Solid Earth Discuss.,
https://doi.org/10.5194/se-2018-124-RC2, 2019

Solid Earth
Discussions
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Interactive comment on "Relative Timing of Uplift along the Zagros Mountain Front Flexure Constrained by Geomorphic Indices and Landscape Modelling, Kurdistan Region of Iraq" *by* Mjahid Zebari et al.

Anonymous Referee #2

Received and published: 8 February 2019

Comments to the manuscript entitled "Relative Timing of Uplift along the Zagros Mountain Front Flexure Constrained by Geomorphic Indices and Landscape Modelling, Kurdistan Region of Iraq" by Zebari et al. (doi:10.5194/se-2018-124).

The authors of this manuscript try to constrain the relative timing of uplift of three anticlinal folds of the Iraqi Zagros Mts., combining the results of landscape evolution models and geomorphic indices. The topic fits the ones of the journal and the manuscript has the potential to be interesting for the international scientific community. Nonetheless, some general comments and minor specific ones are listed below, suggesting that some important revisions are needed before publication.

general comments 1) Considering the deformation style of the Folded Zone of the Zagros Mts. chain, the assumption of constant rock uplift seems too simplistic. Doesn't the evidence of NW-ward propagation of the Harir Anticline (used by the authors for supporting the scenario of independent and diachronist uplift in different fold segments) affect the assumption of homogeneous and constant uplift rate in each structure? In this frame, the hypsometric analysis performed for the entire anticlinal ridges seems to have no sense, while I would suggest to use an approach to the hypsometric analysis such as the one proposed by Pérez-Peña et al. (2009). 2) Also the use of the only equations for fluvial erosion and diffusion processes for landscape evolution modelling may be too simplistic. Besides the justifications provided by the authors in section 5.2, it sounds not realistic that sedimentation on the slopes of anticlines can be neglected over the time-span of landscape evolution modelling (105 years), as well as the assumption of constant erosion rates (and climate!). 3) The authors justify the choice of the present topography of the Harir Anticline as LEM input asserting that in this structure the evolved drainage network overprinted the pre-existing one. Looking at Fig. 6 it seems that the drainage has a pattern similar to the one described by Ramsey et al. (2008; Basin Research (2008) 20, 23–48, doi: 10.1111/j.1365-2117.2007.00342.x) as evidence of lateral propagation of folds in the Zagros. This implies a diachronic fingerprint in the drainage network which could void the sense of performing the hypsometric analysis for the entire anticlinal ridges. 4) Some of the units stratigraphically above the Cretaceous limestones outcropping on the anticlines' crest are transitional to continental (i.e. the Bakhtiari Fm.), thus likely being affected by lateral variability of thickness. What about the effects on the uplift rate calculation based on thickness? Furthermore, this uplift rate was calculated based on the thickness and elevation of units on the anticline crest, but (again) is it correct to extend such a rate to the entire folds given their lateral growth? 5) There are several repetitions over the manuscript (see "specific comments") 6) Some original data (geological cross sections of Fig. 4) are referred to in the geological setting, while should be better described in the results. 7) In some cases, the interpretations seem not supported by data. For example, the fit between some of the hypsometric curves obtained with LEM and the ones computed for the three analyzed anticlines is not evident in Fig. 10 and the "minimum RMS" invoked by the authors to demonstrate the fit is not quantified. On the other hand, authors provide a quite specific timing for the inferred "delay" in the deformation sequence of the three folds which is based on this "fit" and use it to support the diachronic scenario of fold development. In my opinion such a constrain is weak, if based on the hypsometric analysis. Other doubt interpretations are listed in the specific comments. 8) Section 5.1 doesn't sound necessary 9) In the Discussion new data are presented (i.e. Fig. 13), but it is not explained how they have been obtained, in particular the calculation of the slope/area. Is it obtained using just the drainage network or the whole topography? 10) In the Conclusions authors refer to the three analyzed anticlines as "active folds", while in section 5.2 they state that the youngest unit affected by folding is the Mio-Pliocene Bakhtiari Fm.

specific comments and technical corrections -TITLE: I would suggest to change the title into: "Relative Timing of Uplift along the Zagros Mountain Front Flexure (Kurdistan Region of Iraq): Constrains by Geomorphic Indices and Landscape Evolution Modelling"

- ABSTRACT: PAGE 1, LINE 13: maybe "fold and thrust belt" and not "fault and thrust belt"

- INTRODUCTION: PAGE 2, LINES 16-17: Why "The timing of this activity is expected to differ along-strike"? Any reference or explanation?

- GEOLOGICAL SETTING: PAGE 4, LINE 31: change "river terraces" into "terraced alluvium". PAGE 5, LINES 18-19: I suggest not to refer to new data in the geological setting. Fig.4 should be described (if made with newly surveyed data) in the Results.

- DATA AND METHODS: PAGE 5, LINE 26: the hypsometric curve is not an "index" PAGE 5, LINE 27-29: the definition/meaning of the geomorphic tools is vague and in some cases, not correct (i.e. "The hypsometric curve and the hypsometric integral

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highlight raised and flat surfaces"). PAGE 6, LINES 3-4: in general, the convex vs. concave shape of the hypsometric curve not necessarily reflects the "maturity" of a landscape (in terms of its absolute age), but can also depend on the type and rates of earth surface processes which dominate the landscape evolution (e.g. linear incision vs. hillslope diffusion processes). PAGE 6, LINES 10-11: again, the meaning of HI is not clearly defined. Please, rephrase. PAGE 7, LINE 1: change the order of terms into "Nh, NHI and NSR" PAGE 7, LINE 5: Digital Elevation Models (3.1.5.) are not Geomorphic Indices. This section should become 3.2 PAGE 7, LINE 22: soil creep is mentioned as second main process inputed in LEM. Maybe the authors should refer more generally to hillslope diffusion processes. PAGE 7, LINE 23-24: see general comment 2): it sounds strange that over the time-span of the modelling the sedimentation on slopes can be neglected. PAGE 8, LINE 8: again, the authors refer to "soil creep" (see comment above). PAGE 8, LINES 17-18: see general comment 3). PAGE 8, LINE 30: authors refer to "time steps" before defining them. PAGE 9, LINE 13: "Kd = 0.001 m2yr-1": why exactly this value?

