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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.

Gilles Y. Brocard et al.

gilles.brocard@mom.fr

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Reply to interactive comments on Âń Top-down and bottom-up controls on mountain-hopping erosion : insights from detrital 10Be and river profile analysis in Central Guatemala

Overview

We modified the title such that it better reflects the contents of the paper. The new title makes more explicit reference to the contribution of climate and tectonics, and to the discussion on landscape evolution.

Based on remarks of referee #2, we rewrote the end of the introduction to clarify the structure and intentions of the paper, simplified the description of the study area (section 2). We reworked the structure of the discussion, expressing more clearly the respective contributions of precipitation (5.1) and river incision dynamics (5.2) to the slowing down of erosion over the old range. Following remarks from referee #1, we provide a more detailed assessment of the contribution of drainage dynamics to the evolution of incision rates in section (5.2). We then continue the discussion (5.3), like in the first version, with a review of some of the consequences of the slowing down of erosion rates on the topographic evolution of the old range. However, we sieved out interpretations that are mostly of regional significance, moving them to Supplement 4. We believe that the paper is much tighter this way.

Replies to anonymous referee #1

We thank anonymous referee #1 for his comments, which helped us reorganize the paper and make the effects of climate and tectonics more clearly presented.

Replies to the general comments:

Overview Anonymous referee #1 shows a great fondness for drainage divide migration at drainage dynamics at equilibrium, and holds strong views against precipitation control on erosion. In short, the referee seems to ascribe the slowing down of erosion along the northern flank of the older range to "drainage dynamics", or, in mechanistically more explicit terms, to the transient response of the drainage to the enlargement of the range, presumably from one state of dynamic equilibrium (uplift of the old range) to another (uplift of both ranges). The referee therefore dismisses the "top-down", climate driven control on erosion, claiming that only drainage reorganization is at work. From the data at hand, we find that both processes are at work, and state this more clearly in the discussion. The paper is not focused on quantifying their respective merits, but rather on how they team-up to produce the range-hopping erosion pattern.

Referee #1 claims that we ignore the effect of drainage dynamics even if we actually devote an entire section of the paper to bottom-up processes (specifically the effects of the rise of the new range, and of strike-slip tectonics, on the slowing down of erosion rates farther upstream). Referee #1 repeatedly analyses this non-equilibrium landscape within the framework and with the rhetoric of dynamic equilibrium. This has limitations when analysing such a complex landscape, which starts, and remains, far from equilibrium. As a result, inferences made at equilibrium are often disputable. The criticism of the effects of climate on hillslope erosion is based on objections regarding the meaningfulness of field data. Admittedly, field data are rarely optimal (here, due to issues of data acquisition in perilous circumstances). We introduced a test of significant that shows that these data do support a positive relationship between erosion rate and precipitation rather than otherwise. For some reason, referee #1 seeks to find support for drainage divide migration and drainage dynamics using a chi-map. The surprising use of the chi-map itself is detailed hereafter. Notwithstanding, referee #1, while questioning the increase in erosion from the old range to the new range, ignores the fact that the very same dataset clearly shows no significant difference in erosion rate on either side of the older range that would indicate drainage divide migration. Referee #1 likewise does not seem to bother that such migration would have destroyed the remnants of the old Miocene surface. The various pulses of uplift and incision that have affected this landscape have not transmitted their signal as far as the divide yet. In our earlier papers, we reconstructed in detail the evolution of the drainage, based on geomorphology and sediment provenance. It is beyond the scope of this paper but it also demonstrates the absence of the divide.

Specific replies to comments

The first problem is that it does not adequately synthesize the large quantity of data that is presented. Many of the figures are confusing and contain too much information, and too much detail is included in the text. I recommend omitting (or moving to the supplement or another paper) all information that is not crucial to testing one or two hypotheses.

Were are not testing one hypothesis against the other. We show how our broad array of field data is consistent with the influence of both processes, and what their morphological expression is. Simplifications have been introduced, however, which details are provided in the response to referee #2.

