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Earth Surface Dynamics Discussions

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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Review of "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

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.

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 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.

A technical point I would also like to make is that the use of the so-called chi-plots of Perron & Perron, Earth Surface Processes and Landforms (2012) rather than the more simple slope-area loglog plots here would have reduced the scatter and uncertainty in

the values of “k” that are calculated.

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”.

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.

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.)

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

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

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

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

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.

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

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

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

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.

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

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

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

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

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

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

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

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.

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

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?

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

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?

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.

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

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

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

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?

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?

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.

I hope these comments help,

John Armitage, Institut de Physique du Globe de Paris.

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