Response to the short comment of S. Gallen and K. Wegmann

We take this opportunity to thank S. Gallen and K. Wegmann for their comments on our manuscript.

Before we respond in detail to their comments, we would like to make a general statement about the objectives of our work and constrain its scope. This may help allay many of the concerns they expressed.

Objectives:

Our primary interest in our work on Crete is to help constrain its late Quaternary tectonic development. With this work we aim to use first order geomorphic features and stratigraphic relationships to understand the late Quaternary vertical deformation of western Crete. In doing so, we recognised the importance of the Domata fan sequence and the important marine bench cut in upper fan at Domata. This feature allows us to independently derive a Late Quaternary uplift rate from this section of western Crete. The paper is important because of this independence from previously derived uplift rates on western Crete (e.g., Shaw et al., 2008; Strasser et al., 2011; Tiberti et al., 2014; Mouslopoulou et al., 2015 – all references included in the submitted version).

This objective does not include a comprehensive description of the materials of the Domata fans and their developmental chronology, as has been undertaken for other fans of southern Crete by others (e.g., Nemec & Postma, 1993; Gallen et al., 2014; Pope et al., 2008, 2016). Neither is the objective to compare Domata fan stratigraphy with that of other alluvial fan systems on Crete. We welcome the prospect of additional work that would enhance insight into the processes of fan deposition and a refined chronology that may help better understand relationships between sediment transfer rates and climate in this area.

Methods:

To achieve these objectives, we needed to understand the first order fan geomorphology and from it dissect out the sequence of events required in landscape evolution. We derived a basic sequence of events that was demanded by the stratigraphic relationships present at Domata and present these in the paper. We have gone to some effort to place the Domata fan sequence within its stratigraphic and chronologic context so that we fully understand the uplift rate derived. The final piece of the puzzle required to derive our uplift rate was its integration with a high quality sea level curve for the last 125 kyr. We chose Siddall et al.'s (2003) sea level curve because of its precision, relative proximity to the Mediterranean, yet its isolation from the variable tectonic signatures and isostatic problems associated with glacial loading of that region, and for its similarity with the Lisiecki & Raymo (2005) stacked curve.

The luminescence dating we undertook simply provides confirmation of the chronological framework for the events that we had deduced with reasonable confidence from other stratigraphic and geomorphic observations. We completely agree with Gallen & Wegmann that our IRSL dating, with its large uncertainties, cannot provide adequate resolution to separate the individual events presented here. Similarly, the soil descriptions provide a supporting understanding for our stratigraphic conclusions, independently confirming that the two fans are built over distinct time-periods.

In our revised version we explain more clearly these objectives and the evidence for our interpretation of the sequence of events in our 'landscape evolution' at Domata, so that there can be no misunderstanding.

Our detailed responses are presented below in blue.

(1) Lack of a sedimentologic and stratigraphic descriptions of the Domata fan in the context of other alluvial fans on Crete in the current version of the manuscript.

The Klados River gorge itself is not unique, but one of five similar gorges *[Actually, the Klados R. gorge is significantly shorter and its catchment size is much less than the other gorges of the Lefka Ori (c. 11 km^2 compared with 28 km^2 for the Sfakia fan).The size of the Domata fan sequence is also very small (0.1 km^2 vs 5.3 km^2 for Sfakia). Therefore, they are not necessarily similar.]* that drain the Lefka Ori (White Mountains) and there is little difference in the coastal geomorphology at the mouths of each of these gorges [*We disagree with this statement either. Please compare the geomorphology at the mouths of Tripiti, Klados, Samaria and Eligia gorges. They are very different. And the entrenched Aradaina Gorge at Marmara, with its marine benches is* *very different to the alluvial fan systems that extend from Hora Sfakion to Skaloti. There are significant differences in the coastal geomorphology at the mouth of each of these gorges. In addition, V.M., J.B. and D.M. who have walked down all five of them through the years, can confirm that these gorges also differ internally – that was also reflected in the significantly different degree of effort/difficulty to cross them].* However, having visited this fan sequence and studied numerous other fans on Crete, we can say that the Klados (Domata) fan sequence is sedimentologically and stratigraphically unique among fan deposits on Crete *[Please see the statement preceding this section. Our paper is not focussed on this issue. The authors of this short comment have made these observations and have the opportunity to write such a paper making this comparison.]* Most alluvial fans in Crete are coarse grained, clast supported, and weakly stratified. By contrast the Domata fan sequence is finer grained, having horizons that are variably clast and matrix supported, and better stratified than any other fan that we have seen on the island to date. It is these sedimentological details that will provide the most insight into the origins of the fan unit *[We believe that this is a value judgement. Our paper does not alter the opportunity for these reviewers to prove this point in their own paper].* The unique sedimentology and stratigraphy of this fan relative to other fans in Crete suggests that a different process is responsible for the deposition of this fan sequence *[Presumably to be included in their paper].* Given the short transport distance through the Klados River gorge (~ 5 km) *[transport distance is not necessarily a significant factor in the energy of a depositional environment, a high base level is]* and the relatively fine grained nature of the fan deposit, the Domata fan sequence is suggestive of a high-energy process *[Our interpretation differs – we believe the depositional environment is lower energy, influenced by high sea-level].* Stratigraphic and sedimentological descriptions of the fan sequence and discussion of how these observations compare with other studies of fans on Crete would substantially improve the manuscript *[As explained above, these descriptions and comparisons can still be made by these reviewers or by others; they are not the subject of this paper. See our objectives statement above. Further, it is inappropriate to suggest modification of our manuscript on the basis of their interpretation of their data, when we don't even have these data].* Based on the data presented current version of the manuscript it is difficult to evaluate whether or not the authors' argument that the fan sequence represents two fan *[We have now included a specific sentence stating unequivocally that the relict fan sequence at Domata represents two distinct phases of fan deposition within a single channel feeder fan. We have also added a sentence clarifying that both fans are sourced from the entrenched Klados River gorge, the feeder channel].* An alternative interpretation is that the Domata fan sequence represents a single depositional phase followed by unsteady incision, as is common in alluvial fill-cut terrace sequences (also known as complex-response fill terraces of Bull, 1990) *[The presence of a marine cliff at the shoreward side of the upper fan that also truncates an alluvial entrenchment of the upper fan surface indicates clearly that the depositional phase present during upper fan deposition is interrupted, and pre-dates deposition of the lower fan]*.

