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Interactive Comment

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

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The comments of the reviewer1 were helpful and have been addressed in the revised manuscript. We feel that the manuscript is stronger and more adapted to the special issue of HESS. The initial comments and the corresponding replies are included below.

GENERAL COMMENTS

1) 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.

REPLY: The uplift isolines presented in the paper of Demoulin and Hallot (2009) are based on the stratigraphy of the river terraces of the Meuse and Rhine-Mosel rivers. In our study, we have analysed only third-order tributary rivers of the Meuse River; and we do not make any inference based on the terrace stratigraphy that exists for the Meuse and Rhine-Mosel River. Moreover, Demoulin and Hallot (2009) reconstructed past geomorphic features (Tertiary planation surfaces) to corroborate roughly the history of the uplift. The tertiary landscape of the Ardennes and Rhenish Shield was composed of sub-horizontal planation surfaces that have been formed in tropical conditions at low altitudes. The fact that these planation surfaces were almost horizontal during Tertiary era makes them an ideal marker for subsequent deformation (i.e. the Quaternary uplift).

If we refer to recent theories on the upwelling of the Rhenish Shield, the intrusion of a domal buoyant body in the lithosphere would have been the trigger of a regional uplift centred on the Eiffel Range, and including the Ardennes massif. Several papers have used seismic tomography techniques to create a thermal model of the European lithosphere (Tesauro et al., 2009 based on the observations of Ritter et al., 2001). We may interpret their results in terms of thinning of the lithospheric mantle in this region, and the map of the temperature variation at 60km depth shows an important gradient in our area of interest, varying from 1100°C on the Eastern Ardennes to 800°C on the western part. These results from numerical models are conform with the uplift pattern that was proposed by Demoulin and Hallot (2009) based on terrace stratigraphy and planation surfaces. As different techniques (geomorphic markers such as terrace stratigraphy and planation surfaces, but also seismic tomography) indicate a similar pattern of uplift for the Ardennes Massif, we are confident that our analysis is robust.

[1] Tesauro, M., Kaban, M., Cloetingh, S.: A new thermal and rheological model of the European lithosphere, Tectonophysics, 476, 478-495, 2009.

[2] Ritter, J., Jordan M., Christensen, U., and Achauer, U.: A mantle plume below the Eifel volcanic fields, Germany, Earth Planet.Sci. Lett., 186, 7-14, 2001.

2) 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 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.

REPLY: In order to analyse links between tectonic uplift and river incision, we first analysed any possible influence of sea level change related to Quaternary climate change. In our study, we can consider that the effect of eustatic sea level change on the fluviomorphology of the third-order rivers is negligible. This assumption is based on several papers that demonstrate that the fall in sea level during the LGM caused incision only in the lower part of the river Meuse catchment (located in the Netherlands) [1]. The knickpoint created by the eustatic base level fall did not retreat further upstream than Maastricht [2], and therefore did not reach the Ardennes Massif. The analyses of river terrace sequences in the Meuse River confirm this [3 and 4].

Moreover, due to the small latitudinal (0° 52') and longitudinal (1°12') extension of the study area, we assume that all the selected catchments have been affected by similar climatic conditions, as well as similar climate changes during the last Million years. As we are analysing spatial patterns of hillslope and channel metric, the spatial variability in climate over our study area is assumed not to affect our analyses.

The suitability of our methods to detect a tectonic imprint are now clearly addressed in the description of the methodology. We here copy some sentences from the revised version of the manuscript.

- "We used three classical morphometric indexes (Gravelius, Horton, Schumm) to describe the overall morphology of the catchments. We assume that catchments that are subject to high rates of tectonic uplift would be prone to have an elongated shape, and that catchments located in tectonically quiet regions would rather be characterized by a dendritic river network and a round catchment shape.

- "The Local Relief index was used to quantify the local variation in altitude within the catchments. This index can directly be used to assess catchment-wide hillslope steepness values. Catchments with a higher Local Relief Index are assumed to be subject to a greater tectonic influence than catchments presenting a low LR value."

