Line-by-line responses to Anonymous Referee #2

1. 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.**

If comment (2) refers to the modelling, we see it as a point of discussion. We put the landslide modeling component in the discussion because it is an interpretation of the more substantiated results we obtained from mapping and geochronology. We use this modelling to reinforce the argument of the catastrophic sedimentary input and do not consider it a primary result but supplementary to our interpretation. Because it is an interpretation, positioning it earlier on in the manuscript might be perceived as inappropriate and out of place. However, we understand the reviewer's concern and have worked to streamline the presentation to ease readability.

2. 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, and L412).

The reviewer brings up a good point about precise terminology and we have revised the manuscript accordingly to describe the coastal fans as "alluvial fan deltas". We used "alluvial fan" in the original submission for consistency with other studies conducted on coastal alluvial fans in Crete and the fact that the stratigraphy preserved in the deposit is not deltaic in nature (e.g. no forests or bottom sets were observed). For clarification, we have also added stratigraphic sections to the manuscript (Fig. 6).

We do not think that the geomorphic context near a coastline affects how our findings relate to previous studies in non-coastal settings. Beyond coastal erosion, the deposits do not bear evidence of strong interactions with sea level or coastal waters (e.g. no topset-foreset pairs). Moreover, the clear continuity between the individual fans and terraces indicates a regular deposition process. This suggests our observations are upstream of significant sea level influence and, therefore, would be largely comparable with alluvial fan and terrace deposits observed in other settings.

3. 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.

Indeed, as even a short introduction to the modelling methodology requires a lot of specifics, we decided to include a detailed description in the supplementary section of the manuscript. However, the comment on a more in-depth description of the model in the Introduction is noted, and will be implemented into the revised manuscript. Specifically, we have worked to streamline the writing to improve readability and flow.

As noted above, the modelling is used to reinforce the hypothesis that a landslide caused the aggradation and incision cycles which are at odds with the deposits in the nearby valleys. We arrive at this hypothesis based on our primary field observations and data; it is, therefore, regarded as an interpretation of the result. For this reason, we think it is more appropriate to place all discussion of the landslide modeling in the discussion section of the manuscript. But we are thankful for the comment, and will have to discuss the implications of including it as a formal result.

Line by line responses to Anonymous referee # 2

1. 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.

This is an excellent point. We have revised the text to: “These paleoshorelines delineate the temporal position of sea level through tidal or bioerosional notches, cemented beachrock, topographic benches, and shore platforms (Chappell, 2009). The uplift of a Holocene paleoshoreline by as much as 9 m a.s.l. on the southwestern coast of Crete is often attributed to an unusually large earthquake (MW 8.3–8.5) in AD 365 (Mouslopoulou et al., 2015; Shaw et al., 2008), but a more recent study suggests that uplift occurred through a series of earthquakes with Mw < 7.9 in the first centuries AD (Ott et al., 2021). Regardless of conflicting interpretations, this prominent paleoshoreline is observable along > 200 km of coastline in western Crete and provides a robust Late Holocene time marker. Following Ott et al. (2021), we refer to this Late Holocene coastal feature as the Krios paleoshoreline, based on its maximum elevation at Cape Krios in southwestern Crete.” (line 150-158)

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

We agree and have clarified this term as “bulk sediment dating”.

3. 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).

We agree with the reviewer and will introduce the aims of the modelling and the rheology models more clearly, possibly along the following lines: "To test the feasibility of the hypothesis that a rockfall turned landslide provided the necessary material to form the large sedimentary deposits throughout the valley, we utilised [...]" (213-214).

"Several studies report successful model results for landslides when a Voellmy or frictional rheology is used as the basal rheology, and several back-analysed historical events are available using these rheologies (Aaron and Hungr, 2016; Grämiger et al., 2016; Hungr, 1995; Nagelisen et al., 2015). Adding to the basic frictional rheology equation, Voellmy rheology includes a "turbulent term" which is dependent on flow velocity and the density of the material and summarises the velocity-dependent factors of flow resistance (Hungr and Evans, 1996)." (line 216-220)

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

We revised the text here for clarity as suggested. We also point the reader to section 4.6. Volumes of rockfall and valley infill (line 411-417).

We quote from the revised text: "We produced a DEM of the modern landscape without the Holocene deposits mapped in this study as the pre-landslide topography (DEMpre). For this, the thicknesses of all deposits were subtracted from the present-day topography (Fig. S2). The pre-failure surface for the source area was reconstructed using the thicknesses of the reconstructed rockfall wedges creating a rough minimum estimate of the mountain face's bedrock topography before the landslide event." (line 226-230)

5. 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, and L1. Nice use of human for scale!

