).*
- *Modified Figure 6b (modified the white and red lines according to the reviewer's request).*
- *Modified Figure 6c (replaced 'older Quaternary deposits' with 'older deposits').*
- *Provided a new Figure 7, including also Minimum ages, as requested by Prof. Brandon.*
- *Modified Table 1.*
- *Recalculated minimum ages.*
- *Answered all minor comments of both reviewers (see below).*

In summary, we do hope that this revised-revised version, together with our detailed response to the review comments, will convince you that our work warrants publication in the Journal of Earth Surface Dynamics.

Dr. Mouslopoulou and co-authors.

Response to the comments of Associate Editor, Prof. Veerle Vanacker

Based on the comments, I find that your manuscript may be suitable for publication after revisions.

1. To quote reviewer 2: "the manuscript fails to establish an intellectual framework for the work". This comment was already raised in the discussion phase, and reviewer#2 suggests a possible way to sharpen the focus of the paper in the introduction.
Addressed. Please see response to Prof. Brandon's introductory comment.

2. The chronology of the Domata fan units as presented in the paper is challenged by the comments from Gallen and Wegmann. Their viewpoint and arguments need to be addressed in the paper.
We were satisfied to see that Prof. Brandon is also convinced, as is Dr. Tiberti, of the non-Holocene age of the Domata fan (that was the main 'challenge' posed to our work by G&W). We have taken the challenge from G&W that these fans may be of Holocene age seriously and considered all angles of available evidence. We remain unconvinced that this is a possibility. In this revised-version, we have followed Prof. Brandon's, as well as your, advice to directly deal with G&W's comments and have added a paragraph at the end of section 3.1 that we believe leaves no doubt that their argument has some serious flaws.

3. Given the uncertainty on the age estimates, reviewer#2 suggests to "calculate minimum ages". The minimum age components can then be used in the discussion.
Done. We have calculated minimum ages as requested by the reviewer (see response to Prof. Brandon's comments below).

Reviewer≠1: Dr M.M. Tiberti, INGV, Italy

We thank the reviewer Dr. Tiberti for her positive feedback. Below we show that we corrected all three points that she made.

I suggest just some more technical corrections:

- Page 2 Lines 30-31: too many repetition of the word "*episodes*" in this sentence, please correct
  Corrected.
- Page 4 Line 11: "*alluvial fans*" instead of "*an alluvial fan*"
  Corrected.
- Figure 7: remove "*OSL*" from x-axis label
  Corrected and entire Figure 7 is now replaced.

Reviewer≠2: Prof. Mark Brandon, Yale University, USA

We thank the reviewer Prof. Brandon for his very useful comments which improved our manuscript. Below we answer to all of his points and remarks. Note that we have accommodated all of his comments/recommendations.

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"

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.

Thank you.

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.

We completely agree with Prof. Brandon. We have now modified accordingly the introduction and Section 5 (see details below) but we would also like to add the following remark as part of our response to Prof. Brandon's comment:

An island that lies above an active subduction margin is, indeed, a natural 'seismograph', capable of recording vertical motion due to large-magnitude earthquakes. This is why, inspired specifically by Crete, we recently published an article revealing transient uplift-rates along the Hellenic margin due to clusters of mega-earthquakes on upper-plate faults (Mouslopoulou et al., 2015; GRL) and later tested successfully this idea on several other margins globally (Mouslopoulou et al., 2016; Tectonics). This is to clarify that we have performed already a nearly exhaustive analysis on the subduction seismogenesis (including stable vs. transient uplift) of the Hellenic margin.

And that was possible because we had collected uplift rate data through time from individual localities along and across the margin.

Here, at Domata, we only have data for one uplift-rate, deriving from a single time-period (ca. 39 kyr BP). This is still very valuable, for the reasons stated in Section 5 of the current manuscript, but by having only one data-point, we can't independently test different models (e.g. steady-state uplift vs. transient uplift), as proposed by Prof. Brandon (and we did in our GRL paper).

However, what we have done in this current article, that adds value to this Esurf work, is to combine the tectonostratigraphic information that derive from Domata with published

tectonic uplift-rates in order to place constraints on the interaction between eustacy and tectonics and test the idea put forward by Pope et al. (2016) that tectonics do not play a major role in the landscape evolution in southwest Crete. This is now stated clearer in the introduction [Page 2, lines 25-31] and Section 5 [P. 12, lines 28-30; P. 13, lines 32-35; P. 14, lines 4-5] of the revised manuscript. We hope that these additions give a more generic flavour to our work.

References cited above:

Mouslopoulou, V., Nicol, A., Begg, J.,Oncken, O., Moreno, M., 2015.Clusters of mega-earthquakes on upper plate faults control the Eastern Mediterranean hazard. Geophysical Research Letters, 42, 10,282–10,289, doi:10.1002/2015GL066371.

Mouslopoulou, V., Oncken, O., Hainzl, S., Nicol, A., 2016. Uplift rate transients at subduction margins due to earthquake clustering. Tectonics, 35, 2370–2384, doi:10.1002/2016TC004248.

Pope, R. J. J., Candy, I., and Skourtsos, E.: A chronology of alluvial fan response to Late Quaternary sea level and climate change, Crete, Quatern. Res., 86, 170-183, 2016.

New Reference added to the revised version:

Mouslopoulou, V., Oncken, O., Hainzl, S., Nicol, A., 2016. Uplift rate transients at subduction margins due to earthquake clustering. Tectonics, 35, 2370–2384, doi:10.1002/2016TC004248.

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.