- Flint's Law (Slope-Area diagram): "Under steady-state conditions, a balance may exist between local channel steepness and the upstream drainage area. The overall channel steepness can then represent the relationship between net uplift and net erosion. A comparison of the observed Slope-Area relationships for the 10 rivers in the Ardennes Massif allows us to compare the channel morphology of rivers draining highly different tectonic regimes. The normalised steepness values (Ksn) can then be used to compare the steepness between different river systems; with larger values of Ksn representing steeper rivers."

- Stream Concavity Index: "A river profile with positive SCI value will be considered as concave up, and a negative value is an indicator of a convex up river profile. We modified the initial formulation of the index so that the values are comprised between -1 and +1, with an equilibrium concave up longitudinal profile having a SCI of about 0.5. Convexities in the river profiles often indicate disequilibrium along the river stream course that can be related to unsteady tectonic or climatic conditions."

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- Hypsometric Integral: "Differences in the shape of the hypsometric curve have been related to differences in erosive and tectonic processes, and the value of the hypsometric integral has been interpreted as an indicator of the degree of disequilibrium in the balance between erosive and tectonic forces for a particular landform. When comparing the morphology of catchments, the catchment with the largest value of HI has the largest amount of landmass above the outlet, and this can be interpreted in terms of lower erosion or higher tectonic activity."

[1] Törnqvist, T.: Longitudinal proïňAle evolution of the Rhine–Meuse system during the last deglaciation: interplay of climate change and glacio–eustasy? Terra Nova 10, 11–15, 1998.

[2] Veldkamp, A., van den Berg, M.: Three-dimensional modeling of Quaternary fluvial dynamics in a climo-tectonic dependent system. A case study of the Maas record (Maastricht, the Netherlands). Glob. Planet. Change 8, 203–218, 1993.

[3] Juvigné, E. And Renard, F.: Les terraces de la Meuse, de Liege à Maastricht, Annales de la société géologique de Belgique, 115, 167-186, 1992.

[4] Van den Berg, M.: Fluvial sequences of the Maas, a 10Ma record of neotectonics and climate change at various time-scales, Wageningen, 181pp, 1996.

3) 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.

REPLY: Some sentences have been added to the discussion part to nuance the interpretation.

The amounts of uplift that we present in the tables and in the text are the ones that have been published by Demoulin and Hallot (2009) and correspond to the uplift that follows 0.73 My. Those amounts of uplift can be converted into uplift rates by multiplying the

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uplift amplitude by 0.73. Which gives the uplift in m/My.

4) 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.

REPLY: We now added the equations of the morphometric parameters that were missing in the previous version of the manuscript, and also describe the relevance of these parameters for analysing tectonic imprint in the landscape (see reply to comment above).

5) 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.

REPLY: In order to classify the catchments 'objectively' according to their different properties, a multivariate statistical classification method has been chosen. We preferred to use an unsupervised classification method, such as the K-mean clustering method. It is clear that it would have been better to use data from more than 20 catchments to obtain a robust classification.

The limited amount of samples included in our analysis can be explained by the following reasons. First, we limited our analyses to second or third-order rivers with minimum area of 150km^2 to enable a valid comparison between the catchments and avoid scale related issues. Second, we restricted our analyses to catchments that are underlain by similar rock types (in our case, hard metamorphic rocks of the Paleozoic era) to avoid any possible influence of lithology. We also avoided areas where major fault systems have been identified. The selection criteria are now discussed in the text.

Several sentences have been added in the discussion with regard to the limited number of observations.

6) 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.

REPLY: Thanks to the reviewer for this useful suggestion. We have rearranged the Results/Discussion paragraphs in a more transparent way in order to better distinguish the different parts of the manuscript.

7) 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.

REPLY: The identification of the knickpoints was also highlighted by Reviewer3 (see also reply to Reviewer3). We now give more details in the text on the identification of the tectonically induced knickpoints. Knickpoints were identified based on the river longitudinal profiles and the slope-area diagrams. We then carefully analysed any possible influence of lithology or fault systems. Catchments with obvious lithological contrasts

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or presence of reported faults were discarded from our analyses. It is possible that the non-steady character of the river morphology is partially related to changes in climatic conditions during the Quaternary, in conjunction to active tectonic. However, as we are focusing on spatial patterns in river and hillslope morphology in an area of spatially relatively uniform climatic conditions during the Quaternary, this does not affect the quality of our analyses. We have rephrased this section to clarify our ideas.

