Review of 'Geomorphic signature of relief rejuvenation in Sierra Morena (Betic forebulge, Spain): evidence of segmented uplift in a strongly strain-partitioned, tectonic scenario' by Immaculada Expósito et al

Reviewer: Colin P. Stark

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Summary comments

The focus of this paper is part of the "forebulge" area of the Betics and the goal is to find geomorphic responses to ongoing tectonics using geomorphometric methods.

Presentation is to a high standard: the paper is well written, clearly structured, with good graphics, and is thoroughly referenced. The methods employed are a mix of old-school geomorphometry and more modern χ-space analysis, combined with some field observations and GIS processing. It's a solid piece of work.

The hypothesis driving the study is that Neogene-present compression will manifest as a geomorphic response to relative uplift that is localized by strain partitioning and by reactivation of existing structures.

The methods employed, and the underlying philosophy, have a traditional flavour throughout, although the Perron/Royden χ tool is a relatively recent innovation. The key concept espoused by the authors is the old-school idea that landscapes change, and become "rejuvenated" in some sense, because of vertical motions driven by tectonics: signs of such rejuvenation include knickpoint formation and drainage divide motion. The evidence they present is purely morphometric; they argue it confirms their main hypothesis.

Although I am a little uncomfortable with this philosophy and with the classical-style quantitative methods, I strongly encourage publication. This paper represents a substantial body of work that contributes to our knowledge of the tectonic geomorphology of the Betic foreland. While it doesn't make any conceptual advances, it does present new data on this landscape and constraints on its recent evolution. It serves as a good example of the application of geomorphometric tools young and old, and (in my mind) illustrates their limitations.

Specific comments

Abstract

Well written, fairly concise, covering the context, goals, methods, results, and speculations.

1. Introduction

Describes the tectonic geomorphology of the Sierra Morena (as a flexural forebulge) largely in the context of Cenozoic deformation of the Betics. Detailed and apparently comprehensive.

It's here that the authors state their basic hypothesis, which is that "relief rejuvenation" in the SM is the result of Neogene-present faulting and folding. This is perfectly reasonable.

p.2, lines 35, 49, 51, 44: Typos in Cloetingh references, both here and elsewhere in the text.

Fig.1: typo "extracted from the [m] DEM". Component map (c) is in colour, but the hue (green only) carries no real information - it's all in the shade. I recommend simplifying to a grey-scale map instead.

2. Geological setting and regional topographic features

Review of the regional tectonics and topography. I am not familiar with this area or this literature, but this section comes across as thorough and scholarly.

Fig.2: Nice, clear map. All the graphics in this paper are of a very high standard. Advice: the sentence "The altitude base is the 12m…" need only be given in full in the first figure citation; after that, a cross-reference may suffice.

3. Approach and method

This section details the analytical methods used, including geomorphometric indexes such as mountain-front sinuosity, valley floor-to-height ratio, and drainage divide asymmetry. There is an explanation of RS/DEM-based knickpoint mapping and modeling, and χ transformation.

While I am not particularly keen on the old-style tools of geomorphometry (because they are rarely process-based, and are tough to relate to any underlying physics), they are considered to have value. The more modern Perron/Royden tool of χ-space analysis is, however, founded on a process model, and it's the logical choice given the goals of the paper. I found some issues with the explanation of the method though.

p.9, lines 230-231: Lague calls the stream-power law the "Stream-Power Incision Model (SPIM)", which is a good name (for one thing, it's accurate) - so why not stick with that instead of writing "stream-power law (SPL) equation system" (which is a bit misleading).

p.9, line 235: typo "an"

p.9, line 236-241: There are some issues with this explanation of the χ idea. (1) Eqn.3 should use partial derivatives e.g. $\partial z/\partial t$. (2) Uplift rate U is assumed constant with space and time in the integration in eqn.5, so it cannot be allowed to vary with both x and t as given in eqn.3. (3) The exponent n is taken to be $n = 1$, which is standard practice although tough to justify, but the authors associate it with linearization ("linearises the SPL form ($n = 1$)), which is not why it's chosen or how linearization arises. (4) The assumptions involved are not clearly explained.

These shortcomings should be easy to fix. To help, let me be more clear how I see the steps and assumptions here - by writing out the equations again but with some subtle corrections that make explicit those assumptions. Taking the SPIM for the bedrock incision rate in channels in a region subject to a vertical rock uplift rate $U(x, t)$ in the frame whose elevation above some datum is given by $z(x, t)$, the rate of channel elevation change is given by:

$$\frac{\partial z(x,t)}{\partial t} = U(x,t) - KA^m(x)\left|\frac{\partial z(x,t)}{\partial x}\right|^n \tag{1}$$

where erodibility K is assumed to be constant, and the upstream area at each point x is constant with time $A(x)$. In the literature (e.g. Fox et al, 2014), the exponent n is generally assumed to be $n = 1$ for simplicity (and for no other reason really!), while the exponent m is observed to be around $1/3$ to $1/2$ but can be inferred from each data set. In reality, the ratio m/n is inferred - which is what the authors do in section 6. Now, if (a big if) we assume steady-state balance between vertical tectonic uplift and erosion (notice we have assumed not horizontal tectonic deformation, which runs counter to the overall tectonic context of the Sierra Morena), we can write (notice the partials become full)

