

Interactive comment on “Geomorphic analysis of transient landscapes from the Sierra Madre de Chiapas and Maya Mountains (northern Central America): implications for the North American–Caribbean–Cocos plate boundary” by L. Andreani and R. Gloaguen

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Comments from referee are in black.

Reply from authors are in italic.

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Reply to John Armitage

This manuscript uses geomorphological metrics to try to gain farther insight into the tectonics of Central America and in particular the evolution of the Sierra Madre de Chiapas and a fore-arc sliver between the Chiapas Massif and the Cocos plate. Based on the lack of evidence for recent uplift from river long profiles and relative difference in minimum and maximum elevation across the Chiapas Massif, the authors suggest that the region bounded by the fore-arc sliver and the North American Plate is not under compression. Maximum compression is to the south-east of this region. These observations/interpretations allow for a greater understanding of this complex region.

I think that this manuscript demonstrates the usefulness of combining geomorphological metrics with interpretations of the regional tectonics in discriminating between models of regional tectonic evolution. The manuscript is however very long, and many sections are a voyage of discovery for the reader. While this is not an issue with the science, I would question if the average reader would have the patience to plough through the introductions to various sections to get to the science. Therefore, I would suggest that the major improvement would be a thorough edit of the text to better state what the point is behind the study and more concisely present the competing models for the evolution of the triple-junction.

- *We understand that some methodological introductions are superfluous to the specialist, but like Referee 1, a few of our readers are either not familiar with the methodological framework or want to replicate the analyses and require more precisions. Nonetheless, according to your comments, we have strengthened the structure and reduced the volume of details.*

In terms of the science, I would suggest that perhaps all the slight changes in gradient within each river profile should not be assumed to be evidence of a river adjusting to a

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base level fall or uplift within the catchment. It is mentioned in the text that knickpoints can be due to lithology, yet this point is never explored within the context of this study. Can every knickpoint realistically be a perfect recorder of every phase of uplift? Furthermore, does the analysis here allow for spatially varying uplift? And, finally, there is no mention of climate in the manuscript, and I would assume differential erosion from east to west is possibly a consequence of orographic effects on precipitation.

- *We agree on the fact that knickzones cannot be perfect recorders of uplift and that all slight changes might not be an evidence of base level fall or uplift. That the reason why we always try to use a statistical approach. You are entirely right, a single singularity in the landscape can not directly linked to a single process. But if the large majority of a significant sample of data converge we can infer or at least discuss specific processes.*

The question of the effects of lithology and climate is pertinent since both may affect the development of a landscape and in turn tectonics also has a feed-back effect as it creates lithological contacts and topographic uplift creates barriers for precipitations. This is not the purpose of this manuscript to identify single processes.

On the other hand, if landscape patterns can be observed on large areas, where very variable climatic conditions occur and where the lithological signal seem dominated by other processes we have to discard both climatic and lithological controls. Nonetheless, we have modified the manuscript in order to provide a discussion of the potential effects of lithology and climate variations on the different indices. To tackle these problems we used a combination of tools (topographic profiles, morphometric maps and river longitudinal profiles). The interpretation of river profiles is greatly improved by the use of morphometric maps and topographic profiles. We can map the distribution of low-relief and elevated relict landscapes and erosional waves following an uplift of base-level fall and we can associate segments of the river to these two domains.

A technical point I would also like to make is that the use of the so-called chi-plots of Perron & Royden, Earth Surface Processes and Landforms (2012) rather than the more

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simple slope-area log-log plots here would have reduced the scatter and uncertainty in the values of “k” that are calculated.