Concerning the (dis-)equilibrium state of the studied rivers, Demoulin (1998) draw a large number of longitudinal profiles of second and third order rivers in the Ardennes Massif. A large number presents important convexities (knickpoints), which is an indication that some parts of the landscape of the Ardennes massif are not yet in equilibrium state. However, we cannot generalize this concept; and there are also several rivers that present typical concave up profiles (in equilibrium state, with SCI $\sim$0.5, like the Vierre in our dataset). It is exactly this spatial variation in hillslope and channel morphology within the Ardennes Massif that is interesting, and that is the central topic of this research paper. This is also the reason why we observe equilibrium and dis-/non-equilibrium states in the landscape of the Ardennes.The Figure 4 does not pretend to have a prediction potential, the polynomial curve and equation has been set to inform the reader about the overall shape of this relation. The equation has now been removed from the graphic, as it was based on a limited number of observations (10 catchments).

8) 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?

REPLY: A sentence has been added in the discussion section concerning the (in-)validated hypothesis.

Take home message: It is illusory to think that a single morphometric index can reflect the impact of tectonic uplift on the topography of a specific region. On the other hand, the analysis of a reduced number of slope and channel metrics that represents the complexity of landscape morphology (surrounding slopes, channel gradient, overall concavity, etc.), can lead to a more accurate estimation of the exogenous agents that have shaped the landscape. We have added some sentences to the conclusion to clarify this concept.

Channels metrics are usually more prone to highlight tectonic imprint on the landscape because river channels have a shorter response time and are more sensitive to small tectonic perturbations.

It is clear that correlation is not an indication of physical feedback, but only indicates possible statistical association. On the other hand, statistical analyses can provide valuable 'objective' information on associations between different parameters, and are a starting point for further discussion of possible physical feedbacks.

9) 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.

REPLY: The caption has been updated to include the full name of the catchments and their abbreviations. The new Figure 2 (Geological Map) contains the full name of the catchments.

SPECIFIC COMMENTS

6981,7: Delete "wide".

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REPLY: Change has been made in the text.

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

REPLY: The sentence has been modified in: “This work focuses on a range of slope and river channel morphometric indices to study their explanation potential in regions affected by low to moderate tectonic activity”.

6982,9: Insert “Belgium” somewhere here.

REPLY: Change has been made in the text.

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?

REPLY: A sentence has been added in the abstract to localize the river in Europe. The Ardennes Massif is mainly drained by tributaries of the Meuse River, which is one of the major rivers in northern Europe with a catchment area of 36000 km^2.

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

REPLY: Change has been made in the text.

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

REPLY: The article of Douvinet et al is a literature review of “classical” morphometric indices that were developed in the eighties-nineties.

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

REPLY: Change has been made in the text.

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

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outline the implications if hillslope processes indeed were representative of tectonic forcing in your study area.

REPLY: This hypothesis was mainly set up under the statement of Montgomery and Brandon (2002)[1], where they stipulate that in low-relief areas, landscape lowering is mainly driven by hillslope processes. According to their observations, in low-gradient landscapes, erosion rates can be linearly related to mean slope or local relief. Following this hypothesis, changes in climate or tectonic forcing can influence landscape-scale erosion rates in low relief areas through changes in hillslope steepness. This also assumes that landscapes in low relief areas adjust quickly to changes in base level, and that the hill slope processes are controlling erosion processes. We have added a few sentences to clarify our basic assumptions for this hypothesis.

[1] Montgomery, D. and Brandon, M.: Topographic controls on erosion rates in tectonically active mountain ranges, Earth and Planetary Science Letters, 201, 481-489, 2002.

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.

REPLY: Change has been made in the text.

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.

REPLY: the sentence has been modified: “We analyzed possible correlation between the rock uplift pattern and slope and river channel indices; and performed a cluster analysis to explore spatial aggregation of geomorphic response to the NE-SW uplift gradient described by Demoulin and Hallot (2009).”