Importantly, our own observations indicate that the entire Domata fan sequence overlies a beach deposit that is the lateral continuation of the Holocene bioerosional notch (see figure below). If correct, such a stratigraphic relationship demands that the Domata fan sequence is Holocene, rather than Pleistocene, which is in direct challenge to the geochronology presented in this manuscript. [*We firmly believe that this interpretation is seriously flawed. The "bioerosional notch" in their figure (c), with its interpretation clearly points to the 6 m a.s.l. AD 365 earthquake bioerosional notch that we can follow westward all the way to Sougia and beyond to Palaiochora and Elafonisi and from there around the corner all the way to Falasarna. At the point indicated in their figure, the elevation of this notch coincides with the elevation of the contact between the lower fan deposits and bedrock. The bench indicated in the supplementary figure has been preferentially eroded because it relict from an earlier erosion surface (upon which the lower fan deposits sit here.) The same bioerosional notch is also present in bedrock immediately to the east of the indicated location, 1 m and more below the bedrock/lower fan deposits contact, the latter represented by an uneven morphology (see our Fig. 6b, newly annotated to illustrate this point and the onlapping relationship between the fan deposits and bedrock). The contention therefore, that beach deposits underlie the lower fan deposits on the basis of the reviewers' illustration, is stratigraphically unfounded. The lower fan deposits must pre-date the bioerosional notch. To interpret this bioerosional notch as pre-dating the lower fan deposits as an older Holocene feature requires the entire lower fan to post-date c. AD365. This bioerosional notch is cut in bedrock, has been eroded from fan gravels (note the rockfall deposits at the base of the cliff in the lower fan behind the main beach, illustrating that the loose* *gravels of the fan deposits erode very differently to the bedrock), but is further represented by the young, abandoned terraces at the Klados River mouth].*

Furthermore, we are curious as to why luminance dating was only attempted on a fan sequence that is almost entirely comprised of carbonate detritus? Why not also try to date the underlying beach deposit that contains more material suitable for luminance dating and is less likely to suffer from incomplete bleaching?

[The reason is clear: no "beach deposit" as interpreted by the commentators underlies the fan. Dating the "bioerosional notch" they refer to would provide an incorrect estimation for the age of the fans. The reviewers appear to be suggesting that entire >60 m thickness of these fans was deposited in <10kyr. Compare this thickness to those of other fans in Crete, such as those recorded at Sfakia region or even those recorded in central-eastern Crete by Gallen (2013). In addition, no beach deposit was identified on the marine bench cut in upper fan deposits, presumably because if deposited, it was subsequently eroded early in the deposition of the lower fan].

(2) The luminescence geochronology and the lack of tests for, or detailed discussion of the potential for and implications of incomplete bleaching.

The proper tests needed to confirm or reject whether or not incomplete bleaching has occurred were not reported. Without these key tests of samples from a depositional environment that is notorious for incomplete bleaching (Rhodes et al., 2010), it is difficult to interpret the luminescence data as being a trustworthy chronometer reflecting a true burial age. Every other study that has used luminescence dating to constrain the timing of alluvial fan deposition on Crete has successfully used quartz OSL (Pope et al., 2008; 2015; Gallen et al., 2014; Runnels et al., 2014). The fact that this method did not work for this study is of significance provided that the setting is geologically, tectonically and climatically similar to the locations of all of the aforementioned studies. The only thing that makes the Domata fan sequence unique in its sedimentology and stratigraphy, which suggests that a different process is responsible for its deposition (see comment above). Our suspicion is that the unique origin of the Domata fan sequence is why quartz OSL was unsuccessful. Furthermore, we question the reliability of the feldspar IRSL data without bleaching tests. It is acknowledged in the text that incomplete bleaching can explain the noisy IRSL data (P. 8, Lines 4-5), but the significance of this signal and the proper tests for incomplete bleaching are not present in the manuscript. Provided the unique problems with quartz OSL signals in this deposit, coupled with the known problems of incomplete bleaching in alluvial fans, and results that are difficult to explain, one is hard pressed to interpret this data at face value without prior proper vetting of the luminescence signals.

We summarise the reviewer's concerns in three main groups:
1) Quartz seemed not to have worked at Domata but has worked in other studies nearby on Crete (Pope et al., 2008, 2016; Gallen et al., 2014)
2) Feldspar dating at Domata yields imprecise IRSL ages with high uncertainties
3) Feldspar IRSL dating was not carried out following standard procedures (no testdose correction during SAR measurement, no fading test to correct for potential age underestimation).

1: Why was quartz OSL so dim? For readers of the above mentioned studies it's hard to believe that quartz didn't work at Domata, a few kilometres away from the Sfakia fan system with comparable geological settings. But it is an empirical fact, that the OSL signal intensities were very low and that linear modulation (LM) measurements showed that there was almost no "fast component" in the natural OSL signals. Thus, quartz dating is wrong. A possible explanation is that the dated quartz was not sensitized before sedimentation. Quartz needs repeated cycles of daylight bleaching and radioactive irradiation to emit any analysable OSL signal. Dim quartz not suited for OSL dating is often reported from geologically young environments (e. g. the Alps). But, of course, this does not explain why the behaviour of Domata quartz is different from quartz of other locations on Crete.

2: Why are our error in age so large? Our samples consisted almost entirely of carbonate detritus with only few quartz and feldspar grains of a suitable size (exception: UF-2) and the samples were collected mostly near the surface. This implies some problems leading to high age uncertainties: 1) In such inhomogenous and coarse grained samples one can expect radioactive inhomogeneities. 2) Near the surface, any kind of postdepositional mixing is possible (roots, bioturbation, penetration of bleached material into voids). 3) The *dominance of carbonate causes an extremely low dose rate of roughly 0.5 Gy/ka for quartz and 1.0 Gy/ka for potassium feldspar. Hence, the contribution of the cosmic dose rate to the total dose rate is relatively high (ca. 40% for quartz, 20% for potassium feldspar). But the cosmic dose rate is only estimated from the sample position and cannot be measured. 4) Incomplete bleaching (insufficient daylight exposure) is always a risk in fluvial environments. The coarse grain sizes suggest runoff events of high energy, where incomplete bleaching is more likely. The studies of Pope et al. (2008, 2016) suffer less from the mentioned problems because they sampled homogenous sandy material from sand layers/sand lenses within the fan body.*

3: Problems appeared with potassium feldspar IRSL dating? In addition to the problems discussed above (large errors), IRSL dating failed using standard SAR procedures. For instance, it was not possible to recover a known laboratory dose with standard SAR protocols. It was deduced that the testdose correction did not work, but rather caused a systematic underestimation of known laboratory doses. Thus, testdose correction was omitted, but all aliquots affected by sensitivity changes were rejected. Another constraint is the so-called anomalous fading, which affects almost all potassium feldspar samples. Anomalous fading is the unwanted signal loss during burial leading to age underestimation. Often, age underestimation up to 30% of the true age is observed. It is possible to determine fading rates to correct the resulting IRSL ages, but this is very time consuming and there is currently discussion questioning whether this technique is reliable. Nevertheless, such fading tests reveal whether anomalous fading plays an important role or not. Fading correction was not carried out here, because fading is the "antagonist" of incomplete bleaching. Fading causes age underestimation, incomplete bleaching causes age overestimation. It is probable, that our feldspar samples suffer both from fading and incomplete bleaching and it's hard or even impossible to distinguish the two effects.