- *You are right, chi-plots are powerful tools. We have implemented this technique in our last version of TecDEM. On the other hand, the interpretation of chi-plots can be challenging and we did not want to add one more technique if that could be avoided. The purpose of our stream profile analysis was primarily to discriminate between the upper reach associated to relict landscapes and the lower part of the profile which adjust to the base level change and is usually steeper. Both Chi-plots and slope-area allows to observe changes in the gradient of the river related to the transient stage of a river profile. Basically, a knickpoint is associated to a change in ksn values and to a change in the gradient of the profile in a Chi-space. Our main focus was to map such changes and for this purpose slope-area analyses performed well. We do not feel that our analysis was hindered by the scatter in the slope-area plots.*

Over all this is a worthwhile contribution, but I think it should only be published after some revision.

Minor point: In English evidence is not made into a plural as it is apparently uncountable. Change all “evidences” to “evidence”.

- *We corrected all occurrences of ‘evidences’ accordingly.*

Detailed comments as I found them in the text:

Page, 942, line 5: “we try to systemize”, I do not understand what is meant by systemize.

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- *We wanted to characterize and map the landscapes in order to better understand them. We modified this sentence for more clarity: “We intend to characterize and understand the complex tectonic setting that produced an intricate pattern of landscapes using tectonic geomorphology and available geological and geophysical data.”*

Introduction: I found it difficult to understand what the point of the study was. I understand the aim, as it is clearly stated, however I don't know why this is being done. I think my lack of understanding comes down to a second paragraph that is heavy in detail and takes 25 lines to get to the point: the lack of consensus on how the triple junction links to the Cocos plate. I think the concept of the “fore-arc sliver” and the implication of this slice of crust (?) should be the subject of the second paragraph (or of a 3rd dedicated paragraph) so that it is clearer that this is the problem that you wish to solve. (At least this is my understanding.)

- *We reworked the introduction in order to come more rapidly to the main problem. We start with a short paragraph to introduce the plate boundary and the next paragraph is dedicated to the complexity of the plate boundary and why we focus on the Sierra Madre de Chiapas and Maya Mountains. We made a third paragraph that explain our approach. We also avoided to give too much details.*

Page 943, line 24: “does not connects” change to “does not connect”, and delete “Indeed”.

- *Corrected.*

Page 944, lines 3-6: These sentences should be more prominent, as this is the point behind the study.

- *We hope this is the case in the new version of the introduction.*

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Page 945, line 20: Change “have” to “has”.

- *Corrected.*

Are sections 2.1, 2.2 and 2.3 really necessary? Could you not just get straight to the point?

- *We reworked the section 2. We moved the section 2.4 (Models of the triple junction) at the beginning. We reduced the length of section 2.1 (now 2.2) by 50%. Sections 2.2 and 2.3 (now 2.3 and 2.4) were kept as they were since they provide a description of the studied areas that will be needed afterward.*

Section 2.4: Better use of figure 4 could be made by referring to it at the beginning of each of the paragraphs as each model for the triple junction is introduced.

- *We agree with this point. We added a reference to Fig. 4 at the beginning of each sections.*

Page 948, line 17: Is there a diagram for this “simple transform-trench boundary” or is it not included in Figure 4?

- *Earliest models of the triple-junction are not shown in Fig. 4 because these models are very simple to understand (either the Polochic or Motagua fault connects to the trench). We wanted to focus on the more recent models.*

Page 948, line 25: I would replace “Indeed”, with because.

- *Corrected.*

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Page 949, line 8: What is a “fault jog”?

- *In fact the reverse faults act as a restraining bend between two left-lateral fault systems. The term 'fault jog' was introduced by Guzmán-Speziale and Meneses-Rocha (2000) to describe their model. We replaced 'fault jog' by 'restraining bend' for more clarity.*

Page 949, lines 12 to 25: I have to read 13 lines of text before I get a reference to the figure! I strongly suggest that the text is restructured so that the model is first stated and then described.

- *We added a link to each sub-figure at the beginning of each section so the reader can more easily figure out which model is described.*

Page 949, line 27: Add “The” before “Latest models...”

- *Corrected.*

Page 950, line 2: Add a reference to Figure 4c.