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

REPLY: Between 0.65 and 0.7 My ago [1]

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[1] Van Balen, R., Houtgast, R., Van der Wateren, F. and Vandenberghe, J.: Neotectonic evolution and sediment budget of the Meuse catchment in the Ardennes and the Roer Valley Rift System, Netherlands Journal of Geosciences / Geologie en Mijnbouw, 81, 211-215, 2002.

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

REPLY: Strath terraces.

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

REPLY: "Seismic" has been replaced by "Tectonic". The maximum uplift rate to which the Ardennes were subjected to was approximately 0.5 mm.year-1 (500m/MY). Moreover, between 0.75My and 0.4My the elevation of the north-eastern part has increased by 700m. We now specify this in the text.

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

REPLY: Change has been made in the text.

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

REPLY: We rephrase the sentence: "The presence of spatially uniform but temporally variable climatic conditions during the quaternary affords a good opportunity to test the tectonic imprint on landscape evolution."

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

REPLY: This explanation has been also given to reviewer 2. "We have selected only ten catchments for this analysis based on the following criteria. First, we selected catch-

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ments of the same order of size to avoid some unwanted scale-distortion of the results (between 150 and 250 km^2 as written in the paper). Second, we performed a selection of the catchments where a consistent and regular database of topographic and geological data was available. This is now clarified in the text. The “exceptions” we are talking about in the paper are essentially very specific cases were a river capture has occurred. In the Ardennes Massif, those river captures are very well documented (e.g. Demoulin, 1998), and the slope convexities they have induced in the longitudinal profile (following the base level drop) are limited in both horizontal and vertical extension. For obvious reasons, we did not include these catchments in our knickpoint analysis.”

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?

REPLY: This issue has been addressed above.

The uplift rates were based on the stratigraphy of river terraces of the Meuse and Rhine-Mosel Rivers. The uplift pattern is conform with other studies based on local geodetic data (i.e. Mälzer et al., 1983), data on Quaternary volcanism in the Eifel (Ritter et al., 2001; Schmincke, 2007).

[1] Mälzer, H., Hein, G., Zippelt, K.: Height changes in the Rhenish Massif: determination and analysis. In: Fuchs, K., vonGehlen, K., Mälzer, H., Murawski, H., Semmel, A. (Eds.), Plateau Uplift, the Rhenish Shield âĂŤ a Case History, Springer, Berlin, pp.165–176, 1983.

[2] Schmincke, H.U.: The Quaternary volcanic fields of the East and West Eifel (Germany). In: Ritter, J., Christensen, U. (Eds.), Mantle Plumes, a Multidisciplinary Approach, Springer, pp. 241–322, 2007.

[3] Ritter, J., Jordan, M., Christensen, U., Achauer, U.: A mantle plume below the Eifel volcanic fields, Germany, Earth Planet. Sci. Lett., 186, 7–14, 2001.

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

REPLY: Change has been made in the text.

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

REPLY: The same question has been asked by Reviewer #2: “The elevation data provided by the Geographical Institute of Belgium (IGN) is called “DTM 1:10000”, and different data sources were used for the realization of this product : photogrammetric derived points and structure-lines, airborne laserscanning (points) and field observations (points). It actually represents the ground “real” surface (i.e. not the treetops or roofs). The DTM acronym is now used systematically throughout the manuscript.”

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.

REPLY: We have addressed this issue in our reply to S. Grimaldi. This paragraph in the text has been enlarged to include the details of the DTM pre-processing.

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

REPLY: A continuous uplift raster covering the entire Ardennes Massif has been interpolated based on the uplift isolines presented in Demoulin and Hallot (2009). A mean value has been calculated for the selected catchments. As our catchments are small, the variability in uplift rate within the catchment is limited according to the information provided by Demoulin and Hallot (2009).

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.

REPLY: We have added a sentence in each index description to highlight their aptitude

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in determining tectonic influence (see our response to general comment #2).

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?

REPLY: The sentence has been rephrased: “...with local relief here defined as the maximum elevation difference observed in a user defined window of a 5x5 cell square (i.e. a 100m roving window).

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

REPLY: We have chosen the median in order not to be affected by extreme values.