Bleaching tests were not conducted. This is to test, whether the IRSL signal is bleachable in an appropriate time. For this test, aliquots are bleached stepwise in a solar simulator and the remaining signals are measured. This could be easily done. Tests for incomplete bleaching (possible age overestimation) are important and we did it by analysing the OSL and IRSL data (e.g. analysis of age distributions or plotting the signal intensities vs. dose). And yes, ***there is evidence for incomplete bleaching****. Unfortunately, the amount of unbleached signal inherited cannot be quantified clearly. For quartz OSL there are statistical models to select the best bleached grain population when measuring single grains or “small aliquots” containing only few grains. These models are less reliable for feldspar.*

(3) Incomplete review of pertinent literature.

Much of the literature on 1) Cretan alluvial fans and 2) alternative models for the tectonics of the Hellenic forearc are missing from the manuscript. *[We agree that there is some missing literature, mainly because this work was compiled before Pope et al. (2016) was published. We will include this work in the revised version. Also, we will include discussion on alternative tectonic interpretations, although whether or not there are uplift transients on Crete will not impact at all on the results of this study.]* In addition to the excellent work of Pope et al. (2008), Pope et al. (2016), Runnels et al. (2014), and Gallen et al. (2014) employ luminescence geochronology to date alluvial fans on Crete. Pope et al. (2016) and Runnels et al. (2014) are absent from the current version of the manuscript *[For Pope et al., (2016) the reason is explained above. For Runnels et al. (2014): this publication was published in the journal of European prehistory, and it escaped our attention. We thank the reviewers for pointing this article out]*. While Gallen et al. (2014) is cited, no acknowledgement is made for this studies contributions to understanding the Quaternary coastal stratigraphy of Crete *[We have amended the ms to include Gallen et al.’s studies, indicating that they have recognised elevated benches in central-eastern Crete dated as last Interglacial in age. This supports our interpretations on relative age of the Domata fan sequence. Our objective in this ms was to provide a post-Last Interglacial temporal context for the Domata fans]*. In addition to successfully dating alluvial fans with quartz OSL, the Gallen et al. (2014) study dates marine terrace deposits with OSL that are buried by alluvial fans. The authors of the above cited studies, and especially Gallen et al. (2014) use detailed mapping, stratigraphy and sedimentology of the deposits, pedology, OSL geochronology and a global sea level curve to derive a model for the coastal stratigraphy in southern Crete that relates interactions between tectonics, climate and eustacy. Discussion of the findings and interpretations presented by Mouslopoulou et al. in the context of other, similar studies from Crete would greatly improve the manuscript *[See our objectives statement above]*.

The review of the Quaternary tectonics of Crete is incomplete. In the background section and again in the discussion, alternative models for the Quaternary vertical tectonics of the island are not discussed *[See above]*. Section 2 reads as though consensus has been reached regarding “Late Quaternary uplift transients”. However, there is an ongoing scientific debate in the literature about whether or not these Late Quaternary uplift transients actually exist or if there are problems with the geochronology used to derive this model *[The geochronology used in our ms is consistent with the Gallen et al. (2014) OSL geochronology for the Last Interglacial. Furthermore, it contradicts the suggestion that the entire Domata fan sequence is Holocene in age]*. While this may be the favored interpretation of the authors of this manuscript, other interpretations should be acknowledged and the ongoing controversy in the literature noted *[We will make this change in the revised version – but it is unrelated to our conclusions]*.

To answer to some of the reviewer’s concerns, we will add the following text in our revised manuscript:
This work was undertaken prior to the publication of the latest results of OSL and U series dating of the Sfakia fan sequence (Pope et al., 2016). Intriguing conclusions of their high resolution dating work include that at Sfakia, three sometimes overlapping phases of fan deposition since the last interglacial are separated by two phases of fan entrenchment, the first close to the MIS 5/4 (c. 70 kyr) boundary, the other close to the MIS 2/1 boundary (c. 14 kyr), triggered by major climatic change. Fan deposition there has to a large degree persisted through stadial and interstadial periods during the last 125 kyr. Periods of entrenchment at Sfakia do not appear to correlate with the two entrenchment periods at Domata. The Sfakia fan is significantly different from the Domata fan in catchment size (c. 28 km^2 compared with c. 11 km^2), fan size (5.3 km^2 compared with 0.1 km^2), the presence of more than one feeder channel at Sfakia, and in the nature of deposits (primarily clast-supported gravels compared with primarily matrix-supported gravels). Whether these differences are responsible for differences in depositional and entrenchment histories and in preservation of marine cliffs at Sfakia, or differences in local climatic regimes or vegetation changes is uncertain. However, one compatible conclusion of their work with our own is recognition of the importance of base level (sea level) change to the process of entrenchment.

Line-by-line comments:

Introduction:

P. 2, Line 14-16: There is little mention of sediment supply here. The interplay between sediment supply and discharge is an important factor controlling alluvial fan deposition and may have little to do with changes in base level (e.g. rising sea-level). Furthermore, enhanced rainfall does not necessarily translate into alluvial fan deposition, as implied. Enhanced rainfall may favor increased discharge at the expense of reduced hillslope sediment supply because the hillslopes are vegetated more during times of increased annual precipitation and thus, the alluvial fan experiences an episode of incision. The interplay between climate and tectonics, deposition and incision is not straightforward. We would also like to point the authors to alternative models for channel aggradation that might be relevant to this study. In particular, the recent work of Scherler et al. (2016) documents that Late Pleistocene fill terraces in southern California (a region climatically similar to Crete), which were traditionally interpreted as the result of climate change, are more likely the result of changes in sediment supply due to a large landslide in the catchment. This research is also relevant because they use luminance dating of the alluvial fill and discuss at length the geochronologic problems associated with incomplete bleaching.
[We have modified these sentences on the revised version to accommodate some of the reviewer’s concerns.]

P. 2, Line 20-24: These types of interpretations are difficult to discern from field data alone as the drivers of aggradation and incision reflect the interplay between sediment supply and discharge. What seems to be implied by this review is that deposition is driven solely by enhanced precipitation and incision by tectonic uplift. Yes, ultimately, the accommodation space needed for alluvial fan deposition is a result of tectonic processes, but at the time scale of the Late Pleistocene, the amount of tectonic uplift is insignificant in comparison to variations in climate-driven discharge and hillslope sediment supply from the mountainous catchment to the alluvial fan system. Furthermore, precipitation, temperature, and thus, vegetation co-vary in ways that make it difficult to predict how changes in precipitation relate to variations in catchment sediment supply and discharge. Depending on the climate and vegetative response, increased precipitation can lead to a reduction in sediment supply and incision, rather than aggradation.

