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# *Interactive comment on* "Top-down and bottom-up controls on mountain-hopping erosion: insights from detrital $^{10}$Be and river profile analysis in Central Guatemala" *by* Gilles Y. Brocard et al.

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General: This manuscript addresses the history of landscape and river evolution in the mountain ranges across Gautemala and southern Mexico where the North America – Caribbean transform plate boundary meets the Cocos – Central American convergent (volcanic arc) boundary. The area is tectonically complex at present and in the past because of this setting. More relevant here is that the faults are large and have grown over millions of years; therefore, the topography has changed because of the faulting and the resultant climate and landscape processes have reacted to this evolving setting in complex ways. The history studied here stretches over 12 Myr from the middle



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Discussion paper

Miocene to the modern, and therefore a multitude of methods are used across geomorphology and tectonics. I find this a fascinating and potentially enlightening region, but its complexity challenges the researchers when writing papers for an outside audience. The main point of the paper is the variable response of erosion and rivers on two adjacent ranges that were uplifted sequentially in the past 12 Myr across the study area. The authors argue for two main conclusions – that the younger range erodes faster, and erosion correlates with the amount of precipitation, whose distribution is controlled by the rising ranges themselves. They present numerous alternatives to these main conclusions, but in a manner that seriously distracts from the presentation of the meaning of the results. There is a good paper hidden in this manuscript, but the present version needs major revisions. The writing is fine in the Introduction, Methods, and Results sections, though these sections need one more thorough editing. But the writing is much more opaque in the Discussion, and that section needs some serious rewriting or reorganization. The Discussion section badly needs clear topic sentences leading the paragraphs. Many sections of the Discussion include multiple conclusions but not a clear statement at the beginning or end on which was the dominant process, or conclusion. Here are my suggestions for improving the Discussion. First, part of the problem may be the approach to the conclusions of this study using top down and bottom up controls - at first this seems logical, but it also seems to unnecessarily make the author's discussion of the results and the conclusions more complex than necessary. There is a story to tell here that has more universal implications for landscape evolution processes elsewhere, but first we need to hear the local story told in a succinct, coherent fashion. Second, the relation of the main conclusions to the study results in the Discussion section is not at all clear. The authors start the manuscript in the Abstract with two main conclusions from the consecutive uplift of two adjacent ranges since the mid Miocene. The Discussion has no focus on these larger conclusions, but instead wanders across many topics related to the study area. I would completely rewrite the Discussion to start with a section that focuses like a laser on which results from this present study support your main conclusions. Then it would be fine to add a section in

which you can digress to a discussion of alternative conclusions, but I would keep this section short. I would conclude the Discussion with a section on a succinct summary of the 12 Myr history of the landscape evolution of the study area that makes clear where the new results contributed. Many of the figures need improvements – much better explanations (legends) elucidating the details in the figures; more complete captions; better links between figure content and the text. See my comment on figure 5 as an example. And the manuscript needs more citations of the figures in the text. One more regional figure is required for the Introduction and setting sections, and much of the discussion of tectonic controls. There are many references to the larger faults and river drainage areas that lie outside the many map figures of the study area. Most readers will require a new intermediate scale map that covers the region around the study area. That new map should show the larger context of the topography and faulting - I think a topo base (geomapapp?) with the main faults is sufficient. The rough area of such a new map should show the offshore beyond the trench to the west and well into the Caribbean to the east - that is a good area of S Mexico and Honduras.

Specific Comments: (I did not edit the manuscript in detail – the manuscript needs one thorough edit) A few comments relevant throughout the manuscript: âĂć Check for use of modern usage of unit abbreviations throughout: millions of years ago = Ma; million years duration = Myr, so rates are m/Myr. Ages are XX Ma. âĂć Watch for overuse (or any use?) of anthropomorphic language when referring to geology and geomorphologic features. I give a few examples below. âĂć I give only a few examples here, but there are many, many examples of where the writing could be shortened with no loss of content or meaning.

Abstract: It would be much better to get the reader into the link between mountain range uplift and other parameters from the start by adding the names of the ranges in the Abstract: “two parallel, closely spaced mountain ranges formed during two consecutive pulses of single-stepped uplift, one from 12 to 7 Ma (Sierra de Chuacús), and the second one since 7 Ma (Altos de Cuchumatanes).”