Yes, in this revised version, we have followed Prof. Brandon's advice to directly deal with Gallen and Wegmann's comment and have added a paragraph at the end of section 3.1. We trust that now everything is clear.

2) Based on my reading, I side with the authors for their interpretation of the age of the Domata fan units [thank you]. 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. *[Yes, we use Figure 6c to make this point even clearer now in the revised manuscript].* I am also convinced by the IRSL Kspar ages *[thank you]*. 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 [exactly]. 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 [exactly!]. (It would help to organize the data plots in Figure 7 in stratigraphic order, and to discuss this relationship in the text.) *Done.*

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.

The reviewer proposes to calculating minimum ages. Indeed, we have now calculated two new age models: the Central Age Model and the Minimum Age Model (see updated Table 1). We removed the KDEmax column. We agree that this value is not distinct as it depends on the band-width used.

The 'new' ages are consistent with the 'old' in indicating a last glacial age (MIS3) for all samples and they conform to the geological sequence established.

We have added the following text at the upper-half of Page 9:

'The Central Age Model (CAM) and the Minimum Age Model (MAM) according Galbraith et al. (1999) were used to further describe the age distributions. Minimum age models are recommended when dating mixed-age sediments yielding broad age distributions to better estimate the population of well bleached grains (Galbraith and Roberts, 2012)'.
and…

'All Mean values indicate a last glacial age for all samples and appear in stratigraphic order. Median values are all in stratigraphic sequence (except for LF-2a/b) although they are significantly younger than the Mean and the CAM values. The CAM ages are consistent with the Mean and Median values in indicating a last glacial age (MIS3) for all samples. The CAM series data are all in stratigraphic order (except for the LF-2a/b sample), and the absolute values are younger than the means. The MAM ages "lean" towards the younger end of the timescale, which is to be expected, because they are the minimum possible ages, not the most likely ages'.

New reference:
Galbraith, R. F., Roberts, R. G., Laslett, G. M., Yoshida, H. & Olley, J. M. (1999): Optical dating of single and multiple grains of quartz from Jinmium Rock Shelter, northern Australia: Part I, experimental design and statistical models. In: Archaeometry, 41: 339 – 364.

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. *Addressed together with the following 'Comment 5'. We have modified the text at several locations to make it sound less narrative.*

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 *Done.*
p. 10, line 23: We argue that upper-fan deposition is unlikely to have started as early *Done.*
p. 11, line 14: "Marine trimming of the lower-fan surface and deposits is unlikely to have occurred *Done.*
p. 11, line 16: "The Holocene high sea-level stand is the most likely candidate period *Done.*

We have modified the sentences indicated above, and also the caption of Figure 5, to make our interpretation sound more solid (and less narrative).

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.

Done. We have deliberately chosen the non-genetic term "abrasion surface" for this feature as it describes well its morphology without dictating a mode of origin. While there are no marine deposits or fossils associated with the abrasion surface, we conclude from its geometry (and that of the nearby features) that it is marine in origin. Firstly, a cliff incised in the upper fan deposits about 50 m behind the current coastline lies entirely parallel with the present coastal cliff. The older coast-parallel cliff is truncated in the west by an alluvial entrenchment cliff of the Klados River at a high angle (almost orthogonal) to it. The river entrenchment cliff defines the course of the river at a time following deposition of the upper fan and it is barely conceivable under any circumstance that such a steep, unobstructed river course might take such a right angle deviation required to be responsible for erosion of that cliff. Further, as the abrasion surface incised in the upper fan deposits is sub-horizontal across the entire beach-front, there is little or no possibility that this cliff and the abrasion surface could have been eroded by the Klados River. The relationship between the presently active Klados River entrenchment cliff and coastal cliff in the lower fan deposits is entirely analogous. We conclude that the abrasion surface represents a bench in the upper fan deposits that was cut in the surf zone in front of the contemporaneous marine cliff.

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".
Done.

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)." >>> "Labelled arrows show geodetically-derived site velocities (mm/a) relative to a fixed Nubia plate (Reilinger et al., 2010)."
Done.

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.
Done.

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).
profiles > profile Done!
However, reference to Fig. 5a is correct (as each of the two profiles in Figure 4 corresponds to those presented in Figure 5a –and not 5b)!

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.
Done.

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.
Done.

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?
We have changed the label of the feature from marine bench to "abrasion surface". We also changed the position of the label so that it sits on top of the feature. Please also see our response to your 6th comment, that the feature is erosional and it does not refer to the deposits below (they are remnants of the upper fan deposits, and they are alluvial, not terrestrial).

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.
Done.

2) I recommend showing here the "Older Quaternary deposits" unit in the lower left side of this image (see Figure 6c).
Done.
3) The unconformity at the base of the fan units is not highlighted, and the "beach bench" unit is not labeled.
The unconformity at the base of the fan units is represented here by the onlapping of both fan units against the basement rocks (dashed white line). We have now replaced the term 'marine bench' with 'marine terrace' or 'marine abrasion surface' throughout the manuscript for consistency (as requested by the reviewer earlier).

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

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.
Figure 7 is now replaced according to the new age calculations (see response to Reviewer's comment 3. The right axis is labelled 'cumulative IRSL ages'.

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).
Despite the concern of the reviewer, we confirm that the labelling of the left axis as 'kernel density' is correct.

Table 1.

1) The columns showing locations should be labeled "Longitude" and "Latitude". Easting and northing refer distances, as used for UTM coordinates.
   Done.

2) The term "mode" (in the sense of mode of the probability density distribution) should be used instead of "KDE max
   We removed the KDEmax column completely from Table 1. See our response to Reviewer's 3rd comment above.