Hydrol. Earth Syst. Sci. Discuss., 7, C2466–C2476, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2466/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial variability in channel and slope morphology within the Ardennes Massif, and its link with tectonics" *by* N. Sougnez and V. Vanacker

Anonymous Referee #1

Received and published: 26 September 2010

Review of Hydrol. Earth Syst. Sci. Discuss., 7, 6981–7006, 2010: Spatial variability in channel and slope morphology within the Ardennes Massif, and its link with tectonics (N. Sougnez and V. Vanacker)

This study analyses several hillslope and channel morphometric variables and investigates their possible linkage to inferred tectonic uplift in the Ardennes, Belgium. This is one of the few studies that explore such linkages in low-relief and low-uplift terrain, and complements a growing body of literature on the dynamic feedbacks between exogenic and endogenic processes. The authors find that only a number of terrain variables

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

show compelling correlations with the amount of Quaternary uplift, and conclude that channel morphometric variables do a better job than parameters of hillslope morphometry. Although not mentioned, this implies that the underlying research hypothesis, i.e. that hillslope morphometry reflects topographic forcing in low-uplift terrain, needs to be rejected.

The manuscript is well written and presented in adequate format. Data are presented in a mostly transparent and graphically appealing manner. Overall, the theme is of sufficient scope to be of interest for this Special Issue of HESS, although I could imagine that more details on the performance of the various terrain variables could be desirable. I have a number of suggestions to make about this manuscript, and recommend the authors have a look and decide which they find useful for revising their manuscript. The data presented here are worth publishing, though I find the discussion and interpretation of results may need some substantial improvements in the sense of major revisions.

GENERAL COMMENTS

âĂć The authors use a number of DEM-derived hillslope and channel metrics to test for any correlation with the amount of Quaternary uplift and to argue for a detectable tectonic imprint on landscape evolution in the Ardennes. However, I understand that the amount (and hence rates) of rock uplift in the area were derived from fluvial topography (i.e. terraces) in the first place (Demoulin and Hallot, 2009). Therefore I am wondering whether the potential of constructing a circular argument has been addressed sufficiently.

âĂć Moreover, the problem of disentangling significant base- (i.e. nearby sea-) level changes throughout the Quaternary renders the interpretation of a tectonic signal in the terrain variables rather difficult. The authors duly address this point in the discussion, though the problem remains. This is mirrored in the somewhat descriptive and not really novel conclusions. The discussion may want to venture a bit beyond and address

HESSD

7, C2466–C2476, 2010

the suitability of the chosen methods to detect tectonic imprints in the landscape of the Ardennes. For example, if one assumes that tectonic forcing indeed is aptly reflected in hillslope and channel morphometry even at low to moderate uplift rates then the corollary would be that tectonics would have a decisive capability of overriding any other (e.g. climatic, lithological, anthropogenic, . . .) forcing of topographic development.

âĂć Statistical correlation (Fig. 2) does not necessarily imply causal linkage, and the authors may want to explore this potential issue more thoroughly in their discussion. I further note that the authors use the amount of uplift rather than the inferred average rates. This calls for some sort of explanation, if not justification.

âĂć The morphometric parameters used in this study should be a bit more clearly defined (perhaps in equation format within a separate table or as an appendix to Table 1). In particular, the authors may want to briefly sketch the relevance of each single parameter as a potential proxy of tectonic forcing. Ideally, the choice of parameters should satisfy the testability of the research hypothesis.

âĂć The use of a K-means cluster analyses on the range of morphometric parameters derived is what distinguishes this study from many others that have checked for a correlation between various hillslope and channel metrics and parameters of tectonic uplift. Hence this particular method and its results deserve some more exposure. The authors assert to have identified three distinct groups of catchments in terms of tectonic signatures, with the overall sample number being n = 10. Hence I am bit worried about the robustness of this distinction, and a comment or two may want to pick this up in the discussion.