- *Reference added.*

Page 950, line 28: Change “dynamic” to “dynamics”.

- *Corrected.*

Page 951, lines 4 to 5: How does the extraction of topography differ from a standard swath profile? I found this sentence confusing.

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- *In this sentence we compared swath profiles to traditional ones. We meant that in swath profiles we use a series of parallel profiles that are then projected on a single plane, while in standard topographic profiles a single line is used. We modified the sentence for more clarity: 'Topography is extracted from a rectangular swath using a series of parallel profiles, rather than using a single line, as in conventional topographic profiles.'*

Page 951, lines 6 to 7: It is written “usually the maximum, minimum...” In this manuscript you are definitely using these metrics: delete “usually”.

- *We removed 'usually' as suggested.*

Page 951, lines 7 to 8: Change “maximum elevations” to “maximum elevation”

- *Corrected.*

Page 952, line 15: I don't understand what is meant by “to evidence”. I don't think “evidence” is a verb.

- *Corrected. We replaced 'to evidence' by 'to highlight'.*

Section 3.4: The steepness index is based on a theory. Uplift is calculated based on the stream power law which is a heuristic empirical piece of mathematics. With this in mind, the steepness index can be used to propose patterns of uplift, but it does not “show a direct proportionality with uplift rates”. Furthermore, why not use the integral approach proposed by Perron and Royden, *Earth Surface Processes and Landforms*, 2012. This greatly reduces the scatter in the slope-area analysis.

- *The reviewer is right. We modified the sentence: 'The normalized steepness index (ksn) is widely used to investigate tectonically-induced perturbations in river longitudinal profiles and has been used to propose patterns of uplift (Kirby and Whipple, 2001; Wobus et al., 2006; Whittaker et al., 2008).' See answer in general comments for Chi-plots.*

Page 955, lines 3 to 14: A definition of a “relict landscape” is needed.

- *We added a definition as suggested: 'Landscapes tend towards an equilibrium in which rivers are graded to sea level or local base-level. Tectonic or climatically-induced base-level falls modify the equilibrium of the drainage. The result is an erosion wave propagating upstream and the areas not yet affected by the erosion wave form an upper-relict landscape (e.g., Clark et al., 2005; Reinhart et al., 2007; Pérez-Peña et al., 2015).'*

Page 957: Could the assumption that the maximum in minimum elevation in the swath profiles marks the drainage divide be confirmed by mapping the catchments?

- *We mapped the catchments on both sides of the Chiapas Massif (see for instance Fig. 9 and 11) and this peak in minimum elevations marks indeed the drainage divide.*

Page 957, line 22: Add a reference to figure 5 here.

- *We added the reference to Fig. 5 as suggested.*

Page 958, lines 3 to 5: Why does “the general topographic trend as well as the asymmetry of the drainage” indicate a “tilt towards the NE”? Furthermore, is this a tilting of the landscape due to tectonics (or something else?), or a gradual present day slope in the landscape? What about an orographic effect that creates differential erosion on the two slopes of the cross section?

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- *The elevation of the Sierra de los Cuchumatanes decreases towards the North. Indeed, the elevated plateau which forms the highest part of the range is located along the Polochic fault (which marks the southern boundary of the range). The elevation of the Mayan paleosurface preserved on top of the range also decreases towards the North (Authemayou et al., 2011). The most likely interpretation is that the range has been uplifted and slightly tilted towards the North (differential uplift). Whether the asymmetry of the drainage is related to the tilt of the range or to an orographic effect is a question difficult to answer. Perhaps it is a combination of both factors.*

We modified the sentence as follows: 'The general topographic trend with higher elevations along the Polochic fault suggests a tilt towards the NE. The asymmetry of the drainage is possibly related to the tilt of the range or to an orographic effect (or to a combination of both factors).'