- RESULTS: PAGE 10, LINE 6: "HI values are maximum at the Greater Zab River": maybe authors mean that HI values are minimum? PAGE 10, LINE 6-12: This part seems not necessary and the authors should pay attention to the meaning of HI when calculated for square areas and not for single basins. In this case HI measures how rapidly elevation changes and not strictly the amount of incision. PAGE 10, LINE 13-18: results concerning roughness analysis are quite obvious...is it really necessary? PAGE 10, LINE 26-34: this part should be moved to the methodological section. PAGE 11, LINES 11-12: authors state that "In the landscape modelling, various simulations with different parameters and time spans were performed", nonetheless they do not provide details on the simulation (neither in the supplementary material). How did they select the best outputs? PAGE 11, LINES 13-14: authors state that "The evolving drainage system overprints the pre-existing one in the input and gradually becomes more deeply incised from the anticline flanks curving toward its core (Fig. 9)". This is not evident in Fig. 9, according to what already explained in the general comment 3). PAGE 11, LINES 15: change “plain” into “flat”. PAGE 11, LINES 20-22: see general comment 7).

- DISCUSSION: Is section 5.1 necessary? PAGE 12, LINES 14-15: what the authors mean with “The maturity level along these anticlines therefore represents the level when these carbonates cropped out in their latest stage”? PAGE 12, LINES 17-19: authors state that “A landscape survives when its uplift is not completely counterbalanced by erosion (Andreani and Gloaguen, 2016; Burbank and Anderson, 2012; Pérez-Peña et al., 2015)”: it does not sound…maybe authors refer to relict landscapes? PAGE 12, LINES 21-22: the sentence “The locations dissected by rivers show high surface roughness” seems obvious and not necessary. PAGE 12, LINES 29-31: “The same effect is visible in swath topographic profiles (Figs. 12c and 12d): in Harir Anticline, there is a clear topographic step with a higher slope angle, while in Akre Anticline the slope is gentler and more linear”: to outline this evidence swath profiles are not necessary…if they can provide further evidence, the latter should be discussed. PAGES 12-13: “This can be interpreted with one of these premises: either both anticlines started to uplift successively (first Akre, then Perat, and finally Harir), or all of them started at the same time but with different uplift and exhumation rates (Akre the fastest, Harir the slowest)”. This concept is repeated too many times over the manuscript. Furthermore, to justify the different geomorphic stage of the three folds with different uplift rates, shouldn’t the latter be “fastest” in Harir and “slowest” in Akre?? PAGE 13, LINE 13: How much does the assumption of constant rock uplift affect the results obtained? Since it is a “strong” assumption you should give an estimation of that. PAGE 13, LINE 15-26: This part of the discussion is not so clear. âŹč E.g. how did the authors perform the slope/area analysis? âŹč Some statements seem wrong: e.g. “In the Akre Anticline, this relationship [slope/area] is negative (Fig. 13b), which means that the streams have a concave shape and the segments with steeper slopes have migrated toward the core of the anticline. This implies that tectonic activity in the Harir Anticline is younger than in the Akre Anticline. Therefore, the premise of having Harir Anticline starting its uplift later than Akre Anticline is most likely”. Why a higher uplift rate in the Harir couldn’t have caused the same effect? âŹč “Since the Upper Cretaceous carbonates in Harir Anticline were exposed later than in Akre Anticline, a landscape evolution model is a viable approach to estimate the exposure time difference. Here the model is built for the first premise of different onsets of uplift. Even if the second premise of different uplift rates is correct, the estimated time difference of the carbonate exposure will only be 28% less than that for the first scenario. As described in section 4.2, the calculated uplift time difference between Akre and Harir anticline is 200±20 kyr, and if the second scenario is correct, the time difference of the carbonate exposure would be 144±14.4 kyr" This sentences are confused and the interpretation is not clear and a bit circular (choice of scenario based on modelling, based on constant uplift rates…). PAGE 13, LINE 27-34: see general comment 2). PAGE 14, LINE 8: The variations in stratigraphic thickness in between the anticlines is constrained by field data? And how does this variability affect the calculations of uplift rates? PAGE 14, LINE 13-26: Is this part really necessary and functional?

- CONCLUSIONS: See general comment 10

FIGURES: FIG.1: i) I suggest to change "transform fault" into "strike-slip fault" in legend; ii) I suggest to add a legend for the different colors in transparency (e.g. the rose one corresponds to 2 zones…); check the use (or not) of parenthesis in the naming of zones; FIG.2: i) I suggest to draw the border between different units in map; ii) check the numbering of the figures recalled in the sketch (they do not correspond to the figure recalled) FIG.4: move and comment it in the Results. FIG.5: symbols for wind gap and limit of Cretaceous limestone outcrop are not visible in figure FIG.6: Symbols for the strike-slip component of displacement are missing FIG.7: i) I suggest to prepare a new figure after having used the approach by Pérez-Peña et al (2009) to the hypsometric analysis. FIGs.8, 9, 10: to be revised according to the comments on the analytical procedures.

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