âĂć The interpretation of results mingles with their presentation. This makes it difficult to view the data separately from the underlying notion of tectonic forcing, and somewhat biases the study. This may be well due to publishing space constraints, though I would recommend trying to separate results from interpretations as much as possible. This would avoid that the discussion returns to the description of some of the results.

HESSD

7, C2466–C2476, 2010

âĂć The discussion further offers a number of partly conflicting explanations, which should be carefully revisited, as it contains the heart of the matter. For example, it is not clear how tectonically induced knickpoints were identified in the first place. There is also alternating mention of equilibrium and dis-/non-equilibrium states in the landscape of the Ardennes. Given the small number of catchments this notion seems difficult to accept or reject. Also, the prediction potential of the Hypsometric Integral (HI, Fig. 4) seems to be both over- and under-rated. The paragraph on hillslope-channel coupling is interesting, but largely descriptive, while the argument about morphometric indices of topographic rejuvenation is unsupported by the data.

âĂć The conclusions are not really that compelling, and mostly confirm the results of many previous studies. What I missed instead was a statement of whether the initial hypothesis was being upheld or rejected eventually. Also, what is the take-home message from this exercise? Which of the many parameters should one use to decipher tectonic imprints in low-uplift areas? Are these imprints always detectable? Why is it that only channel metrics seem to offer reasonable correlation with uplift measures? Why is it not hillslope metrics even if there are different degrees of coupling with channels? Finally, is correlation an indication of physical feedback indeed?

âĂć The navigation through the study area may be a bit difficult for those not familiar with the Ardennes. There is frequent mention of river names that are not adequately shown in the location figures. This makes it difficult to judge some of the regional trends in the data. Along these lines, Fig. 1a features a number of abbreviations that should be explained in the caption, or at least featured in panel b.

SPECIFIC COMMENTS All remarks refer to page and line numbers in the proof:

6981,7: Delete “wide”.

6981,8: Indices do not have “behavior and strength”. They may have prediction or explanation potential instead.

HESSD

7, C2466–C2476, 2010

6982,9: Insert “Belgium” somewhere here.

6982,18: “Meuse” – This is the first time you mention this river. Keep in mind readers not familiar with the study area: how important is this particular river?

6983,5: “Actually” – Perhaps rephrase to “Nowadays”?

6983,8: “More recently” – Conflicts with the publication dates.

6983,21: You may want to refer to “hillslope” instead of “slope” in order to make things more clear.

6983,24-25: “we hypothesize that hill slope processes are the main drivers of topographic evolution” – Please outline your basic assumption for this hypothesis. Why is not bedrock rivers that would dictate topographic evolution instead? You should also outline the implications if hillslope processes indeed were representative of tectonic forcing in your study area.

6984,7: “We analysed” – Some slight repetition of what you mentioned earlier. Try keeping the research hypothesis and objectives as concise and tightly interwoven as possible.

6984,10: “tectonic gradient observed by Demoulin and Hallot (2009)” – Sketch out the nature of this gradient. Not all readers may have read this paper.

6984,22: When did this “erosional phase” occur?

6985,1: “Terrace sequences” – Is it strath or fill terraces that provide this record?

6985,10: “characterized by a moderate seismic activity” – You may want to quantify what you mean by “moderate”.

6985,17: Delete “relatively”. You do not compare this pattern with others.

6985,18: “good opportunity to isolate the tectonic imprint” – This needs some more explanation. Is this not part of your hypothesis? The way this reads here suggests that

HESSD

7, C2466–C2476, 2010

you are somewhat biased towards this opportunity, whereas you should test it first in order to keep the argument consistent.

6985,19: "selected 10 catchments" – What was the rationale for selecting these catchments? In which ways are these considered being representative?

6985, 25: "tectonic domains with uplift rates" – If these rates were derived from incised river terraces originally, then how have possible effects of sea-level changes been considered in disentangling climatic signals from tectonic ones in terms of uplift?

6985,27: I do not think that you need a negative sign here.

6986,3: "DTM" – Conflicts with what is stated in preceding sentence. Did you use a DEM or DTM?