The authors also appear unaware of a critical new body of research by Pope et al. (2016). *[See above explanation].* In this paper, Pope and colleagues 30 present 32 new OSL and U-series dates for what is undoubtedly the best dated alluvial fan sequence on the south coast of Crete, the Sfakia fan. Importantly, Pope et al. (2016) conclude that over the entirety of the late Quaternary, the Sfakia fan only experienced two episodes of entrenchment (incision), during the transition between Marine Isotope Stages (MIS) 5a/4 and MIS 2/1. They propose that the MIS 5a/4 period of fan incision was driven by sea level-induced base level fall; whereas the MIS 2/1 interval of incision (during a time of rapid eustatic sea level rise) was the result of reduced hillslope sediment supply to the fan resulting from landscape stabilization (re-vegetation) during the onset of the current interglacial (Holocene). If their data is correct – and they have lots of reliable geochronology to support their conclusions – the most 5 recent episode of fan incision, for example had little if anything to do with base level fall or tectonic uplift. Pope and colleagues conclude that, with the exception of the above mentioned intervals of fan entrenchment (incision), fan aggradation occurred across the entire last interglacial/glacial cycle in all climatic settings (i.e. interglacials, interstadials, and stadials). The Domata fan is at the same latitude and only 25 km west of the Sfakia fan studied by Pope and colleagues (2016). It would be surprising if two nearby fan sequences on the south coast of Crete had markedly different aggradation-incision histories if the driving processes were climate change and/or eustatic variations, as both of these factors should almost certainly be nearly identical for the two sites. If there are real differences in the timing of fan aggradation and incision episodes between Domata and Sfakia they likely are the result of internal stochastic variations in catchment hillslope sediment supply to the channels feeding the alluvial fans.
[We have modified these sentences on the revised version to accommodate the reviewer's concerns]

P. 2, Line 26-29: It is an inference, based solely upon a morphogenetic interpretation of the topography of the Domata fan that the sequence represents two episodes of fan building as no stratigraphic evidence is provided. An alternative interpretation is that the Domata fan sequence represents a single depositional phase followed by unsteady incision, as is common in alluvial fill-cut terrace sequences (also known as complex-response fill terraces of Bull, 1990). For the former interpretation to be convincing, stratigraphic data delineating two distinct fan depositional units needs to be provided and would substantially improve the manuscript.
[We believe that the stratigraphic evidence inherent in the presence of a marine cliff that truncates both the upper fan surface and underlying deposits, and the alluvial entrenchment wall of the Klados River suffice as convincing stratigraphic data.]

Geological setting of Crete and Vertical tectonics:
P. 3, Lines 18-24: We think that it is important to qualify these statements. The way that it is written herein is that there is scientific consensus on this topic, which is not the case. The debate is ongoing about uplift transients in the Hellenic forearc and it is important to acknowledge that this only presents one side of the argument. Many researchers favor a slow, mostly steady (at least at time scales greater than several earthquake events) Quaternary history of uplift for the island.
[We will modify this sentence on the revised version but actually, the only two studies that have derived a series of uplift rates through time on western Crete are Tiberti et al. (2014) and Mouslopoulou et al.(2015) show transient uplift. But in reality, whether there is transient uplift or steady-state uplift doesn't change our conclusions regarding Domata].

P. 3-4, Lines 32, 1-2: The Holocene notch is buried by the fan at Domata (see supporting data). Basic stratigraphic principles demand that if an extensive coastal geomorphic (geodetic) marker is locally buried by a sedimentary deposit, the deposit must by younger than the geomorphic marker, in this case the Holocene notch. This single field geomorphic observation places the geochronologic results and subsequent conclusions of this manuscript into doubt.
[In our view, the reviewers' interpretation is incorrect. The notch is cut in the surface of the bedrock, but eroded from the alluvial fan gravel surface due to its susceptibility to erosion. The notch is not preserved behind the beach in the gravel cliffs at Domata for this reason. However, its presence is represented by the low, relict alluvial terraces at the mouth of the Klados River. Between the reviewers' illustration and the Klados River mouth, the bioerosion notch (which we correlate with the c. 365AD paleoshoreline) underlies the base of the fan gravels and is preserved as a notch in bedrock. It is therefore not possible that this feature underlies the alluvial fan sequence].

Data – methods – Chronology:
P. 4, Lines 4-5: This is an interpretation that requires supporting data. *[The supporting data are presented in the following paragraphs, as we state in the following sentence of the manuscript].* The morphology of the fan

might equally well be represented by a single filling episode followed by unsteady incision into the fan deposit *[see above]*.

Coastal geomorphic features at Domata:

P. 4, lines 20-21: Where does the quartz and feldspar in the fan come from if the bedrock in the Klados River catchment is mostly carbonate? *[This is now incorporated in the text through better description of the bedrock units]*.

P. 5, Lines 16-17: This is a key observation, but from figures 2-3 there is no evidence that the "lower fan" onlaps the "upper fan". The lower fan surface could simply be a fill-cut terrace into the maximum aggradational surface of the "upper fan". Please provide stratigraphic observations to support this interpretation. *[Upper fan deposits are cut by alluvial entrenchment and subsequent marine cliffing. Alluvial deposits of the lower fan lap against the cut alluvial entrenchment cliff and the lower fan surface is seamless between the alluvial cliff and the marine cliff. This stratigraphic relationship is clear and there is no other explanation needed, or indeed possible.]*

P. 5, Line 23-24: It is difficult to see these details in Figure 6a. Is it possible to add some close-up photos of what the deposit looks like in detail with examples of the features provided? It would help readers' understanding of the stratigraphy if they could "see" what the fan deposit looks like. All of the overview photographs are great, but readers will be left wondering what the deposits looks like up close.
Also, what lithology makes up the fan deposits? We assume that it is carbonate, but no details are provided. If the deposit is mostly carbonate, where is the quartz and feldspar used for OSL coming from? *[See above]*

P. 5, Lines 23-25: Details on the stratigraphy for the "lower-fan" are great! How does the "upper-fan" stratigraphy differ? In other words, how does one distinguish between the lower and upper fan units as illustrated in figure 6b? These details are essential to the interpretation of two distinct fan units. Perhaps a composite stratigraphic column of the fan sequence would help. *[We have added some more descriptions in the revised version and we have replaced old Figure 5 with a new Figure 5 (attached below) that illustrates clearer the stratigraphic features we discuss as well as the location of the luminescence samples with respect to these features. The two deposits are clearly distinguishable using surface geomorphology. Figure 6b is at the west end of the Domata fan exposures and far removed from the luminescence age sampling sites. The geomorphic and stratigraphic context of these sites are now illustrated schematically in the revised Fig. 5. Lithological characterisation of the two fans units is not a key part of our study]*.