Page 959, line 2 and line 10: From line two I got the impression that the hypsometric integral values are controlled by tectonics everywhere, yet on line 10 this control is apparently “also obvious in some other” regions. I am confused.

- *We simply wanted to say that the structural control was obvious not only in Chiapas but also in others regions such as the Yucatan peninsula and areas along the Polochic fault. We modified the text to clarify this.*

Page 961: How gentle does a knickpoint have to be before it is ignored?

- *We are not sure about the meaning of this question. There is no mention in the manuscript that knickpoints were 'ignored'. On the other hand, we only discussed knickpoints that are significant (either in their intensity or in the statistic similarity in terms of altitudes). We identified knickzones based on changes in the gradient of river profiles and provided that these changes were not related to artifacts (dams, dem filling, etc.). For the description we used the terms of 'prominents' for knickzones where the gradient change is*

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abrupt (for instance profile 24 in Fig. 10) and 'gentle' when the gradient change is less pronounced (for instance profile 15 in Fig. 10).

Page 961, line 10: Change "The two third..." to "Two thirds..."

- *Corrected.*

Page 961, lines 22 to 23: Could the two knickpoints not be due to two distributions of Uplift?

- *We are open to any alternative explanations but at this stage we rather think that this area marks a change in the uplift pattern. We use the Occam's razor approach: "Among competing hypotheses, the one with the fewest assumptions should be selected"*

Page 962, lines 25 to 28: Could you elaborate on how the effects of dams were removed from the river long profiles? How much did the method used influence the inferred uplift history?

- *We did not removed the effects of the dam but we rather attempted to infer the geometry of the Grijalva river profile using the segments upstream and downstream of the flooded area. For instance the segments of the river upstream and downstream of La Angostura dammed lake could be fit by the same regression in a logarithmic plot of slope vs area. We are thus confident about the fact that no knickpoints are hidden below the lake (otherwise we would have a change in the gradient of the river). By contrast the gradient of the river upstream and downstream of the Chicoasen dammed lake appear different and could not be fit by the same regression. We thus think that the dammed lake hides a flooded knickzone. For the Malpaso and Peñitas dam the gradient of the river became too low and the scatter in slope-area data too high to make such test. The overall interpretation of*

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the lower part of the Grijalva river is consistent with what we observed in La Venta river: a main knickzone with marks the limit between an elevated landscape where rivers are graded and a lower area associated to deeply entrenched valleys and canyons.

Figure 9: I notice that there is an association with drainage length and the steepness index and the inferred base level change: Longer catchments are associated with higher values for the steepness index and base level change. Could the shorter catchments just not be sampling, if that is the right word, the uplift within the Chiapas Massif as they do not extend far enough into the range? Also is there really no lithological change from west to east?

- *You are right, there is a relationship between catchment size and steepness. But we do not look at absolute intensities but at relative changes. The areas are not identified by a specific value but by a significantly different values than their surroundings. The lithology of the Chiapas Massif remains homogenous along strike. The lithology consists mainly in a Permian batholith (granites and granodiorites). There are also more recent (Miocene) granodioritic intrusions distributed along the Tonalá Shear Zone. The contact between the Permian batholith and the Miocene intrusives is barely visible in the topography and we observe no changes in rivers gradients along the contact. The observed knickpoints are located upstream of the contact.*

Over all, I found this manuscript far too long. There is good science hidden within a lot of description and introduction. I know the primary purpose of a review is to critique the scientific content, however this paper would also be greatly improved if the introduction was significantly condensed.

- *We thank the reviewer for its overall appreciation of our manuscript and for providing valuable comments and suggestions. We hope that the changes we made in the introduction and in section 2 improved the fluidity of our manuscript.*

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Please also note the supplement to this comment:
<http://www.earth-surf-dynam-discuss.net/3/C463/2015/esurfd-3-C463-2015-supplement.zip>

Interactive comment on Earth Surf. Dynam. Discuss., 3, 941, 2015.

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