Good point. We have revised the figure accordingly.

6. 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.

This is a good point. We wanted to separate the results and interpretation strongly in the original submission, but recognize that this is a critical observation. We have therefore revised the text to add more discussion of this key finding here.

We have also added a new figure highlighting this key observation.

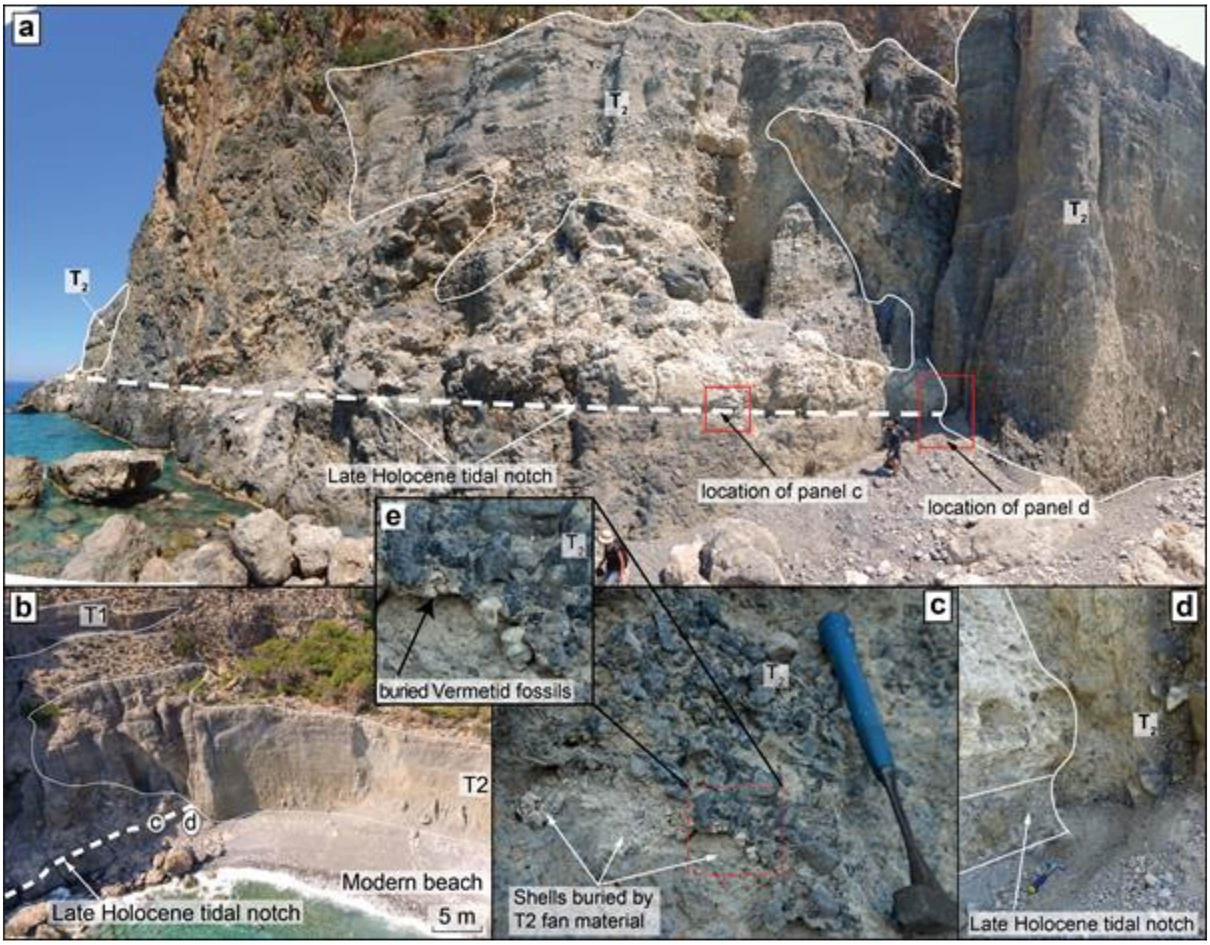

Figure 5: The contacts between the tidal notch, T_2, and the paleobeach are illustrated by photographs from the west side of the study area. (a) Overview showing the unconformable relationship of the Late Holocene tidal notch and the T_2 fan highlighting the location of figures in other panels. (b) Oblique aerial perspective view of the outcrop with the major features highlighted. (c) Detail of the Vermetid extraction site shows how gravels of T_2 overlie a Vermetid shell pocket in the tidal notch. (d) Detail of the contact zone between the carbonaceous bedrock, T_2, and the tidal notch (partly buried by colluvium). (e) The Vermetid fossil pocket is covered by T_2 fan material (detail of (c)).