P. 5, Lines 25-31: This is a key observation, but the level of detail in Figure 6 is insufficient for the reader to be able to see this relationship. *[See comment above]*.

P. 5, Lines 33-34 & Page 6, Lines 1-2: From the way that this section of the text is written, it is unclear if the paleoshoreline (marine bench) is cut into, or buried by the fan. Our interpretation of Figure 6 is that it appear as though the paleoshoreline is buried by the fan. Our field observations from this area suggest that this Holocene shoreline is buried by the fan (see supporting figure).
[We firmly disagree with this interpretation. This is a surficial bioerosional notch cut into the fan (now removed through erosion) and the bedrock (where applicable). This notch can be nicely followed westward along the bedrock all the way to Sougia and relates to the 365 AD earthquake that uplifted western Crete up to 10m (Pirazzoli et al., 1982, 1996; Shaw et al., 2008; Stiros, 2010; Mouslopoulou et al., 2015)].

P. 6, Line 10: Wegmann, (2008) and Gallen et al. (2014) also studied Pleistocene terraces on Crete and interpreted them in the context of stratigraphic relationships with interfingered alluvial fan deposits. Furthermore, Gallen et al. (2014) and Runnels et al. (2014) dated several alluvial fans in southern Crete with OSL, in addition to dating marine terraces with the same technique. *[We now include reference to Gallen et al. (2014) in this statement]*.

P. 6, Line 10-13: Based on stratigraphic relationships, pedology and OSL geochronology, Gallen et al., 2014 suggest a stratigraphic model for the genesis of marine terraces and alluvial fans based on tectonic, climatic and eustatic considerations in which marine abrasion platforms are cut and marine terrace deposits emplaced during eustatic transgressive-to-highstand phases , whereas Pleistocene alluvial fans are deposited during cooler (and drier) periods associated with relative sea-level low stands when sediment supply presumably is elevated relative to discharge. In addition to the geochronologic constraints on alluvial fan age, the other observation that implies deposition during cool periods is that the surface gradient of coastal alluvial fans on the south coast of are steep and prograde to a base level far lower than modern day sea level. This observation suggests that the Pleistocene fans are deposited when relative sea level is lower than the present day. This stratigraphic model is relevant because, if preservation potential were not a problem, a fan at lower elevation might be older than a marine terrace found at a high elevation relative to modern sea level. *[We agree that MIS3 and MIS2 represent relative sea level low stands compared to the present day. We make no assumptions of sediment supply or river carrying capacity. We base our ms on clear stratigraphic and geomorphic data that require explanation by a certain sequence of events. We recognise that the Domata fan deposits are MIS3 in age, and the older fan is benched by marine trimming. Given the well-established sea level curve, our objective is to calculate an uplift rate from this observation. We propose no model, but are familiar with those of Gallen et al. (2014) and Pope et al. (2016). To propose a model or even support one existing model on the basis of our work at Domata would be presumptuous.]*

OSL dating of alluvial fans:

P. 6, Line 13: Perhaps consider changing the heading of this section and all subsections. OSL stands for optically stimulated luminescence and IRSL stands for infra-red stimulated luminescence. They are different techniques and should be treated as such in the section headings. Similarly, figure 7 shows IRSL results, rather than OSL results as is indicated by the caption and the labels on the x-axis of the figures. Perhaps use "Luminescence dating of alluvial fans"?
[We agree it may be better to differentiate OSL from IRSL and now use the general term "luminescence dating"].

P. 6, Line 14-21: The sampling strategy is well thought out; however, why are there no samples from the base of fan unit 2? *[As stated previously, the reason is simply because the deposits below the lower fan are not part of the fan.]* Also what did the sampled horizons look like? The only information on this is for UF-2. Our field observations of this deposit suggest that it is composed primarily of carbonate sediment. Were there individual fine sand-to-silt lenses that were sampled, or simply stratigraphic horizons that were soft enough to hammer a tube into? *[We have added some more descriptions in the revised version and we have replaced old Figure 5 with a new Figure that illustrates clearer the stratigraphic features we discuss as well as the location of the luminescence samples with respect to this features].*

OSL results:
P. 7, Line 19-20: Gallen et al. (2014) and Runnels et al. (2014) also dated alluvial fans in southern Crete with Quartz OSL. There is also a new paper by Pope et al. (2016) that has an abundance of OSL data on the Sfakia fan sequence (see comment above). *[See comments above and elsewhere].*
P. 7, Line 19-27: This might be better reserved for the discussion, but what makes Domata unique in that quartz OSL of the fans there does not work? Quartz OSL has worked fine for multiple other studies where geologic conditions are similar 20 (Pope et al., 2008, 2016; Gallen et al., 2014; Runnels et al., 2014). *[See general comments on luminescence dating in our first set of answers].*

P. 7, Line 29: What about the ages makes them reliable? *[Reliable in terms of the sampling and measurement methodology regardless of problems like incomplete bleaching].*

P. 8, Line 4-5: In alluvial fans, incomplete bleaching is a problem (e.g., Rhodes, 2010). It appears that the Domata fan samples suffer from incomplete bleaching. What tests, if any, were preformed to rule-out incomplete bleaching? For quartz OSL results that deviate from every other published study that has been performed on alluvial fans on Crete it is worth 25 investigating why the signals are so different. Provided the short transport distance of the Klados River gorge (~ 5 km) and the high-energy nature of the Domata fan sequence, incomplete bleaching is potentially a major concern.
Taken at face value, the IRSL ages reported in Table 1 imply that the lower (supposedly younger) fan unit was emplaced before the end of deposition of the Upper (older) Fan unit. This is difficult to reconcile with the stratigraphic arguments advanced in this manuscript. Furthermore, the observation that the fan buries a presumably Holocene age paleoshoreline is problematic (see supplemental figure). Some experiments to test for incomplete bleaching or at least a detailed explanation of why incomplete bleaching isn't an issue should be added. *[See general comments on luminescence dating in our first set of answers].*