Figure 2 and 3: more clearly label the Maya surface. Lines 89-91: The manuscript is longer than it needs to be. One example is writing that could be more concise with one example provided: In Middle Miocene time, the topography of Central Guatemala was very subdued . Remnants of this low relief (referred to as the Maya surface) still cap many mountaintops across the study area (Fig.2). The low middle Miocene relief formed from the topographic due to the decay of Eocene folds (Authemayou et al., 2011b; Brocard et al., 2011). Lines 72-73: Delete this sentence – you just said this in the line 51 paragraph or tell us for what purpose did you investigate these ranges? line 113: "westward decrease in the length of river deflections along the Polochic fault" this is not clear – if anything the black arrows in figure 2 are increasing in length to the west. Do you mean "increase"? If not, this interpretation needs to be better explained. line 114: "consecutive to an earlier rise" clumsy use of consecutive – better(?) to say "caused by an earlier rise" or "following an earlier rise" Line 121: Ixcán fault is on figure 5 and not on figure 4. Line 131-135: Cite figure 1 for the lake. And again a map that covers a larger area would show the releasing bend. And the eastern end of the Motagua fault is not on any map. Line 138: "Slip on the Motagua fault is purely left-lateral today. . ." Despite the major bend through the study area? Are you sure it is not transpressional along its western bend? Line 140 paragraph: Cite figure 5 and add the name Subinal Fm to figure 5. Line 144: high angle faults instead of steep angle faults Line 157: cite your figures more – here cite figure 1. Line 159: up to the editors, but most journals would prefer non-anthropomorphic words to replace "benefits" and "at the expense of" a catchment. There are many examples of this writing style across the manuscript. Line 169: should be figure 4, not 3. Line 195 paragraph and Figure 5: Add the formation names to the figure 5 explanation. Add the metamorphic to N America basement line on figure 5. Line 207: "processes in its carbonate rocks generates complex. . ." make it clear: "processes in the carbonate rocks of the AC range generates complex. . ." Line 209: better: "over the carbonates in Late Cretaceous (Campanian) time." Many (most?) geoscientists don't know the time scale at the stage level. Line 243: do you mean the timing or rate or what parameter

of hillslope erosion? Figure 8: The four maps are A, B, C, D (capitalized). Lithologies would be more clearly labeled as a, b, c, d rather than Greek letters.

Section 3.6: most of the knickpoint classification would be better moved to supplemental material. Then you could just keep the text on the other methods related to the knickpoint analysis.

Line 446-447: cite your figures 2 and 3 for Maya surface. Line 452-453: I would add the new ages to figure 2 and cite that here. Does the incision have to be immediately after the 12 Ma lahar? How much after can it be – that is what is the constraint on the younger side of the window of possible incision timing? Where is the Cuilco River valley? The Colotenango valley? Line 460: here and elsewhere: mixing abbreviations SC range etc with the occasional Sierra de Chuacús spelled out is more confusing than sticking to one nomenclature. Line 470: an elongate basin is a variation on a trough – drop the trough and just refer to it as a basin. There is no "trough basin" in the world of sedimentary basins. Lines 474-483: much of this argument is not convincing, or at least a role (major?) for faulting cannot be disregarded based on your points here. Sediments bypassing an actively faulted basin are common with high enough sediment flux and an overfilled basin. The climate – erosion machine can easily outpace subsidence from faulting. You may well be missing evidence for normal faults cutting alluvial fans because that evidence lies deeper in the subsurface of the basin – this happens all the time in young basins where only the upper alluvial fans are exposed. The last point is valid but with a very large offset strike-slip fault, how much differential offset across it would one expect? Might the two paired strike-slip faults work in unison to result in roughly the same elevation of old surfaces across the Motagua fault? Line 490: "Using the modern gradient of the Huijo River valley as a proxy. . .". But above in the text you give results showing that at 6 Ma the SM range incision was slowing greatly over a few Myr. So is it valid to compare the modern gradient to one at 6 Ma? Was the river gradient also higher at about 6 Ma? Section 4.1: Overall I find the conclusions in this section solid.

Discussion: Line 645 – 647: Would a simple graph of study-derived erosion rates vs precipitation at that site be effective here? Section 5.2.1.2: This section is emblematic of the mixed conclusions resulting partly (largely?) by the organization of the Discussion. But this section (and others) also needs a clearer initial statement of the conclusions of your study relative to the topic of the section. You state up front in this section that "we conclude that the increase observed across the AC is a response to faster tectonic uplift" – Im assuming the increase you refer to is the increased river gradient across the AC range. Then you spend most of the section discussing what seems like a better explanation of the control of the bedload type on the rivers. Figure 15: which parts of these three rivers do these profiles represent? What is point 0 km on the diagram mean? What are the symbols on the three profile lines? Section 5.3: this section is the epitome of the problem I have with the Discussion section as a whole. The section does not focus the reader on the results from the present study, and the conclusions from those results. Instead, the section is a jumble of alternative conclusions about the migration of knickpoints. In addition, to even consider the statements on the tectonics of the study area requires the new figure I asked for of a larger area.

Conclusions: I would repeat the main conclusions from the Abstract here, then organize the conclusions within that context. Line 975: The relation of the conclusions of this study to orogenic plateau growth is a stretch here because the authors do not have the space to make the case. I would drop this theme and this conclusion here and earlier in the paper.

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