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

We thank the reviewer for this comment. We corrected the sequence to follow the appearance in text in the revised manuscript.

8. Throughout: “Aeolian” → “aeolian” or “eolian”

We use “eolian” in the revised manuscript.

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

Fair point. We revised this sentence.

10. 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.”

We made this change.

11. 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.

Firstly, the reported radiocarbon ages cannot be directly compared with calendar years, as they have not been calibrated. We adjusted the manuscript to include calibrated calendar years of the fossil dates to ease comparison, which reduces the discrepancy. Secondly, there are three options to explain the old ages. Either (1) the paleoshoreline (tidal notch) was not uplifted in one single event as proposed in previous literature (Pirazzoli et al., 1982, 1996; Shaw et al., 2008; Stiros, 2001), but is the result of gradual uplift (Ott et al., 2021), or (2) the organisms were killed and preserved by intermittent burial by older T_1 deposits, or (3) the organisms have really been preserved for this amount of time. We lack data to distinguish between these possibilities but none of these options has any effect on our primary conclusions.

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

Thank you for the comment, we will clarify in the text that of the 6 wedges, 2 relate to the maximum and minimum values and only 4 to the intermediate-sized wedges. It is worth highlighting that the maximum value is oversized and was not used in any of the subsequent analyses.

13. 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 why 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?

We thank the reviewer for this comment. We revised the text to provide a more detailed description of the biases that the previously published IRSL samples might suffer from. We quote from the revised text: “Luminescence burial dating of deposits exploits the assumption that charge is gradually built up in feldspar or quartz grains due to radiation from radiogenic decay of radioactive elements and cosmic rays. To relate the amount of charge a grain releases as luminescence signal to the duration of sediment burial (depositional time of unit), all charge within the crystal lattice needs to be fully released by sun bleaching before deposition; a process that requires seconds of full sun exposure for quartz and minutes for feldspar (Rhodes, 2011). Alluvial fans, especially in small catchments with short transport and a significant portion of debris flow deposits, are therefore prone to biases in luminescence measurements because the short transport in sediment-rich flows usually does not allow for a complete bleaching of the mineral grains, and especially not feldspar (Rhodes, 2011). This effect is enhanced because minerals freshly released from the bedrock have worse luminescence characteristics and take longer to bleach (Rhodes, 2011).

The anomalously old luminescence ages reported by Mouslopoulou et al. (2017) are likely biased due to incomplete bleaching caused by the turbulent mode of transport (Rhodes, 2011). The broad positively skewed age distributions of measured equivalent dose measurements (the amount of charge released from the grains) in Mouslopoulou et al. (2017) from feldspar IRSL indicate a mix of bleached and unbleached grains resulting in late Pleistocene ages for both fan units. The mixture of bleached and unbleached grains is especially evident because Mouslopoulou et al. (2017) also measured the quartz OSL signal, and found the same positively skewed age distributions but with younger ages. The discrepancy between the younger quartz OSL and older feldspar IRSL measurements can be explained by the more rapid bleaching of quartz grains; however, these authors discarded and did not report the OSL ages choosing instead to construct their interpretation on the IRSL measurements alone." (line 478-498)

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

We added some text to this point in the revision. In short, we largely relied on runout distance, speed and model thickness to define the best-fitting model. For example, we discarded models with maximum slide velocities of sound speed or larger, and travel times of less than 1 minute (see Table 3). The best-fit model reproduces our field observations of deposits up to 100 m above the modern stream channel, and reports the most realistic natural outflow, but of course still contains a lot of assumptions.

15. L454-501: Section 5 is the Discussion, but these lines present a lot of additional results. Consider moving this material earlier in the manuscript.

The reviewer raises an important point that we discussed during the process of writing this manuscript. Though the landslide modelling does show important additional results that are presented in the discussion, the whole idea of doing a landslide runout model hinges on the interpretation of the alluvial deposits. To generate a logical flow and now jump ahead with interpretations in the result section, we chose to present these results in the discussion section of the manuscript.

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

Yes, we can (Sect. 5.5; Fig. 10).

References used by the authors in the response

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