Soil development:
P. 8, Line 28-29: Were these colors derived from a Munsell color chart? *[A Munsell chart was not used. The described colour is the obvious macroscopically difference in colour which is presented also clearly in figure 8b*

and 8c. However, the difference in colour was obvious and we consider it as in dry conditions (sampling performed in summer, after prolonged period without rainfall)]. If so, the hue, value and chroma values should be provided as they represent a semi-quantitative measure of color and several combinations of hue, value and chroma have the 5 same color name. It would be useful to point out where the weak B horizon is on the Upper fan soil in figure 8C and the soil texture evidence used to support this interpretation. *[The B horizon in the upper fan is already indicated with a dashed line in Figure 8c. It is not clear to us why the reviewer made this comment.]* Having worked on soil profiles on Crete, and based on the photo presented in Fig. 8C, it looks like this profile could be characterized as a thin A horizon over a C horizon. *[As it is stated in our study, a macroscopic approach in the field was used in order to describe the soils, thus the possible B horizon in the upper fan was identified by the technique of knife penetration in the excavated profile. The lower knife penetration (less friable), the change in the texture (accumulation of finer material clay+calcite) and the absence of organic material (aggregates due to organic accumulation are obviously less also in figure 8c) lead us to the conclusion of possible B horizon in the upper fan. However, ongoing work in the soil horizons in the special case of Domata (as it appears from the spectacular alluvial fan development) will reveal further evidence on what we describe as a "(weak) B horizon". In addition, the authors believe that whether the horizon is B or A/B (A/B: in the initiation stage of B horizon), doesn't impact on our basic argument that the upper fan soil is better developed compared to the lower fan soil].*

P. 8-9, Line 31-34, 1-4: Gallen (2013) provides detailed descriptions of alluvial fan soil profiles with OSL geochronology in Chapter 2. Gallen et al., 2014 and Runnels et al., 2014 describe the pedology of alluvial fans of Crete in conjunction with OSL geochronology. These studies should be discussed in the context of this study, as soils formed on alluvial fans of reportedly the same age are distinctly different. Furthermore, aside from color, observations supporting the notion that the soil profiles shown in figure 8B and C are consistent with the descriptions of stage 2C soils from Pope et al., 2008, particularly soil textures consistent with an increase in clay content (e.g. Bt horizon), are not provided. Based on observations presented in Gallen (2013) and Gallen et al. (2014), the soils on these fan surfaces are less mature than fans dated to ca. 40 ka that have evidence of pedogenic alteration to > 1.5 m below the present-day surface (see figure 4 of Gallen et al., 2014 and Appendix of Chapter 2 in Gallen, 2013).

[The reviewers imply that the soil in our study area is less mature compared to the thicker and better developed soils of the same (or older) ages presented by Gallen (2013) and Gallen et al. (2014). However, we believe that several parameters can dramatically change the soil cover thickness in areas of similar ages, especially when these regions span several 10's of kilometres. Rainfall on the south flanks of Lefkai Ori varies radically from the crest of the 2450 m of mountain range to the south coast (i.e. Domata). The elevation change in the 6.5 km north from the coast is 2100 m, (a gradient of 32%) while in Tsoutsoros area where the Gallen et al. 2014 studied the 'marine terrace soil profiles, has a relief of 0 to 480 m within 3.44 km (13% gradient). There is clearly a difference in orographic rainfall. Overcoming the local rainfall variability, another parameter which is decisive in the soil development is the texture and lithology of the parent rock. Gallen et al. 2014 investigated terraces developed in both Neogene formations and in Mesozoic limestones-sandstones-mudstones of Pindos nappe which are absent from our site. Thus, the results of the soil profiles which are presented by Pope et al. 2008 are the best available "benchmark" where we could interpolate our findings. As already mentioned, the soil depth and the physical characteristics presented by Pope et al. (2008) showed several similarities with those profiles presented in our work. Finally, chronosequence studies on soils are those which are explicitly giving answers in the above questions, however, constrains and limitations of a chronosequence study make them rare especially on the island of Crete where landscape is dictated by intense tectonic evolution. Lin (2011) nicely summarised soil development as an archive of the cumulative effects of tiny repeatedly cyclic changes where even a small difference in soil parameters can create a remarkable difference in pedogenesis].

P. 9, Line 11-13: This isn't supported by the geochronology (keep in mind the difference between OSL and IRSL). The data suggests that the alluvial fan units are synchronous. The Upper fan unit is bracketed between ~54 and 23 ka and the lower fan unit ceased deposition ~40-28 ka.

[The authors thoroughly understand the meaning of the data. The IRSL data (with inherent errors) presented in our article support the chronology demanded by the stratigraphy that we have established. The soil development independently supports our stratigraphic interpretation. We conclude that the stratigraphy, luminescence dating and soil analysis independently point to the same interpretation: that the upper fan is older than the lower fan and that they are last glacial (MIS4 to MIS2) in deposition].

P. 9, Line 13-15: Aside from the problem that the geochronology suggests that the Upper fan surface was abandoned ~5-15 ka *after* the lower fan surface *[this is wrong and we don't understand why the reviewers have included this comment; they appear to be taking the geochronology out of the stratigraphic context and we regard this as inappropriate, particularly given the error margins on IRSL ages]*, is the Liar et al. (2009) reference relevant in this context? Liar et al. (2009) work on Holocene soils. This study, according to the authors is about Pleistocene soils. The effects of a 5 ka difference in geomorphic surface age for surfaces that are presumably an order of magnitude older than that would need to be shown. One would expect that soils forming on different aged surfaces would become more similar over time. For example, a 5 ka time gap for the initiation of soil formation might be negligible after a 30-40 ka shared history of soil development.
[We acknowledge that the study of Liar et al. (2009) presents Holocene soils and that the soils at Domata are upper Pleistocene. So in the revised version we complete our argument by presenting the study from Huang et al (2016), where they found a clear difference in amorphous and crystalline Fe oxides concentration (hematite) in the soils from 135 ka and 125 ka. In our study, soil age differences of between 4 ka (minimum) and 14 ka (maximum) could easily have left a significant weathering imprint if this time span was characterized by more intense rainfall resulting in faster soil development than today (e.g. see our evolution model, Figure 10). We disagree with their argument that over greater time periods, soils always progressively assume a similar appearance. Soil development is related to climatic (and other) parameters. Our interpretation (Figure 9) is that the upper fan was deposited within the MIS3 and the soil development started after surface abandonment at 40-45 ka; the time that followed was climatically warm and wet in Greece (intense weathering; Tzedakis et al., 2004). The lower fan was deposited from 36 to 29 ka, the start of a dry and cold period. Thus, the soil beneath the lower fan surface developed under cold and dry conditions, while the upper fan soils developed under warm and wet conditions. It is difficult to conceive that soil development has converged during MIS2 and MIS1, "erasing" the past pedogenic processes].