$$KA^m(x)\left|\frac{\mathrm{d}z(x)}{\mathrm{d}x}\right|^n = U \tag{2}$$

with U and z considered constant in x: x dependence in U would make integration problematic, while t dependence would mean the equation would remain a partial differential equation and therefore not simply integrable:

$$\frac{\mathrm{d}z(x)}{\mathrm{d}x} = \left(\frac{U}{KA^m(x)}\right)^{1/n} = \left(\frac{U}{KA_0^m}\right)^{1/n}\left(\frac{A(x)}{A_0}\right)^{-m/n} \tag{3}$$

where the sign of the gradient has been ignored. This gives the integration

$$z(x) = \left(\frac{U}{KA_0^m}\right)^{1/n}\int_{x_b}^{x}\left(\frac{A(x')}{A_0}\right)^{-m/n}\mathrm{d}x' \tag{4}$$

which is effectively what's given in eqns.4 and 5. My point here is that U cannot be allowed to vary with x, otherwise it cannot be extracted from the integral. Nor can we legitimately allow U to vary with time, because elevation has been assumed to be time-invariant $z(x)$. What's more, upstream area $A(x)$ does not, as written, vary with time either, which means that catchments areas can't grow or shrink and drainage divides can't move. Finally, erodibility K must be the same everywhere.

These assumptions are not consistent with the very inferences typically made using the χ method, such as a history of changes in uplift rate, formation and propagation of knickpoints, and divide migration. In other words, the targets of χ analysis seem to be phenomena that the χ model precludes.

The logical escape route, at least as I understand it, is to say that the χ transformation provides a kind of null hypothesis (formation of channel profile subject to the strict assumptions above) against which a real profile can be compared. This is perfectly fine, and is the standard thinking (I believe), so I am not criticizing the authors for issues I associate with the method. Rather, I would prefer to see the logic of the χ explained better - either as I have tried to do, or in some other way if my understanding is incorrect. Specifically, would the authors comment on how they reconcile their knowledge of spatial variations on rock type (erodibility) and tectonic deformation with model constancy in K and U?

p.10 line 241: should be x_b not xb

p.10 line 256: capitalize "Gaussian"

p. 10 line 266: "with [the] Topo ordering scheme" - is this the name of the algorithm?

4. Signs of relief rejuvenation in Sierra Morena

Some qualitative geomorphological analysis using a mix of field observations, GIS mapping, and simple remote sensing. Geomorphic indexes and hypsometry are discussed. The work here is solid. I particularly like the observation of incised meanders, which (in my experience) can be a good indicator of local "uplift". It would have been nice to seem more description of this and other features that are mentioned only in passing and illustrated only with a blurred GE image.

A minor comment: here and elsewhere the indexes are sometimes written Vf, Smf, etc, and sometimes as V_f, S_{mf}. I would prefer to see subscripts used throughout.

5. Structures related to relief segmentation and rejuvenation

Structural fabrics. These are useful data to have, and their interpretation looks fine.

6. Knickpoint pattern analysis

An analysis of knickpoints using topographic profiles, both raw and χ-transformed. Careful attention is given to the distorting effect of artificial dams. Observations are made regarding overall convexity or concavity of profiles in each catchment S1-S6: the strong up-convexity of S2 and S6 is much more clear in the χ-transformed profiles.

p.18 table 2: The values of m/n are incorrectly given as negative numbers. Both exponents are strictly positive, so this is probably just a misreading of eqn.5.

p.18 line 427: "S3 and S4 plots are seen to display a concave-up shape" - I don't see this. They look pretty straight to me, at least in the χ-transformed profiles.

Fig. 8: Should be "Tributary inlets into reservoirs". And why are there two different symbols (empty circles and full diamonds) for these?

Fig. 8: I would prefer to see a specific explanation in this figure caption for how I'm supposed to interpret the figures. There is a lot of information here, but the caption provides no guidance as to what it's all supposed to mean.

Fig. 8: It would help if each graph pair were labeled with the name of the catchments to which they belong. I take it S1 = (a), (b); S2 = (c), (d), etc, but the caption is too cryptic and the subfigures themselves have no information. Readers shouldn't have to work to understand such fundamentals: they should "pop" out of the graphics themselves.

p.21 line 457: "show a strong consistency between the model and the location of a robust number of actual knickpoints" - what model? Having read this far, I'm still not clear on what model I should have in mind here.

7. Discussion

Speculation about the structural origins of the geomorphic features observed in the body of the paper, and on the tectonic origins of these structural influences.

p.23 fig 9: Nice plot of χ vs elevation, but it lacks any explanation as to how to interpret it. Explanation is given on p.22 lines 507-517 “the slope of plots can be interpreted as a proxy for [uplift rate]” - I suggest adding it to the caption here.

p.25 line 558: typo: “regards” should be “regard”

References

Typo on line 705