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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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Comments from referee are in black.

Reply from authors are in italic.

Reply to Anonymous Referee #1

1 General comments

This paper presents an interesting study that uses geomorphic indexes to classify the landscape of different regions in order to unravel its tectonic history. The analysis focuses in the North America – Caribbean – Cocos plate boundary, in the Sierra *[note from authors: sentence was cut in the attached .pdf file]*

The geomorphic analysis is quite detailed and the authors do a good job by interpreting the extracted geomorphic indicators in terms of landscape tectonic evolution and relate them to different tectonic events. Some of the conclusions of this paper are new and quite interesting to improve the knowledge about this plate boundary. Authors define different landscape stages with the aid of geomorphic indexes. One of the main findings is the interpretation of the evolution of the Maya Mountains in the context of the plate boundary (although a proposal, it is very interesting). I think that this paper is suitable for publication if some aspects are improved.

- *We are grateful for the constructive comments and the appreciation of our work. We took into account and we implemented all the suggestions made by the reviewer.*

The introduction in some parts is a bit vague and it could be improved by highlighting one of the key points of this study; the interpretation of Maya Mountains in the context of the plate boundary. Authors stated in the introduction that MM is one of the key areas in this plate boundary but do not explain why. Until the 2.3 section readers don't know that this region has been poorly studied respect to other areas of the North American–Caribbean plate boundary. Moreover, MM is out of all the revisited tectonic models, proving that this region has been obviated. Authors should address these problems in the introduction as one of the objectives of this paper.

- *The introduction has been modified accordingly. We reworked the introduction using comments of both reviewers. In the new version we added new sentences to specify the interest of the Maya Mountains.*

Some of the references for methodological sections are not up to date. Authors should include some relevant references missing in their study (I proposed some recent works, see detailed comments).

- *We included some more recent references. The bibliography was updated using references proposed by the reviewer. On the other hand we focussed on the seminal works.*

The authors use in the paper "isobase maps" but they are not described in the methodological section.

- *We reworked the methodological section based on the suggestions of the reviewer. We provide a more detailed description of surface roughness and isobase maps.*

2 Specific comments

The bibliography in section 3.1 (swath profiles) is scarce. Swath profiles have been used intensively in tectonic geomorphology. Reinforce this methodology section by adding some recent and relevant references.

- *The reviewer is right. We forgot to provide references regarding this method. We added citations to the following works which explain/use swath topographic profiles: Isacks (1992), Masek et al. (1994), Telbisz et al. (2013), Hergarten et al. (2014).*

HI has demonstrated to be a very useful tool to analyze landscape dissection. There are a lot of works that use HI values to classify landscape forms but only few that do such classification spatially. Your results also reinforce the usefulness of this methodological approach to evaluate spatially tectonic activity. You could add some references to this kind of analysis (e.g. you could add some of these references in this section: e.g. Mahmood and Gloaguen, 2011; Siddiqui and Soldati, 2014; Andreani et al. 2014).

- *We thank the reviewer for his comment. We added the suggested references to section 3.2. We also added Pérez-Peña et al. (2009) in the list.*

There are many kind of surface roughness parameters in earth sciences (see Smith 2014), and some of them do not necessarily have to indicate high landscape dissection but only an irregular surface. There are many different formulations for surface roughness, and probably for clarity you should describe a bit more the selected approach for this paper. Maybe a synthetic figure where HI, SR and RA are explained would help readers.

- *We agree with this comment. This index lacks a proper description. The method used in TecDEM (Shahzad and Gloaguen, 2011) is adapted from the GRASS algorithm of Grohman (2004). We rewrote the section corresponding to the description of surface roughness in the manuscript in order to provide details on the method we used as well as adequate references. We maintain that a new figure is not necessary. Our manuscript already contains a consequent numbers of figures. More importantly, HI, SR and RA are not new algorithms and we use methods that are already extensively described in the literature.*

The last two area-slope plots of the Figure 10 (profiles 21 and 24) present a problem. I guess that in each regressed segment the two black lines should represent ks and ksn.

As ksn index is defined with a fixed reference concavity of 0.45, all ksn lines should have the same gradient in the different segments. This is not true in the presented plots; segments 1-2-3 in profile 21 and 1-2 in profile 24 have very different gradients so I think that authors did not present ksn but only ks and other kind of regression index.

- *In fact the two black lines show the envelope of the 500 regressions used to estimate the ksn values (this is mentioned in the Fig. 10 caption). As mentioned in section 3.4 we used a bootstrapping approach to address the uncertainties related to DEM artifacts. We performed a series of regressions on subsets containing 75% of the points (randomly selected). We then used these regressions to compute ksn values which are displayed in the central plots. To summarize the lower plots shows the regressions which give ks and θ while the central plots show the ksn values computed using ks, θ, and θref= 0.45.*

3 Minor comments

P 942 - Line 5. Although the used DEM has been extracted by radar, I'm not sure if the term "remote sensing tectonic geomorphology" is suitable for this paper. I would remove "remote sensing".

- *We agree with this point. We removed 'remote sensing' from the abstract.*

P 953 – Line 12. D8 algorithm was first defined by O'Callaghan and Mark (1984), include this reference.

- *We added this reference as suggested by the reviewer.*

P 967 – Line 15. Sort references by date (check this throughout the paper). In this point your last reference is 13 years old (2002), you could include some newer references

about this interesting topic (there are a lot of works in the recent literature). The last work of Ferrater et al. 2015 is a good example of how a change in the tectonic setting can produce complex landscapes where relict landscapes can remain in the upper parts of big drainage systems.

- *We checked the references and sorted them by date. We also added Ferrater et al. (2015) to the reference list. In fact, we already provide recent references about relict landscapes (Legrain et al., 2014; Giletycz et al., 2015), though they come at the end of the paragraph (p967, lines 20-21).*

P 967 – Line 17. You could reinforce your discussion by adding references to other places where river base-level fall produces headward erosion waves that propagate upstream and leave relict landscapes (Reinhart et al., 2007; Pérez-Peña et al,. 2015). At this respect the use of the term "erosion wave" could be better than "erosion front".

- *We agree with this comment. We replaced 'Recent tectonic or climatically-induced base-level fall are associated to a propagating front of river incision' by Recent tectonic or climatically-induced base-level fall are associated to the propagation of an erosion wave'. We also added the references to Reinhart et al. (2007) and Pérez-Peña et al. (2015).*

P 969 – Line 2. Change "extremely flat" by "almost flat". Something cannot be flatter than flat.

- *We replaced "extremely flat" by "almost flat" as suggested.*

P 972 – Line 13. You could include the more recent work of Kirby and Whipple (2012) here.

- *We included the citation to Kirby and Whipple (2012) as suggested.*

Figures 11 and 12. These two figures should include “see figure 10 for plot description”.

- *We modified the two figures captions accordingly.*

Figure 17. This figure is the core of the paper and it should be self-explicative. Authors should include in caption all the abbreviations used in the figure, avoiding readers to look for them through the paper.

- *We followed this suggestion. Abbreviations are now included in the caption of Fig. 17.*

4 References

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Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/3/C476/2015/esurfd-3-C476-2015-supplement.zip

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