Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC2
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# Comment on esurf-2021-4

Anonymous Referee #2

Referee comment on "Stochastic alluvial fan and terrace formation triggered by a high-magnitude Holocene landslide in the Klados Gorge, Crete" by Elena T. Bruni et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-4-RC2, 2021

This study uses a combination of stratigraphic observations, geochronology, and numerical modeling of landslide runout to propose a revised explanation for valley evolution in a small catchment in southwestern Crete. The authors propose that a prominent sedimentary deposit at the river mouth and associated terraces resulted from deep-seated landslides in the catchment, rather than past climate change. Though the study is highly focused on a relatively small drainage basin, it covers a lot of ground methodologically and presents compelling visuals for this landscape.

Orienting the reader to keep track of all the methodological moving parts is a significant challenge. The manuscript could be substantially strengthened by (1) further explaining some of the key observations, and (2) reorganizing the text to more consistently separate the results from the discussion.

Regarding #1, The Introduction situates the work in the context of strath and fill terraces and alluvial fans. However, the largest geomorphic feature in this study sits squarely on a shoreline, and likely better described as a fluvial fan delta (see Sun et al. (2002), *WRR*, doi: 10.1029/2001WR000284). How if at all does this distinct geomorphic context affect how the present results are related to previous studies for river terraces and alluvial fans in non-coastal settings? The line-by-line comments below also note several places where the stratigraphic observations could be more fully explained (see comments for L220, L238, L311, L412).

Regarding #2, I found the text regarding the landslide modeling difficult to follow (see comments for L178, L186, L463, and L454). The model description appears abruptly in the Introduction, and could use further description there. Then the model results are shown in the Discussion (section 5) rather than the main results section (section 4). As a result, the landslide modeling feels pasted on, rather than integrated with the rest of the work. I think it is an impressive part of the paper, and worthy of inclusion in the formal results.

**Line-by-line comments:**

L137: “tidal notch” – consider providing a concise definition (and perhaps a citation) for this geomorphic indicator, which seems to be important for this study. Also, it could be helpful to briefly describe how this feature will be “used as a relative age marker” at this

point in the text.

L164: "Bulk sediment measurements" seems to be a vague title for this subsection, which focuses on radiocarbon dating. Suggest renaming to emphasize dating.

L178: The landslide model appears rather abruptly, and the specific objectives of the modeling are not stated until the end of this section (L196-200). For clarity, consider moving these objects to the start of the section. More explanation is also needed for these rheology models (e.g., Voellmy – not familiar with this model).

L186 "pre-landslide topography" – clarify whether you reconstructed the pre-failure surface for the landslides source area.

L211: Figure 3: for clarity, assign the sketch in the upper left as a formal subfigure (subfigure ("a"). Suggest also adding a word or two to describe each of T1, T2, T3, L1. Nice use of human for scale!

L220: "that T2 unconformably overlies a paleo-beach deposit" – this seems like one of the key observations to establish a new chronology for this landscape (and is highlighted in the abstract). Yet the observation goes by quickly and is tucked away (Fig. 4e) in part of a very busy figure. I suggest expanding this description, particularly to build the case that this is a paleo-beach deposit. Some of the related text comes in L263-264, but presenting all of the observations together would make it easier to follow.

L238: the subfigures in Figure 4 are discussed out of sequence, which makes the argument more difficult to follow.

Throughout: "Aeolian" à "aeolian" or "eolian"

L296: "river attempted to adjust its slope" – be careful about anthropomorphizing (a river cannot attempt to do anything).

L297-298: "deposits change vertically from unsorted debris flows at the bottom to layered sheet flows" – correct usage is "debris flow deposits" and "sheet flow deposits."

L311-312: The observed radiocarbon ages from the shells – 800 to 1000 years older than the inferred age of the uplift that raised the notch above sea-level – seems to pose a significant complication for the proposed timeline of events. For this scenario to hold, the shells would have needed to have been preserved for 800 years after the organisms' death. Is that plausible? This issue goes beyond my expertise, but I am curious. Perhaps an additional sentence or two, or a related example from the literature, could flesh out this point.

L356: In Table 2, it is unclear why there are 4 numbers listed under "Intermediate." The text mentions 6 wedges, is that related?

L412-413: The comparison of the radiocarbon dates with the existing IRSL dates is a critical point in this paper. I suggest going a bit further to explain who you think the IRSL dates could be biased, particularly in a way that is accessible to those outside the geochronology community. You think the IRSL samples included "of a mix of bleached and unbleached grains resulting in late Pleistocene ages" – can you expand on this point using more accessible language?

L463-464: How was "best fit" model determined?

L454-501: Section 5 is the Discussion, but these lines present a lot of additional results.

Consider moving this material earlier in the manuscript.

L511-536: Can you tie this sequence to Figure 8 using specific references to each of the subfigures?