The second problem is that the main conclusion that the observed spatial variation in erosion rates is controlled by climate is not supported by the evidence. The main conclusion does not state this anywhere. It actually states that both climate and drainage dynamics are important (hence the title, which stresses the importance of both processes).

The correlation between the erosion index (which take into account precipitation) and erosion rates appears to be weak (no measure of correlation is presented)...

There is a threefold increase in erosion rates from the older range to the younger range, associated to a twofold increase in erosion index values. These data do not show, therefore, an absence of increase, nor a decrease, from the older to the younger range. In detail, the correlation is strong among the sites in the younger range, and weak among sites within the older range. In the discussion, we briefly discuss the absence of correlation within the older range (lines 667-671). We would happily discuss this in length, but it would distract the reader from the main plot.

... and drainage basin dynamics (which appear to be important here) are not considered.

In the very introduction (line 56-59), we stress that drainage dynamics is the primary motivation of this work, following our previous studies of drainage dynamics in the area (Brocard et al., 2011; 2012).

In a chi map for the region (see Giachetta and Willett, 2018, A global dataset of river network geometry), one can see that the slowly eroding northern flank of the older range are tributaries to a large basin that is predicted to be losing drainage area (see bottom left of attached screenshot in which one basin has higher chi – warmer colors – than the surrounding basins).

It does not seem to bother you the least that the calculation of chi starts far away from the studied range, and therefore that the streams reach the range (in the upstream direction), with chi values that already “predict” drainage migration. The very use of this chi map is problematic anyway here, given the lack of spatial and temporal homogeneity and the lack of accurate weighting, on this chi-map, of precipitation, rock uplift rates, rock erodibility, fluvial processes, and base level stability effects. Considering that no of these parameters is spatially nor temporally constant, teasing out their respective contributions to the chi values is difficult. In the case at hand, chi values capture changes in basin shape and river length of streams that drain karstic lowlands (in the north), bypass lowlands, and feed subaerial sediment depocenters. If we could quickly rewind the evolution of chi-values at the divide, we would see that they are affected by high-frequency differential oscillations triggered by changes in sea level, an unequal increase in chi values on both sides of the divide, related to the lengthening, over at least the past 5 Ma, of the outlet rivers over their shelves in the Caribbean Sea and in the Gulf of Mexico, and then, within the range, a drop in chi-value on the northern side 7 My ago, resulting from the integration of many parallel rivers with elongated catchments, into a single, larger, more dendritic drainage, and then, likely, a drainage reversal of Rio Motagua, on the southern side, with a big increase in chi values. Therefore, the current differences in chi values across the divide, 1st do not mean much in terms of drainage dynamics, and, 2nd, in effect, never led to divide migration, as documented by the field data (clast provenance studies on the northern and southern sides, preservation of the Miocene paleo-surface along the divide).

Higher chi means higher predicted steady-state elevation. Thus, the basin will need to steepen for erosion rates to keep up with uplift rates. In response, erosion rates slow and the region is passively uplifted. The geometric disequilibrium could be a result of initial geometry or potentially a result of lengthening of the mainstem along

the strike-slip faults. Deformation of channels along the faults is mentioned, but how that lengthening might impact erosion rates upstream is not addressed.

These two points are actually addressed in section 5.2.2, and correspond to the “bottom-up” controls on incision in the older range. To follow your reasoning in the context of dynamic equilibrium, we need to view the rise of the new range as an enlargement of an equilibrium prism. In the new section 5.2.. we discuss is more detail how the rise of the new range results in surface uplift of the old range. Likewise, the impact of lengthening is addressed in the same section: “maintenance of a downstream gradient sufficiently steep (. . .) along the lengthened reaches, to convey the sediment load, requires additional surface uplift, upstream of the tectonic deflections, which may contribute, in part or in whole, to the current passive surface uplift of the SC range”. In the new version, we provide an estimate of how much uplift can be ascribed to uplift and lengthening.

Furthermore, the mechanism by which precipitation would impact erosion rates is not clearly explained. They are clearly specified both when presenting the erosion index (methods), and then in the discussion of climate influences. Besides, the positive correlation between precipitation and erosion is commonly observed in temperate and subtropical climates.