6986,12-13: "transversal river and slope profiles were extracted using the "3-D Analyst" ArcGIS extension" – This needs some more specification, particularly as this Special Issue of HESS is concerned with morphometric techniques.

6986,15: Please discuss the meaningfulness of an "average" uplift rate per catchment.

6986;19: Despite being "Classical morphometric indices" you may want to briefly explain their definition or at least their hypothesized potential for indicating tectonic signals in topography. This ties up with my comment on the topic of this Special Issue.

6986,26: "relief in a 100m range moving window" – Relief cannot be part of a definition of relief. Use something like "maximum elevation difference" instead. Also briefly justify the use of a 100-m window, given that relief is partly scale-dependent. What was the shape of the window?

6987,1: "consists of the median value" – Again, please justify the choice of this particular statistic.

6987,4: "extracted the river longitudinal profiles" – There are several techniques to do this with a range of (dis-)advantages that come along. It may be useful to learn what

HESSD

7, C2466–C2476, 2010

method you applied here.

6987,6-7: “not affected by local morphological changes related to river confluences” – Reads a bit vague. What changes do you mean here, and how were these detected?

6987,11: The index iterations in Equations 1 and 2 should be explained. Is this iteration cell-wise or profile-wise?

6987,16: “important increase” – Consider rephrasing to “significant”. Are you only looking at concave-up knickpoint reaches? If not, modify “increase” to “change”.

6987,21: “knickpoints with tectonic origin have been identified in the selected river channels” – Unclear how you achieved this. What were your criteria for establishing a tectonic origin of knickpoints?

6987,23: Unit catchment area is [mˆ2].

6988,1: Unit channel-bed slope is [m/m] or [1].

6988,11: “has been proved to be” – Rewrite to “is”. There is nothing to prove here.

6988,19: “some rivers display clear convexities (also called knick zones)” – Convexity of which type?

6989,2: “are comprised” could read “range”.

6989,19: “scale-dependency of the hypsometric integral” – Please provide a reference. The description of the HI outweighs those of the other metrics you have used. Make sure you give a balanced account of each of these in order to avoid any preliminary bias.

6989,22: “position of the knick zone” – How did you delineate these knick zones? And how did you objectively identify the “main stream convexity”? (line 24).

6990,1-10: Almost every sentence in this paragraph features the word “cluster”. Try to be more concise and use the space for better explaining the method itself rather than

HESSD

7, C2466–C2476, 2010

mentioning it several times.

6990,16: Drop quotation marks.

6990,18: “the absolute height of the river channel convexity” – Explain how convexity can be delineated by a single elevation.

6990,24-25: “correlation between the mean uplift of the catchments and the stream concavity index” – This is intriguing, given that stream-power theory predicts that river longitudinal profile concavity is independent of uplift rates. Perhaps consider elaborating on this point?

6991,1-3: “This might partially be explained by the presence of local lithological contrasts, but might also be associated with local tectonic activity” – So which of the two do you think is more plausible?

6991,7: “K-means cluster analysis” – This would be a good location to state the total number of observations per parameter for your ten catchments. In other words, state the size of your matrix. Consider showing the results of the cluster analysis (dendrogram, etc.) in a separate figure.

6991,23-24: “a river with low intercept (i.e. ksn = 4) can be interpreted as having a greater steepness of its longitudinal profile” – This is odd. Is the normalized steepness index not an indication of the steepness of a longitudinal river profile? Please reconcile.

6992,7-8: “region of highest uplift rates [. . .] is characterized by high values of local relief (Fig. 2c)” – Fig. 2c shows the amount of mean uplift, but not any rate. Please reconcile. Furthermore, the depicted trend is weak (R^2 = 0.2) and most likely insignificant, given that local relief varies less than twofold (i.e. between >4 and <8 m!) for more than a twofold increase in total uplift. Has the outlier “Ai” been included in the fit? One might imagine that it could easily distort the trend.

6992,10: Change “long-term” to “Quaternary”.

HESSD

7, C2466–C2476, 2010

6992,13-16: “not only the direct result of the differential uplift pattern, but might also reflect the transient response of the catchments to relative base level lowering”. – So how can you be so sure about detecting tectonic signals in your set of terrain variables?