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 method you applied here.

REPLY: We extracted longitudinal profiles by crossing the river flow line with the contour lines. For each intersection, the distances to outlet (upstream distance) as well as the elevation values were obtained in a table. The main advantage of this method is to avoid flat areas in the profiles (there is always a slope gradient between adjacent contour lines). We now specify this in the text.

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?

REPLY: The location of the transversal profiles was selected manually along the river channel, thereby avoiding areas of confluence with the main tributaries where we detect a decrease of proximal slopes. We now specify this in the text.

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

REPLY: Profile wise for the transversal profiles. We now clarify this in the text.

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

REPLY: This sentence has been rephrased: "Those knickzones are sections of the river channel that display significant increase in the slope gradient, ..."

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?

REPLY: We assumed that those are tectonic knickpoints due to the absence of lithological contrasts, faults and to the absence of anthropogenic elements in the surroundings.

6987,23: Unit catchment area is [mËĘ2].

REPLY: Change has been made in the text.

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

REPLY: Change has been made in the text.

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

REPLY: Change has been made in the text.

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

REPLY: Convexities in the longitudinal profiles. Change has been made in the text.

6989,2: "are comprised" could read "range".

REPLY: Change has been made in the text.

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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.

REPLY: The following reference has been used: Willgoose, G., Hancock, G.: Revisiting the hypsometric curve as an indicator of form and process in transport-limited catchment, Earth Surface Processes and Landforms, 23, 611–623, 1998.

Change has been made in the text; we now also describe the other hillslope and channel metrics in more detail.

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).

REPLY: We have addressed this comment partly above. In the study area, there are knickpoints of various origins and of various magnitudes. Variations in lithology are a frequent cause of knickpoint development, and are also observed in the Ardennes Massif where there are clear lithological contrasts between the hard metamorphic rocks and the relatively soft sedimentary rocks. The absence of major lithological contrasts was one of the main selection criteria for the catchments (see reply to comments above).

In some of our catchments, there are small lithological contrasts where small knickpoints have developed. They are visible in the longitudinal profiles, and these lithological-induced knickpoints are always of minor vertical amplitude (range comprised between 5 to 10 meters) and are spatially limited to a narrow zone. They can be differentiated from the tectonic knickpoints that have a much larger vertical extension and extend over several hundred meters. The precise location of knickpoint has been identified with the greatest curvature value along the presumed knickpoint section.

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

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mentioning it several times.

REPLY: Change has been made in the text.

6990,16: Drop quotation marks.

REPLY: Change has been made in the text.

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

REPLY: We have rephrased this sentence. We derived the elevation of the shoulder (upper part) of the convexity.

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?

REPLY: We observe that the stream concavity index is an indicator of the disequilibrium of the river profiles. Streams that are characterized by clear convexities in their longitudinal profile are concentrated in the region with higher rates of tectonic uplift. Given the fact that we only have data from 10 catchments, we cannot make any hard statements, but we have elaborated this point in the revised version of the text.

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?

REPLY: Local tectonic activity is the most plausible according to the abnormal shape of the terraces in this region (local subsidence) (see Demoulin, 1998)

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 (dendro-

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gram, etc.) in a separate figure.

REPLY: The size of the matrix is [10x5] (observations x parameters). Here follow (Annex 1) the dendrogram given by the Ward classification method. We did not add this graphic as a figure due to the space requirements and the little information it provides, but the figure is available in the online version of HESSD.

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.

REPLY: This is correct. Indeed, a relatively high value of Ksn (intercept) can be interpreted in terms of high steepness in the longitudinal profile. We deeply revised the Table 1 according to the reviewer comments and found two problems in our calculation of the Ksn values. The values of Ksn have been corrected in the table as well as the interpretation in the text.

The Bocq and the Warche display a particular shape in the Slope-Area diagram (initial Figure 2). Indeed, in the lowest part of their profile, they present relatively high slopes (black dots around 10E8 square meters) that were not taken into account in the calculation of the Ksn. For that reason, we only calculate the Ksn for the upper part of the profile (the part located upstream of the knickpoint), where relatively smooth slopes can be observed. The values of Ksn were recalculated (taking into account the lower part of the catchment) and are respectively 38.5 for the Bocq and 95.5 for the Warche.