Theory says landscapes adjust their erosion rates to keep pace with uplift, so spatial variations in precipitation (with uniform uplift) should manifest as differences in normalized channel steepness (ksn), with steeper rivers in drier places, rather than differences in erosion rates.

Precisely: Ksn values are high in the younger range, despite wetter conditions and higher erodibility. This supports the point that the range is uplifting faster (and, measurably, erodes faster). However, because rivers are not all detachment-limited, we considered that the use of the behaviour of Ksn could be misleading. This is why we analysed separately the behaviour of Ksn according to segments that are boulderarmoured or transport-limited.

However, it's possible aridification resulted in a dramatic decrease in K (erosional efficiency of a river) which would lead to an increase in response times in rivers draining the drier zone, and so you wouldn't see steeper rivers in drier zones because the basin has not had time to respond (and yes you would also see stalled knickpoints as they do). Erosion rates would drop below uplift rates in the upper reaches that have lost precipitation but not steepened yet, and adjacent basins that have not lost precipitation would start to capture area off the basins that experienced aridification.

. . . this is basically what we observe, including the start of a tendency for wetter catchments to capture dry catchments (the area of the Quaternary captures). We were very tempted to include a paragraph on the future evolution of the drainage, precipitations, and erosion, based on the current dynamics, but this would make the paper unnecessarily longer.

If I were testing the hypothesis that aridification resulted in a slowing of erosion rates, I would also ask the questions: 1) Can the spatial variations in erosion rates be explained by drainage basin dynamics alone? 2) Is the whole region that experienced aridification now experiencing area loss?

1: considering that both processes act in concert in reducing erosion rates, assessing their contribution may at best highlight the importance of one over the other, but certainly not disprove the contribution of one of them. Besides, higher precipitations would easily lead to higher erosion rates, and a faster topographic decay of the older range, despite low river incision rates due to drainage dynamics. Besides, incision and hillslope erosion have also stalled also along the southern, dry side of the old range, were no change in drainage dynamics is observed. We expose these facts more clearly in the discussion, after presenting evidence for both climate and tectonic controls.

2: drainage migration from the wetter to the drier is observed between the Chixóy (dry) and Panima (wet) basins. Still, some tectonic forcing contributes to this dynamics (Brocard et al., 2012). On the main range drainage divide, migration is not as simplistic as a SPL-equilibrium-blitz response predicts: drainage dynamics and climate did not trigger any substantial migration of the drainage divide. The presence of knickpoints, dissecting the middle Miocene surface, clearly shows that the pulses of incision generated over the past 12 My have not yet been felt at the divide.

Replies to the specific comments:

The section on knickpoint analysis could be made more concise. I don't think you use any knickpoints that aren't migrating knickpoints, so you can just say you eliminated other knickpoints using image analysis. I think you can get rid of the section explaining all the different types of knickpoints. Are there knickpoints in multiple basins that you can trace to a single origin? If so you could use this as evidence for a change in K in basins that experienced aridification. Do the knickpoints you claim are stalled in SC cluster at the same chi value (and hence, have a single downstream origin)?

The use of such approach is already presented when explaining how the selection was done. (considering that only 14 % of the knickpoints are most likely migrating, it is important to explain how the screening was done). As explained in the text, the ranges here are too complex to track knickpoints to a single origin. We have been able to do this more reliably elsewhere, for example, on in Puerto Rico (clustering of elevations), but even there, simple variations in bedrock erodibility generate a broad scatter in chi-values among knickpoints tied to a single origin.

How were erosion rates for nested basins dealt with? Was a mixing model used to calculate the erosion rates from the additional area added with each larger area? If not, I think it would be a good idea to do this. Also, clarify why nested basins were chosen and what hypotheses they were used to test. Mixing models are cool but are overkill here. They are not needed to show that the older range erodes more slowly than the new one.

3.3 – A "hillslope erosion index" is presented but then stream discharge and stream gradient are used, suggesting channel networks not hillslopes are actually being analysed. Correct. We corrected stream to slope. The EI is meant there to capture overland flow on hillslopes.

