Dear Associate Editor Pr. Metivier,

Below you will find our detailed responses to Dr. Boulton's review of our manuscript entitled "River profile response to normal fault growth and linkage: An example from the Hellenic forearc of south-central Crete, Greece". In this document we detail where we have made changes to the manuscript to reflect comments or criticisms raised by Dr. Boulton or explain why we respectfully disagree with a specific comment. Since the discussion period is still open, we are awaiting further reviews before submitting the revised version of our manuscript.

Sincerely,

Dr. Sean F. Gallen & Dr. Karl W. Wegmann

Interactive comment on "River profile response to normal fault growth and linkage: An example from the Hellenic forearc of south-central Crete, Greece" by Sean F. Gallen and Karl W. Wegmann

S. Boulton (Referee)

sarah.boulton@plymouth.ac.uk

Received and published: 21 December 2016

This paper investigates river response to uplift along the southern margin of Crete, using well known fluvial geomorphic metrics and previously published data on the rate of uplift and behaviour of recently linked normal faults. The paper draws some interesting conclusions on the role of river capture on river profile evolution and the potential pitfalls of this technique in some areas. The background to river analysis and the geology/geomorphology of the study area are very well presented and described as are the results. The discussion is well structured and outlines the authors arguments well. There are only minor comments that need to be addressed prior to publication.

*We thank Dr. Boulton for her thorough review and below we detail how we have addressed each of her specific comments in the revised version of our manuscript. Dr. Boulton's comments are in **bold text** and our responses are in blue italics.*

Page 3, lines 10-15. This paragraph sets up an argument based upon the work of Attal et al., (2015) and Shobe et al., (2016). Shobe et al., is described but the work of Attal is not elaborated upon and instead Brocard et al., (2016) is introduced. I find that this is rather unsatisfactory as Attal et al.'s contribution is unclear in this review.

We agree with the reviewer and have elaborated on Attal et al.'s contribution to our understanding of this topic. We have also revised the text so that the Brocard study is better integrated into the paragraph by removing the initial sentence that specifically mentions Attal et al., (2015) and Shobe et al., (2016) because it was unnecessary. The new text is as follows:

"Several recent studies highlight observations of spatially heterogeneous changes in the flux and size of sediment delivered to the channel in transient landscapes that might be missed by simple expressions of sediment flux that are only scaled to local channel slope and drainage area, a proxy for discharge. Attal et al. (2015) document that hillslope steepness and erosion rates are positively correlated to changes in sediment grain size on hillslopes and along the adjoining river channel bed in catchments undergoing transient adjustment."

Page 7, line 6. Missing parenthesis after (Fig. 2.

We have added the closing parenthesis.

Page 7, Lines 10 and 11. See also Kent et al., (2016). Kent, E., Boulton, S. J., Whittaker, A.C., Stewart, I.S., & Alçiçek, M.C. 2016. River profiles as recorders of fault linkage and slip rate increases in the Gediz (Ala¸sehir) Graben, Turkey. Earth Surface Process and Landforms. Doi: 10.1002/esp.4049

Thank you for pointing us toward this relevant study. We have read this new research and have included the reference into our manuscript.

Page 9, line 19. Headward

We have corrected this spelling error.

Section 3.1/4.1. These sections incorporate a monte carlo approach to calculate the uplift rate along the coast and at individual sites, a nice idea but the results leave me with a number of questions/comments. An average uplift rate is determined, shown in figure 3a (I think the figure caption should state the uplift rate or it should be shown on the figure). However, it needs to be made clearer that these are post-linkage uplift rates.

We have added a sentence to the figure caption indicating that the site average uplift rate is for site 6. To better clarify that figure 3b shows post-linkage uplift rates we have changed the caption to state "b. Post-fault linkage site average uplift rates…".

Also I would have liked to see the authors try to narrow down the timing of linkage, as currently they simply use the previous estimate of < 1 Myrs ago.

In the first section of the discussion (section 5.1 Long-term versus intermediate vertical displacement patterns) we discuss the rational that we use to approximate the timing of linkage. Our rational for placing this calculation so late in the text is because it is an interpretation and would be difficult to explain elsewhere in the text.

Also have you any constraints on pre-linkage uplift rates?

We do not have any independent constraints on the pre-linkage uplift rates. We had initially hoped that we would be able to: (1) use the post-linkage uplift rates and lowest river profile segments that cross the normal fault footwalls in order to empirically derive variables in the stream power incision model. (2) With a calibrated incision model, we would then be able to infer pre-linkage uplift rates from the middle river profile segment (the segment between the lower and higher knickpoints) from each footwall. However, for reasons that become clear in the manuscript, we lack confidence in our empirical calibration of the stream power model for south-central Crete, and thus decided not to infer uplift rates from the river profiles.

Although the mouth of the rivers on the Dikti block enter the sea on the hangingwall block, knickpoints in these rivers are still going to be formed by the initiation of faulting or change in footwall uplift rates on the SSCF. How do the rates of hangingwall and footwall uplift compare? Many studies of the hangingwall to footwall motion cite ratios of 1/4 to 1/3 partitioning.

We do not have independent constraints on the rates of uplift on the Dikti footwall; however, we can compare the post-linkage uplift rates from the hanging wall and footwall of the composite fault system near the site of linkage (see figure 3b). This analysis was initially reported in Gallen et al. (2014), in which we published our marine terrace chronostratigraphy and rock uplift results that we utilize as a starting point for this manuscript. In Gallen et al. (2014), we found that the hanging wall-to-footwall uplift rate ratio is roughly 1/3.

Page 12 section 3.3 I am interested that you have defined knickpoints as a 25% difference between Ksn upstream and downstream, what is your rationale for this number?