Landscape evolution at Domata:
P. 9, Line 29-30: Why would falling sea-level promote deposition? *[This has been corrected in the revised version].*

P. 10, Line 10-11: Why does relatively high sea-level now promote deposition? What are "deteriorating climate conditions", what evidence is there to support them and why do they promote fan deposition? *[Fan deposition is promoted by a high base level, in this case, sea level of the time. When climatic conditions deteriorated, resulting in a falling sea level, the balance between sediment supply, gradient, rainfall and catchment vegetation re-adjusts, but with on-going sea level fall, entrenchment is the likely outcome (e.g., see Pope et al. 2016)].*

P. 10, line 27-29: But isn't the fan burying this Holocene deposit (see supplemental figure)? *[We firmly believe that the reviewers' interpretation is wrong].*

P. 9-11, Line 17 – 32, 1 – 34, 1-3: Provided that the geochronology is correct, any number of interpretations could be argued to be equally valid within the uncertainty of the data. For a given date the authors utilize either the mean or the median as preferred to provide an older or younger age estimate, respectively. Furthermore, taken at face value the data indicate that the upper fan surface was active until 25 ka. *[We don't believe this is true. The dates are single samples, and they have errors that are presented and documented. Deposition could be anywhere within the error limits. Stratigraphy determines the sequence of events, but dated events provide some chronological information (within error)].* It isn't until lines 1-3 on page 11 that the reader is told that this sandy horizon is a fine-grained cap on the entire deposit. The position of the luminescence samples in a composite stratigraphy of the fan sequence is needed in order to understand the context of the data. *[We have replaced old Figure 5 with a new Figure that illustrates clearer the stratigraphic features we discuss as well as the location of the OSL samples with respect to this features].*

Another observation which should be addressed in the context of the stratigraphic model is how an unvegetated near-vertical cliff of unconsolidated gravel can remain between the Upper and Lower fan units for ~ 39 ka? Furthermore, why is the morphology of the cliffs on the Upper and Lower fan units so similar despite an inferred 35 kyr age difference (see figures 5 and 10 in manuscript)? *[We don't understand how the reviewers get a 35 kyr age difference. The time period between abandonment of the upper fan surface (c.45 kyr) and abandonment of the lower fan surface (36 kyr) is a 9 kyr age difference].* When fault scarps are formed in unconsolidated alluvial fan sediments in places like the Basin and 10 Range of the southwestern United States, they may initially be vertical geomorphic features, but through hillslope erosional processes, the morphology of the scarp changes through time (e.g. McCalpin, 1996). Diffusion of scarps through time has proven to be a useful relative dating tool in studies of both fault scarps (e.g. Nash, 1980) and paleo-shoreline scarps (e.g. Andrews and Bucknam, 1987). Perhaps this could be attempted in this study.

Why aren't similar fan deposits observed in the five gorges that drain southward off the Lefka Ori (White Mountains)? The Klados River gorge is the only one that preserves such spectacular fans. Despite the contention that Domata beach is geomorphically unique along the southwest coast of Crete, it is not. *[We don't agree with this statement. Catchment sizes vary, river lengths vary, gradients vary, vegetation varies, rainfall varies, and none of the others have the bedrock ridge that forms a gorge close to present day sea level (see Fig. 1 in our manuscript)].* All the major gorge outlets to the ocean along the Lefka Ori are morphologically similar, yet none host similar fan deposits. Each gorge should preserve similar features if, as it is implied the forcing that generated the fans is a coupled climate-tectonic-eustasy signal that should affect the island regionally. *[Again, we disagree. Each gorge is different as indicated above and in lots of other ways, too. Regional signals are always complicated by local factors].*

The importance of tectonic uplift at Domata:

P. 11 Line 17-18: Where is the marine terrace that cut this cliff? Based on the stratigraphic model, one world expect marine deposits between the two fans. *[While it would be useful if this was the case, it is not necessarily true. Cliff collapse, as well as the 365AD uplift, keep the sea at bay from the foot of the Holocene sea cliff. In an erosional setting, marine deposition is not necessarily likely, and in addition, early phase fluvial erosion associated with lower fan deposition may have removed such marine deposits, if they were ever deposited].* An uncertainty analysis on the elevation of the inner shoreline and some evidence that this cliff corresponds with a marine abrasion platform would be beneficial for readers. Furthermore, if it is assumed that the base of the fan lies on a marine abrasion platform (e.g. marine terrace), the difference in age between the marine platform and the overlying alluvial fan can be substantial (see Gallen et al., 2014 for examples from southern Crete). *[This appears to contradict the reviewers' suggestion that the fans were both deposited within the Holocene. Given the nature of the Siddall et al. (2003) MIS3 and MIS2 sea-level curve and other high resolution sea-level curves that have been published to date, where sea-level declines from 55 kyr to the last glacial maximum, the time period between platform cutting and renewed alluvial deposition cannot be long. Any tectonic uplift that occurred during this period would merely exacerbate a tendency for rapid entrenchment].*

P. 11 Line 11-32: There is reason to suspect that Pleistocene radiocarbon ages might suffer from alteration of primary material, shifting radiocarbon dead ages to ones that are younger. This is nicely discussed by Wegmann, 2008 and can be noted in δO_{18} and δC_{13} shifted to more negative (terrestrial) values, relative to marine standards, in Triberti et al., 2014. *[The ages presented in Mouslopoulou et al. (2015) are only of secondary importance in this manuscript. Nevertheless, the results of the current study, using completely different methods, provide an independent test on the results of the Mouslopoulou et al. (2015) study. In both cases uplift rates, averaged over the last ~40 kyr, are similar to one another and are also similar to those derived by dating other marine terraces in western Crete (Shaw et al., 2008; Strasser et al., 2011; Tiberti et al., 2014].*

P. 12 Lines 1 – 15: Again, this is only the opinion of a few and does not represent the view of many other researchers that study the tectonic geomorphology of Crete. It would be useful to include a more thorough discussion of all the relevant literature.

P. 12, Line 17-20: No evidence is provided for the climatic link. The geochronology is simply not precise enough to permit such interpretations. *[Sea-level fluctuations are controlled by (and reflect) climate fluctuations].*

Conclusions:

P. 12, Line 25-26: This is an inference based on uncertain geochronology. Perhaps it is better to say "One interpretation of the data" rather than "Data analysis shows". *[In fact, we use the geochronology presented here as independent support for the hard geomorphic and stratigraphic data demanded to develop this sequence of events].*

P. 12, Line 28-29: It's not entirely clear to us how this interpretation is supported by the data.

References cited in review that are not cited in the manuscript:

The references below that are indicated in red are ones we find relevant to our study and will be included in the revised version.

Andrews, D. J., and Bucknam, R. C., 1987, Fitting degradation of shoreline scarps by a nonlinear diffusion model: Journal of Geophysical Research: Solid Earth, v. 92, no. B12, p. 12857-12867.

