Clarifying points on response of Mouslopoulou et al. to short comment by Gallen and Wegmann.

We appreciated the detailed response of Mouslopoulou et al. to our short comment; however, the authors fail to adequately address some of our major concerns. Below we outline several weaknesses of the manuscript that still need to be addressed in the context of the response provided by the authors.

1) **Lack of sedimentologic and stratigraphic context.** In their response, the authors state that their specific objective to use geomorphic markers to assess tectonic signals obviates the need to place the Domata fan sequence in a stratigraphic and sedimentological context. To the contrary, the origin and genesis of the fan sequence is of utmost importance to evaluating their utility as a geomorphic marker. The interpretations forwarded in this manuscript hinge on the assumption that sedimentary deposit at the mouth of the Klados Gorge represents two fan units and that the cliff on the upper fan unit was cut by wave action. However, no evidence is provided to support the interpretation of two fan units. Currently the interpretation of Mouslopoulou et al. that the Domata fan sequence represents two fan units is based solely on morphology. As we pointed out in our short comment, this morphology is not unique to two fans, but can be equally well represented by a fill-cut sequence into a single depositional unit. Essential to the use of the Domata fan sequence is that the cliff in the Upper fan unit was cut by a marine incursion; however no evidence is provided to support this interpretation.

   The geochronology presented in the manuscript is unable to distinguish between two fan units (more on the geochronology below). Therefore, *the only way for the authors' to compelling demonstrate that the Domata fan sequence contains two fan units is by presenting detailed stratigraphic and sedimentological evidence.* The only revision that the authors present is to figure 5, where they now put their *interpretation* of the fan units without providing supporting evidence. Details and observations are required. The stratigraphic information is paramount to the goals of the paper, but the necessary details are not provided, and therefore, the authors' arguments remain unconvincing.

2) **The beach underlying the fan sequence and the 365AD paleo-shoreline.** In their response Mouslopoulou et al. seem to challenge the existence of this beach deposit. The beach deposit underlying the fans is clearly visible in the images that we provided. This is not an interpretation, but a *basic stratigraphic observation* and the photos provided in our previous comment show clear proof that the fan sequence overlies a beach. The existence of the beach is not in question.

   In our short comment we show evidence that portions of the fan sequence overly a wave-cut bench that is associated with the 365 AD paleo-shoreline. This simple, but key observation suggests that the fan sequence is Holocene in age and directly challenges the interpretations made in this manuscript. The authors' fail to address this observation in their response to our comment.

   We reason that because the fan buries a Holocene wave-cut bench, the beach buried by the fan sequence is likely Holocene and by association, was also uplifted in the 365 AD earthquake. Mouslopoulou et al. forward that this interpretation is flawed based on two reasons: (1) they suggest that the 365 AD paleo-shoreline is only represented by a bioerosional notch, and (2) that the elevations of the beach and notch are separated by several meters along the modern coastline. First, the 365 AD paleo-shoreline is not only represented as a bio-erosional notch. Many uplifted

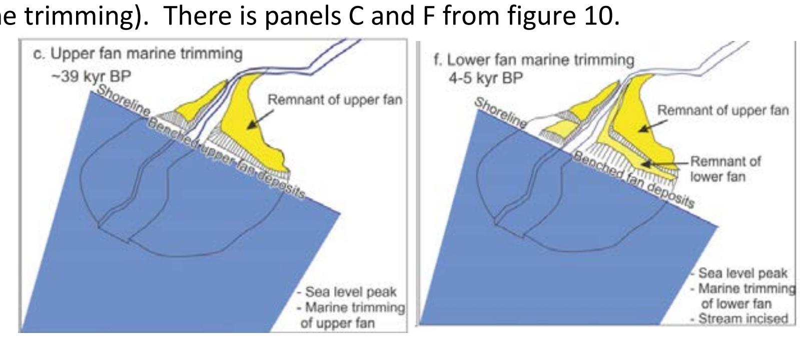
Holocene beaches are preserved all along the western and southern coastlines of Crete (e.g. in Paleochora, Moni Chrisoskalitissis, Damnoni, and numerous other locations). Lateral facies changes are common along the coastlines of Crete and every other coastal environment on Earth. As an example, take the modern Domata shoreline. It is a sediment-rich beach at the mouth of the gorge and is a bioerosional notch on the limestone cliffs to the east and the west of the gorge.

The observation that the beach buried by the fan and the 365 AD notch do not coincide at the modern coastline is not only not surprising, it is an expected observation given the geometry of the different coastal geomorphic markers (paleo beach deposits and bioerosional notches formed in bedrock). Beaches have a topographic slope. The paleo-shoreline (e.g. the elevation where the bedrock notch and beach will meet) will be represented by the inner shoreline angle elevation. This is currently buried by the fan sequence. The 365 AD notch likely continues along the cliff face behind the fan (e.g. a buttress unconformity) and meets up with the inner shoreline angle of the beach upslope from where the beach deposits buried by the fan deposits are visible today.

3) **Luminescence geochronology.** While the authors more thoroughly discuss the luminance results in their response, they fail to demonstrate that the data is reliable. Rather, they explicitly describe problems with the data that more clearly demonstrate that the geochronology is questionable. To quote the authors "yes, there is evidence for incomplete bleaching. Unfortunately, the amount of unbleached signal inherited cannot be quantified clearly." This statement seriously undermines the interpretations presented in the manuscript because they so heavily rely on the geochronology.

4) **Relative age of fan units.** In their response to our comment Mouslopoulou et al. acknowledge that they did not conduct a proper description of the soils in either fan unit. Rather, they use basic visual observations augmented by a "knife penetration". Without quantitative descriptions of the soils it is difficult to compare results to the thorough descriptions of soil profile on other alluvial fans in Crete provided by Pope et al. (2008), Gallen et al. (2014) and Runnels et al. (2014). The soil profiles described by Gallen et al. (2014) do provide a basis for comparison, despite the authors' contention that they do not. Furthermore, as stated in our previous comment, the soils described by Pope et al. (2008) *<u>are not similar</u>* to the soils shown in figure 8.

One of the key points that we make in our comment is the similar morphologies of the cliff faces on the fan units. In their response the authors' mistake the inferred age of the fan units with the age of the cliffs cut into the fans. In the model presented in figure 10 and from the text in the manuscript the authors' infer that the cliff on the upper fan unit was cut at 39 kyr BP (figure 10, panel c. Upper fan marine trimming) and that the cliff on the lower fan was cut at 4-5 kyr BP (figure 10 panel f. Lower fan marine trimming). There is panels C and F from figure 10.



> This interpretation states that the Upper fan cliff is ~ 35 kyrs older than the Lower fan cliff. So, again we reiterate "*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)? When fault scarps are formed in unconsolidated alluvial fan sediments in places like the Basin and 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.*"

In summary, the authors fail to adequately address our major concerns with the manuscript. The lack of basic stratigraphic observations, questionable geochronology, and poor relative age control on the fans makes it difficult to support their interpretations.

Sincerely,

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