The following Figure (annex 2) represents the set of selected rivers in the AS space and with the corrected normalized steepness values (instead of the normal steepness).

We can see the three clusters that have been designed by the k-mean analysis. The first group (Hermeton, Molignée, Vierre ; green circles) have a Normalized Steepness (intercept) lower than the other groups. The second group is characterized by a relatively high value of steepness (Hoegne, Aisne, Wamme ; red crosses). Finally, the last cluster of rivers (Warche, Bocq, Hoyoux, Salm ; blue dots) is considered as having a medium normalized steepness index, except for the Warche that have the highest steepness of the studied rivers. The fact that the Warche is in the same group than the Bocq, Hoyoux and Salm, is due to the fact that the clustering is multivariate and is based on other river and hillslopes indexes (Rch, Ho, HI and SCI).

The steepness value that we discussed in the original manuscript was not the Normalized steepness (Ksn) value but the Normal Steepness (Ks). The latter is known to be highly influenced by the theta value (Ks=4, and not Ksn=4). We have adjusted the text of the discussion, and now base our discussion on the normalized steepness values.

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 (RËĘ2 = 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.

REPLY: The amounts of uplift that we present in the tables and in the text are the ones that have been published by Demoulin and Hallot (2009) and correspond to the uplift that follows 0.73 My. Those amounts of uplift can be converted into uplift rates by multiplying the uplift amplitude by 0.73. Which gives the uplift in m/My.

The Local Relief Index was created by taking the median value of the local relief maps (so this index is less affected by the outliers than the mean). This explains why the value range of this index is comprised between 4 and 12 meters (threefold increase). The Aisne (Ai) value has been considered into the analysis, and this is why the R^2 is rather weak. Given the fact that the number of observations is small and that the R^2 is low, we do not include the regression fit in the revised version of the manuscript.

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

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REPLY: Change has been made in the text.

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?

REPLY: the relative base level lowering has to be distinguished from the climatic base level lowering that affected the entire area and that is limited outside the studied area (see general comment #2). Here, the tectonic signal can be interpreted in terms of relative base level fall due to the uplift. We now clarify this in our revised text.

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

REPLY: Catchments located far upstream will ‘feel’ the relative base level lowering later than the catchments that are located in the downstream part of the Meuse River catchment.

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.

REPLY: A paragraph that points out the cluster grouping in this graph has been added to the text.

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?

REPLY: In our study, we are not considering the eustatic sea level changes as they affected mainly the lower part of the Meuse River system. For the Meuse-Rhine River delta, separating the effects of eustasy from neotectonics and climate remains extremely complex (see TOPOEurope project). For the Ardennes Massif, there is a good record of relatively well-dated terrace levels of major tributaries of the Meuse River; and the uplift pattern is constrained using very different techniques (stratigraphy, tomography, and planation surfaces, see references in our reply). The base level changes that

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we are referring to in our manuscript are small changes in the elevation of the confluences of the tributaries with the Meuse River that are caused by gradual incision of the rivers in the Ardennes Massif.

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.

REPLY: A geologic map has been requested by another reviewer and added as a new figure. The incision history is now given in more detail in the introduction. The initial relief was characterized by planation surfaces that developed from the early Cretaceous until the Oligocene. From then, the tectonic activity of the Ardennes started to increase, to reach maximum uplift rates in the Middle of the Pleistocene. The rivers started to incise the valleys, and different terrace levels have been identified and dated for the major tributaries of the Meuse River (Mid-Pleistocene).

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

REPLY: The “theoretical model” of proximal hillslopes evolution following knickpoint migration that we discussed in this paper. Some specifications have been added in the text to clarify this.

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?

REPLY: Yes, there is a mistake in the text. We should read: “. . . that has not yet been affected by knickpoint retreat.” The regional uplift affected all the catchments located in the Ardennes.

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

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whether this full understanding has been achieved eventually.