Throughout: Ma should be used for “millions of years ago” and Myr should be used for millions of years. See Aubry et al (2009) Terminology of geological time: Establishment of a community standard We are aware of this rule, which varies from a journal to the next. We will follow ESurf’s standard.

Line comments 169 fig 4 not fig 3 . Modified. 398 and with uniform uplift, otherwise K matters . Uniform uplift added 654 I do not see a correlation, nor are correlation measurements presented for either b or c. We added a correlation coefficient. 711 theory is off here, see general comments. You assume that the range gets dryer whereas it gets wetter there. 774-776 how does uplift lead to a slowing of incision rates upstream? As per the very theory of dynamic equilibrium and drainage dynamics you defend so much.

801 Not intuitive. Why doesn’t it reflect the magnitude in change of forcing? Clarify. This is explained in the mentioned references.

807-808 I’m not familiar with Whittaker and Boulton, 2012, but stream power theory states celerity only depends on uplift rates if n is not unity (Whipple, 2001). . . . Only in this case, yes. It’s not because you love the SPL that the real word is offered that only possibility.

853 what? Knickpoints are areas where the rivers are anomalously steep so how can you say they are caused by river steepening? We cite the proper reference here, see the modelling results of Nicole Gasparini.

873 what does it mean for a knickpoint to have an amplitude? Knickpoints are often described as waves of erosion, with a celerity. We replaced it by height

937-940 citation? They are the same as two lines before, we repeated them for clarity.

Figure comments Fig 1 DEM resolution not great, also figure shows elevation not relief so title is confusing. This DEM resolution was chosen because on higher resolution DEM, the shading prevents seeing overlays clearly. Fig 2/3 recommend using the same colors for the paleosurface throughout Color adjusted Fig 5 make legend transparent also so colors match We improved the color match.

Fig 8 I find this figure very overwhelming. If you keep it, the legend needs reworking or it at least needs to be more clear that the numbers are knickpoints. Leaving off lithology would help. We transferred text from the caption to the legend to make it easier to read.

Fig 9 Do you say how many incision measurements you have? If so I missed it. Could make the boxes proper box plots rather than just showing range. These are not box plots.

Fig 10 This figure is confusing. Would be more helpful to see rates on a map. Fig 11 Clarify what this figure shows. Fig. 10 is a neutral figure in the result sections. It provides rates values that could not be read on a map. Fig11: We clarified the caption.

Fig 13 How were the 10Be rates normalized? Recommend one map showing erosion rates and put the map of the erosion index in the supplement. Add main divides. The 10Be rates are not normalized. We understand you don’t like EI, but the map nicely illustrates the predicted distribution of erosion rates through the study area, at the same scale as that of the introductory figures, which is informative for people interested in the area. This map is also a support to the cross sections of figure 16.

Replies to anonymous referee #2

We thank anonymous referee #2 for a careful review of the paper and we expose here which changes were implemented

Replies to the general comments:

The authors present numerous alternatives to the main conclusions, but in a manner that seriously distracts from the presentation of the meaning of the results. 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. 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. 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.

We simplified the presentation of the study area (section 2), transferring some of its material to the discussion section, and also simplified the presentation of methods (section 3) and results presentation (section 4). We reorganized the discussion (section 5), into three subsections, one presenting links between climate and erosion (5.1), the second one the effects of drainage dynamics (5.2), and then a section that discusses their combined effects on the current topographic evolution of the old range that are of most general significance (5.3: slowing down of headward-migrating knickpoints, pediment formation). We end up with a subsection and the implications for the drying down of mountain ranges and plateau formation.

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. 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.

We added a map that clarifies the regional context, although we feel that it is more of regional significance here. Indeed, we have tried to avoid as much as possible providing information that distracts the reader from the two-ranges story, which is the focus of this paper. We have presented the intricacies of the regional, tectonic context in other papers.

Specific comments We used the specific comments to modify the text, through the thorough rewriting of various sections.

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