6992,28: “hydrologically more distant parts” – Do you mean “upstream” or “downstream” here?

6993,3: Maybe it is not so much the “nonlinear relation” that is striking here, but rather the grouping of data points into distinct domains. See my comment on Fig. 4 below.

6993,8: “base level changes following the uplift” – How do you know it was not the other way around, i.e. that uplift followed base-level changes?

6993,12: “regions with weaker lithologies or long incision history” – You have provided no information whatsoever on the importance of rock type or incision history. If these are deemed important, you should give some background information.

6993,13: Which “theoretical model” do you refer to?

6993,16: How were “tectonically-driven knickpoint”s identified? Also; “not yet been affected by the uplift and thus remained in equilibrium state” – Earlier on, you argued for a strong disequilibrium in the landscape. What evidence do you have of potential lag effects? 6993,18-19: “multivariate analysis [...] is necessary to fully understand the link between slope and channel morphology and tectonic activity” – You may want to comment whether this full understanding has been achieved eventually.

6993,20: “When we...” – This would be a good paragraph to kick-start the discussion, although some of your groups contain only two members.

6993,27-28: “alluvial stream systems where slope and channel processes are coupled” – But are alluvial streams not characteristic of a decoupling of hillslope and channel processes, given that the alluvial sediments act as a buffer?

6993,29: “The B scheme” could read “Group B”. Same below.

HESSD

7, C2466–C2476, 2010

6994,2: “decoupling of channel and slope processes” – You may want to specify that it is the upper hillslopes that become decoupled (if I understand this correctly).

6994,10-11: “confirms the strength of stream proximal curvatures and stream proximal slopes as morphometric indices of relief rejuvenation” – This assertion is unfortunately not substantiated by your data. You do not show any evidence of rejuvenation (i.e. temporal information for at least two distinct phases of erosion), hence your morphometric variables cannot verify this. You may want to look at similar work that has used similar metrics, e.g. Densmore and Hovius, 2000 (Geology) or Korup and Schlunegger, 2007 (JGR Earth Surface).

6994,16-17: “slope and channel morphology is an indicator of transient adjustment of rivers to tectonic uplift” – Consider adding “and/or base-level changes”. Overall, this finding is hardly novel, and a number of studies (some of which you duly cite) have demonstrated this.

6994,17: “general agreement” should read “correlation”.

6994,19: “some metrics appear to be insensitive” – This is some important information. Why not outline the peculiarities of the various parameters in the discussion?

6994,21-22: “are better indicators of recent tectonic activity than the general hillslope form and relief” – In general, this has also been proposed by many recent studies. In your specific case, you base this notion on the goodness of fit, i.e. statistical correlation, but not necessarily on physical based arguments. Moreover, this statement seems to require rejection of your initial hypothesis, which you seem to have discarded along the way. Re-visiting this would make the conclusions a bit stronger, bearing in mind the caveats mentioned above.

6994,22: “located far in the hydrological network seems” – Grammar. Also use either “upstream” or “downstream” or refer to stream order to clarify what you mean.

Table 1 features a number of spurious parameters; MU was surely not determined

HESSD

7, C2466–C2476, 2010

to the nearest mm. Please round to the most reliable digit. Is “surface” a planform or projected area? And what are the bounds? Make sure you explain all parameter abbreviations tabulated. Once again, please state how you objectively detected the “convexities”. References cited need publication years.

Fig. 1b shows the “main channel convexity” as circles. How has this been identified?

Fig. 3 could feature the locations of knick zones described in the text. Is it possible to group profile signatures by cluster group affinity?

Fig. 4 shows a second-order polynomial fit to the data. Is there any physical rationale why you would expect the HI to scale with the second power of the relative knickpoint height? Maybe the trend line masks more than it elucidates here. From your descriptions I gather that some sort of discriminant analysis would be more suitable here. After all, you are trying to distinguish between three types of catchments.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 6981, 2010.