This number is admittedly arbitrary, but was sufficiently large to avoid inclusion of small jumps in river channel steepness that are more likely to represent noise or artifacts in the DEM, while also being small enough that we could identify inflections in all river profiles analyzed in the study. To clarify this point we have added the following text:

"While this change in steepness is admittedly an arbitrary cut-off to identify knickpoints, it is sufficiently large to avoid including unwanted noise or DEM artifacts in our analysis and sufficiently small so that we could confidently identify inflection points in all river profiles analyzed in this study."

Is that consistent with where known active faults cross channels?

The jumps in normalized channel steepness (Ksn) where the channel crosses over active faults are generally higher than 25%. The mapping of active faults in the study area is detailed enough that we are confident that we can identify knickpoints related to spatial changes in rock uplift rate across mapped faults from transient knickpoints resulting from temporal changes in slip rate/uplift rate across said faults.

Section 4.3 Although I agree that the two sets of knickpoints represent two phases of development it would be nice if there was some test of this hypothesis. How about presenting distance migrated upstream vs catchment drainage area. Faults of the same generation should exhibit a power law relationship.

The suggested distance migrated vs. drainage area analysis was performed before our initial submission. We decided not to include this analysis because the metric χ can be used in much the same way and thus the extra figure provided little additional insight. Provided that substrate erodibility is roughly homogeneous, knickpoints of a common origin will have the same χ value. To clarify the ability of χ to assist in the determination of whether or not knickpoints are of a common origin we have added the following text to the end of section 2.5, our background on river profile analysis: "Furthermore, if substrate erodibility is homogenous, χ represents a measure of the river response time to a state change in incision (uplift) rate that is manifested in the formation and presence of a knickpoint. Knickpoints from a common origin will travel at the same rate in χ transformed space. In other words, knickpoints found at approximately the same χ distance are related."

It is worth noting that much of our discussion focuses on testing if these knickpoints are related. The most compelling evidence is the changes in the relative patterns of river channel steepness that are consistent with the geological evidence of recent fault linkage and the predictions of changes in the spatial and temporal patterns of uplift based on fault mechanics theory. Our discussion largely focuses on an attempt to understand why knickpoint travel distances for streams from the south-central coast of Crete do not conform to predictions of the stream power incision model.

Section 5.1 lines 18-21. Ah – I think that this information on the timing of fault linkage should be presented earlier.

We understand the desire of the reviewer to present this earlier, but we think that this calculation is better suited for the discussion because it is an interpretation. However, if the reviewer feels strongly that we should move this discussion to earlier in the manuscript we can try to accommodate this suggestion.

Page 15, Line 31 – . . .in uplift rate determinations. . .?

Thanks for catching this typo. The reviewer is correct and we have fixed the typo.

Page 16, line 21 (also page 17, line) What mechanism caused the first increase in uplift rate, if there is no linkage? What evidence is there for previous slow uplift? Why does a shallow river indicate slow uplift? Why could not the upper knickpoint represent the initiation of faulting?

This is an excellent question. We discuss this exact question at the end of this section. The reviewer brings up a good point about the possible origins of the signal of uplift recorded by the river reaches above generation 1 knickpoints and we address this below.

Page 18, lines 29-32. I might have misunderstood but this sentence appears to contradict the discussion two pages earlier, as you are now saying the upland areas are 'relict' topography from prior to fault initiation.

Dr. Boulton brings up a good point and based on the previous comment we have revised the second sentence of the last paragraph in Section 5.3.1 for clarification. The revised sentence is as follows: "Our interpretation is that many of these low-gradient reaches possibly record the background regional rates of rock uplift of the forearc prior to faulting and/or the uplift rates at the initiation of normal faulting along the south-central coast of Crete."

Page 19, lines 23. I know that assumptions need to be made, but having been to gorges in southern Crete, channel narrowing seems to be important and should not be discounted so easily. Perhaps saying that this variable is beyond the scope of the paper would be better than saying it is not important. This change might also explain some of the variability you observe in your data.

The reviewer is entirely correct and we acknowledge that so easily disregarding the influence of changes in channel width is not permitted. To clarify we have modified the end of the first paragraph in section 5.3.2 as follow:

"However, it is beyond the scope of this study to assess the impact of this variable on river profile analysis. Nonetheless, our results demonstrate that rivers in south-central Crete steepen in response to increased uplift rates. Under the assumption that river channel steepening is the dominant mechanism by which rivers respond to changes in rock uplift rate, regression through k_{sn} – uplift rate data by an orthogonal least-squares method can be used to empirically calibrate parameters in the stream power incision model under different assumptions."

Page 20, line 24. Whittaker and Boulton also (2011) demonstrated that knickpoint migration is a function of uplift rate, with higher uplift rates resulting in more rapid migration of knickpoints through the landscape.

We are aware of the empirical work of Whittaker and Boulton (2011) on the relationship between knickpoint migration rates, climate and uplift. Here, however, we are simply referring to theoretical considerations and have modified this sentence to make this explicit. The revised sentence is as follows: "It is important to note that Eq. (9) does not incorporate a component of uplift, such that from theory alone, knickpoint celerity is not dependent on uplift rate."

We note that while Whittaker and Boulton (2011) document a correlation between rock uplift rate and knickpoint travel distance, the exact factor determining those changes in unknown. For example, unaccounted for changes in channel width, sediment flux, sediment grain size, and or rock fracture may change in concert with rock uplift rate. In such a case, this would result in changes in the slope exponent, n, or erodibility parameter, K, in the stream power incision model. In such a case, changes in n or K would be responsible for driving changes in knickpoint celerity, rather than uplift rate, although these parameters might be dependent on rock uplift rates. However, we think that an extended discussion of the work of Whittaker and Boulton (2011) is beyond the scope of this study as we are merely trying to illustrate the link between river channel response time and the erodibility constant.