REPLY: Any multivariate analysis can be able to reveal statistical relations between the tectonic activity and the landscape morphology. But in our case, by selecting this specific study area, we were able to diminish the influence of some exogenous factors (such as climatic influence) and focus on the links between a specific tectonic event and the morphological parameters of the hillslopes and channels. Change has been made in the text to clarify this concept.

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

REPLY: We will take into account the comment of the reviewer to better arrange the discussion part.

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?

REPLY: Correct. We have rephrased this section.

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

REPLY: Change has been made in the text.

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).

REPLY: Correct. The hill slopes surrounding a knickpoint area are often characterized by steep and straight terrains (in the valley plain) and are decoupled from the processes that affect the plateau area, which is located further in the transversal profiles and characterized by a smooth sub-horizontal surface.

6994,10-11: “confirms the strength of stream proximal curvatures and stream proximal slopes as morphometric indices of relief rejuvenation” – This assertion is unfortunately

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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).

REPLY: We have rephrased this section, and now make reference to the work of Densmore and Hovius and Korup and Schlunegger.

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.

REPLY: Change has been made in the text. The reviewer is correct when he states that there are various studies that have addressed this topic in regions that are characterized by high relief and high uplift rates. This study on the Ardennes Massif is different from these previous studies as it discusses the link between slope and channel morphology and tectonic uplift for a region with low to moderate uplift rates. Several authors have argued that the link between landscape morphometry, erosion and tectonics is different for regions with low relief and absence of tectonic activity.

6994,17: "general agreement" should read "correlation".

REPLY: Change has been made in the text.

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?

REPLY: We now give more details on the different metrics, and their explanation potential for tectonic signals for the Ardennes Massif. We observed that classical morphometric indexes did not show any particular relation with the tectonic pattern observed in the study area.

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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.

REPLY: We have added some sentences to the conclusions, where we specifically address our initial research hypothesis.

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 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.

REPLY: Changes have been made in the text and tables. The catchment surface has been calculated on a projected surface (Belge Lambert, 1972). Publications years have been added.

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

REPLY: The same comment has been answered above about the identification of the tectonic knickpoints.

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

REPLY: We will indicate the location of the knickzones in this Figure. Due to the number of channel profiles in this graphic, it is difficult to colour the streamlines according to

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the different clusters.

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.

REPLY: There is no physical based argument here to explain the polynomial equation. Therefore, it has been deleted as it was based on a limited number of observations. A discriminant analysis could have been helpful. We will try to implement this in further research on the studied area.

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

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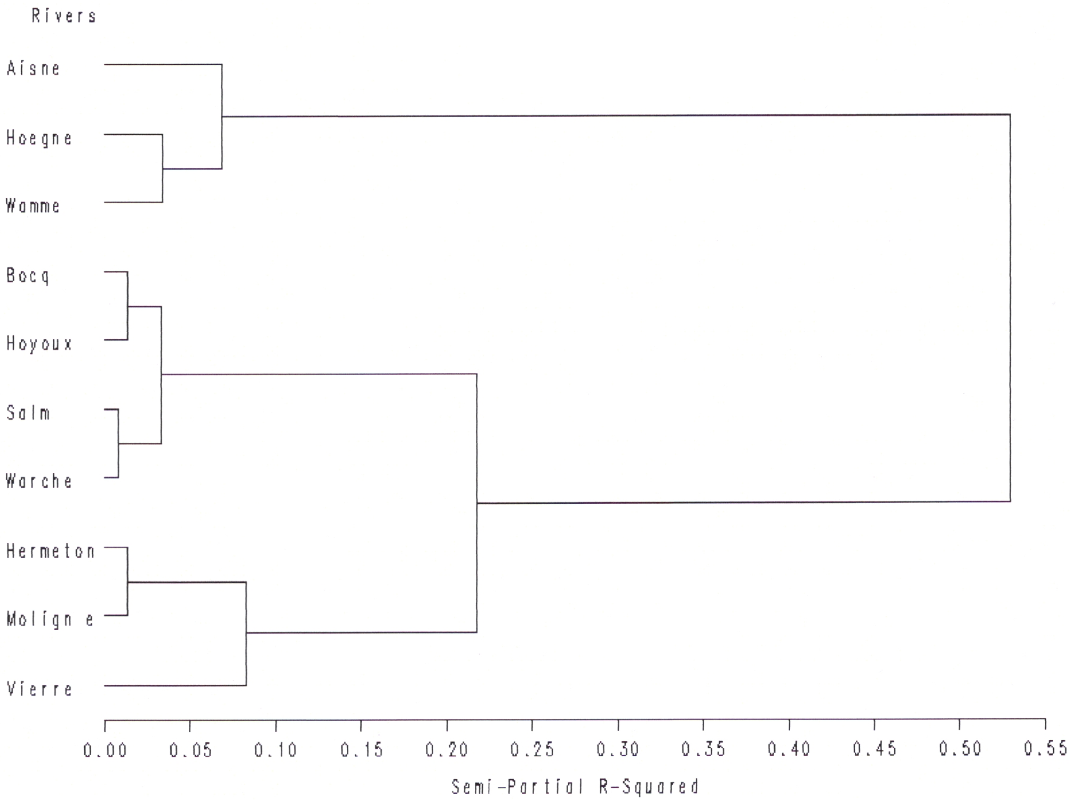