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McCalpin, J., 1996, Paleoseismology: San Diego, Academic Press, p. 583.

Nash, D. B., 1980, Morphologic Dating of Degraded Normal Fault Scarps: The Journal of Geology, v. 88, no. 3, p. 353-360. 15

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

Rhodes, E.J., 2011. Optically Stimulated Luminescence Dating of Sediments over the Past 200,000 Years, *Ann. Rev. Earth planet Sci.*, **39,** 461-488, doi:10.1146/annurev-earth-040610-133425.

Runnels, C., DiGregorio, C., Wegmann, K.W., Gallen, S.F., Strasser, T.F., and Panagopoulou, E., 2014, Lower Palaeolithic 20 artifacts from Plakias, Crete: Implications for Hominin Dispersals: Eurasian Prehistory, v. 11, p. 129-152.

Scherler, D., Lamb, M. P., Rhodes, E. J., and Avouac, J.-P., 2016, Climate-change versus landslide origin of fill terraces in a rapidly eroding bedrock landscape: San Gabriel River, California: Geological Society of America Bulletin.

Wegmann, K.W., 2008. Tectonic geomorphology above Mediterranean subduction zones; northern Apennines of Italy and Crete, Greece, Ph.D. Dissertation, Lehigh University, Bethlehem, PA, 169 pp. 25.

We have also used the following additional references in response to the reviewers' comments:

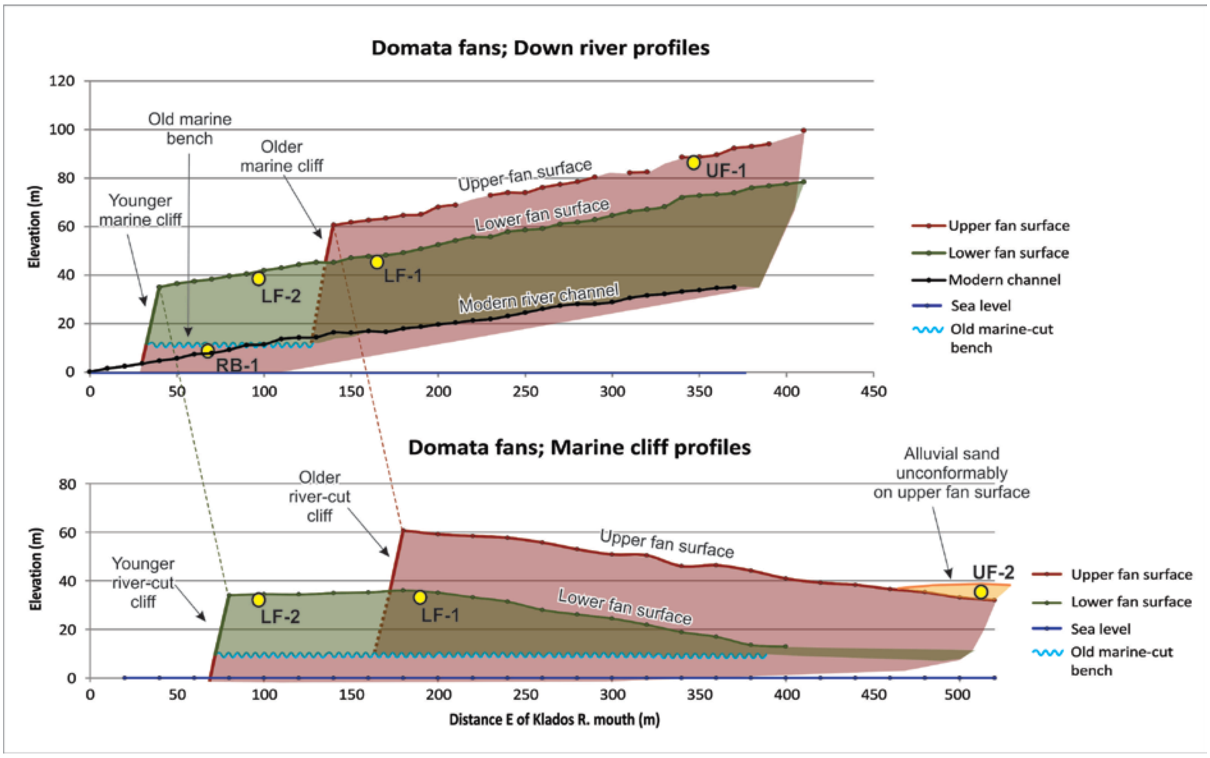
Tzedakis, P.C., Frogley, M.R., Lawson, I.T., Preece, R.C., Cacho, I., de Abreu, L., 2004. Ecological thresholds and patterns of millennialscale climate variability: the response of vegetation in Greece during the last glacial period. Geology 32 (2), 109–112.

Wen-Shu Huang, Shih-Hao Jien, Heng Tsai, Zeng-Yi Hseu, Shiuh-Tsuen Huang, 2016 Soil evolution in a tropical climate: An example from a chronosequence on marine terraces in Taiwan. Catena, 139, 61–72.

Lin Henry 2011 Three Principles of Soil Change and Pedogenesis in Time and Space. Soil Sci. Soc. Am. J. 75:2049–2070.

Sincerely,

Vasiliki Mouslopoulou and all co-authors



***New Figure 5:** Profiles of fan surfaces projected onto common planes parallel with the modern Klados River channel (above) and parallel with the modern coastline (below). Horizontal and vertical scales for each profile are similar, with a VE of c. 1.25. Note that the downstream slope of the upper and lower-fan surfaces are about the same, both a little steeper than the slope of the modern stream channel. Also note that the upper-fan surface slopes less to the east than the lower-fan surface, so that the upper-fan marine cliff is higher at the eastern end of the beach than at the west. The highest elevation of the lower-fan surface is close to the foot of the incision of the upper-fan, while the upper-fan surface is highest at its incision point. We schematically illustrate the volumes beneath the measured profiles to indicate the likely extent of the volumes of upper fan materials (pink) and lower fan materials (green) and have added the locations of each of the luminescence sample points (annotated yellow dots).”*

Updated Figure 6b: Looking west across the Klados River mouth (foreground), the uplifted shoreline attributed to the 365 AD earthquake (dashed red line, lower left) aligns well with a low terrace riser on the west side of the river (dashed thick white line, middle). Incision of the modern channel below the surface is attributable to post-earthquake adjustment to new base levels. Note the sea-cliffing of the last interglacial? marine bench (thick red dashed line at the top of the image) and the upper and lower fan deposits. The upper fan and western parts of the lower fan are overlain by accumulated rockfall debris and the solid thin white line approximates its surface. The lower fan surface is marked with fine dotted white line.