Fig. 1. Annex 1 : Dendrogram

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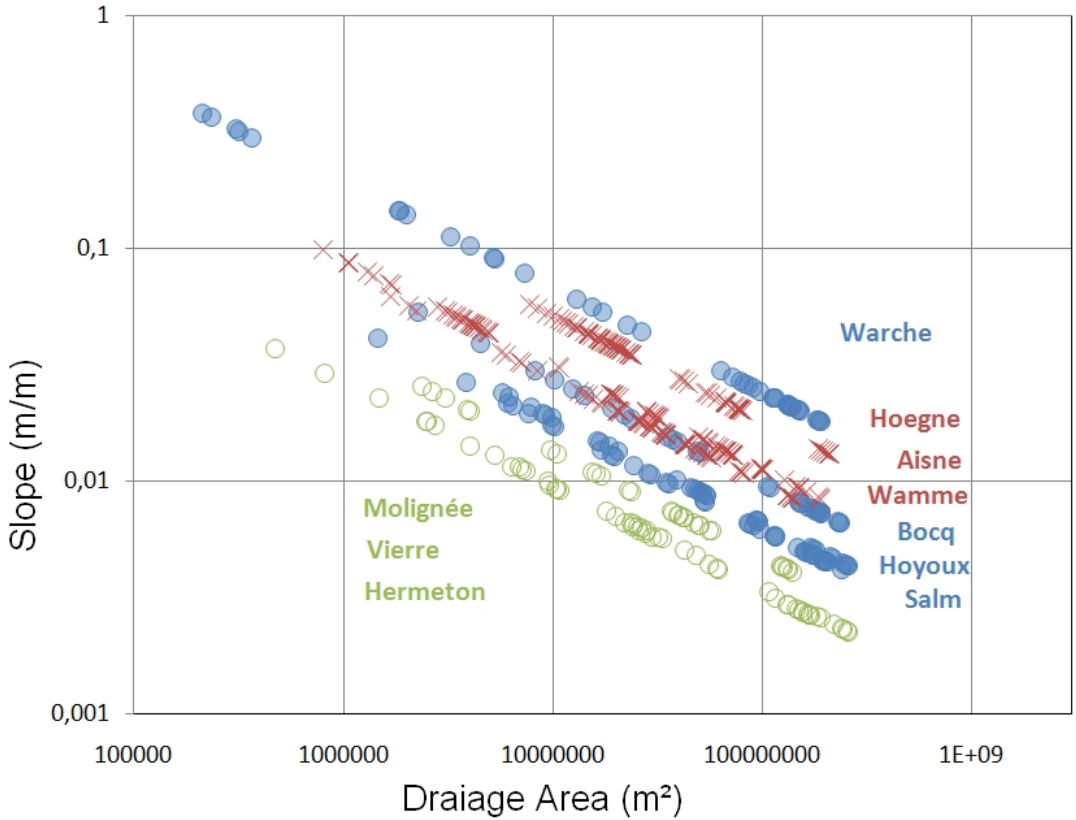

Fig. 2. Annex2 : Ksn distribution in AxS space

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