

***Interactive comment on*** “Regional scale analysis  
of landform configuration with base-level maps”  
**by C. H. Grohmann et al.**

**C. H. Grohmann et al.**

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April 13, 2011

Peter Molnar  
Institute of Environmental Engineering  
ETH Zurich, Switzerland

Dear Prof. Molnar

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As requested, we are submitting our responses to all comments made by the reviewers. We have carefully addressed all the issues that were raised. We were very pleased with the suggestions, comments and criticism as they helped to greatly improve the manuscript.

With kind regards,

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## S. Castellort

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*Received and published: 11 January 2011*

*Dear HESS community and Authors of the HESS-2010-365 paper "Regional scale analysis of landform configuration with base-level maps", This paper is a very interesting contribution, It raises a couple of additional questions:*

*1) Since water flows perpendicularly to topographic contour lines, and accumulates at a rate roughly proportional to slope, it seems inevitable that base-level lines defined on the base of stream orders will reflect closely the actual topography. Does that make sense?*

Yes. Base-level maps are derived from combined data of thalweg an stream order and thus reflect closely the actual topography. The difference between the actual topography and base-level maps is that these allows the elimination of the noise in topography caused by first-order drainage.

*2) If yes, the question then is: does topography always reflect the field of tectonic uplift?*

Not necessarily. Topography might be a reflex of lithological differences, of the structure, or both. Our intention is to present a fast GIS-based method that allows, once the interpreter discard the lithological effect, to aid in detection of areas that could show a reflex of the structure.

*In the example of Figure 4, the base-level surface is not very different from the actual*

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*topography, and the topography reflects the tectonics very closely because it is active.*

First we must remember that figure 4 is just an example of a normal fault, but it does not imply active tectonics. There are studies that relate neotectonic (e.g. Costa et al., 2001) and seismic (Assumpção et al., 1985, 2001) activity, but not necessarily in our study area. So it is possible that some of the lineaments interpreted in the study area are in fact active tectonic structures, or at least with recent tectonic activity.

*How does that work in other contexts, either when the tectonic field is more complex, for instance in the context of strike-slip deformation, or when tectonics are inactive and one wants to use base-level maps to unravel hidden geological structures? Hope these comments can help the discussion, Best Regards, S.C.*

Yes it is possible to use base-level (isobase) maps to detect structures in strike-slip contexts, as long as that topographic features have been dislocated by transcurrent faults (e.g. Ribeiro et al., 2006). In our paper, we show an application of this method in the detection of tectonic structures that still present some topographic expression in a region considered (generally speaking) to be tectonic inactive, in a similar way as other studies of the same nature carried on in Brazil (e.g., Grohmann, 2004; Grohmann et al., 2007).

Thank you for your comments.

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*Received and published: 11 January 2011*

1. *Abstract and Page 91 "Base-level maps are usually applied in semi-detail scale (e.g., 1:50 000 or larger) morphotectonic analysis" This is not correct. Filosofov (1960, p. 10) recommended to use 1:100 000 topographic maps with 20-m contours for platform terrains (plains). 1:50 000 and less were recommended for "poor"-manifested topography or poor topographic maps. Also, he recommended to use 1:1 000 000 topographic maps (Filosofov, 1960, p. 86) to reveal a general tectonic composition of large territories. I do not have his later publications (and publications of his collaborators), but the Filosofov method has been applied to small scales in 1950s-1980s in the USSR. There is a need to search Soviet (petroleum) geological journals and books of those years.*

2. *Page 98: "The base-level map also presented a good correlation with anomalies in geophysical data, which shows that the method is sensitive enough to detect features with little topographic expression." Again, since the Filosofov method has been used by petroleum geologists for about 30 years, such correlations has been observed and reported. Moreover, in mid 1980s Filosofov began to write a book "Theoretical Principles of Morphometry" presenting his theory of relationships between hypsometric and gravitational fields (<http://www.sgu.ru/node/56524>). This book was not completed and published, but there is a paper: Filosofov, V.P., 1988. Unity of hypsometric and gravitational fields. In: Logachev, N.A., Timofeev, D.A., and Ufimtsev, G.F. (Eds.), *Problems of Theoretical Geomorphology*. Nauka, Moscow, pp. 82-90 (in Russian). Sorry, I do not have this paper. <http://lccn.loc.gov/89121587>*

Thank you very much for such information. It is really hard for us to find (and read)

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Russian references. We were able to identify those passages in Filosofov's paper, and the text was modified to include this information. Unfortunately, we were unable to find those last works, but we have included a passage on the correlation between geophysics and base-level maps according to your comments:

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Base-level maps have been recently applied in semi-detail scale (e.g., 1:50.000 or larger) morphotectonic analysis (Golts and Rosenthal, 1993; Modenesi-Gauttieri et al., 2002; Grohmann et al., 2007; Jaboyedoff et al., 2009). Filosofov (1960) recommended to use 1:100.000 topographic maps with 20 m contours for flat terrains and 1:50.000 or less for 'poor'-manifested topography or poor topographic maps. This author also recommended to use 1:1.000.000 topographic maps to reveal a general tectonic composition of large territories. Moreover, the Filosofov method has been used by former-USSR petroleum geologists for about 30 years, who have observed and reported correlations between base-level and gravity maps (I. Florinsky, personal communication).

## Anonymous Referee #1

*Received and published: 18 January 2011*

*In the paper "Regional scale analysis of landform configuration with base-level maps", Grohmann et al. present a test of the applicability of base-level maps to regional scale analyses of landscape evolution. To that end, the authors have used a meso-scale (90m SRTM) dataset to produce base-level maps for various stream order combinations. They come to the conclusion that a 2-3rd order base-level map is capable of*

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*capturing tectonic strain from small-scale maps (1:250K to 1:1Million). The authors have done a fair job of covering the history and applications of base-level mapping, but do not address the much wider body of literature related to morphometric analysis as a whole. As detailed below, I have a few questions regarding the methodology itself, and the results of this analysis in particular.*

*General Comments: The idea of testing the utility of base-level maps at larger spatial scales is sound; however, based on the comment by Dr. Florinsky, it appears that this may have been done before (albeit in Russian). One of the vital, and unwritten, assumptions of this method is that streams are faithful recorders of tectonic strain. This is not a trivial assumption and requires a bit of space in the introduction. Because this method relies on the preservation of strain in the stream network, it may be con-founded when this is not the case. Stream long profiles are sensitive to many forces; lithology, climate, tectonics, bed-load, etc. If we think of two simple geologic scenarios, we can see that base-level maps may not be appropriate tools for identifying tectonic strain. The first case is a simple lithologic boundary. If the two rock types have different erosional properties (related to granular strength, joint spacing, bedding orientation, etc.), then the erosional coefficient  $K_{in}E = KQ^mS^n$  (where  $E$  is erosion rate,  $Q$  is discharge,  $S$  is channel gradient and  $m$  and  $n$  are variables) will change, which necessarily results in a change in the channel slope. The local order base-levels will therefore be different even in the absence of deformation. The authors themselves bring this up on page 91 and while discussing Figure 6. The second case is one of uniform strain in the form of rock uplift. For uniform rock uplift, the entire stream network is elevated and the relative base-levels that are investigated will show no difference relative to one another. Other channel metrics, which are also easily calculated, such as the reference slope  $S_r = S * (A_r/A)^{-m/n}$  (Sklar and Dietrich, 1998, or the channel steepness  $ks$  where  $S = ksA^{-\theta}$  (Hack 1973; Flint, 1974; Wobus et al., 2006 for methods), are capable of recording such strain. The upshot of this is that the authors should include a more complete analysis of the method assumptions and theory.*

*The authors also bring up the question of response time scales to tectonic perturba-*

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tions; first generally in the Abstract and Conclusions and specifically in Section 2.1 pg.93. I am happy to see that they have placed a boundary on the lifespan of a fault scarp, but this information does not help us much since they have not provided the tectonic history of the study area. I would like to see a section devoted to the geologic, tectonic, and geomorphic history of the study area. In order to assess the usefulness of this method, the reader must have information. Finally, I do not feel that enough has been written about the actual methods used to identify lineaments on the base-level maps. We are told that they are identifiable, but not how. I would like to see a section describing this in detail. Further detailed comments can be found below.

Thank you very much for your detailed review and comments. We agree that a better geological and tectonic context of the study area was important and it was included in the text. We also included a brief passage following your comments on sensibility of streams to other forces than tectonics:

Drainage network is a reliable indicator of tectonic activity. It is important to note, however, that stream long profiles are sensitive to other forces than tectonics, such as lithology, climate and bed-load, among others. For instance, a lithologic boundary of two rocks of different erosional properties will result in a change in the channel slope and the local order base-levels will therefore be different even in the absence of deformation. Other example is the case of an uniform rock uplift. If the entire stream network is elevated the relative base-levels that are investigated will show no difference relative to one another. In this case, other channel metrics such as the reference slope (Sklar and Dietrich, 1998), or the channel steepness (Hack, 1973), are capable of recording such strain.

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*Specific Comments:*

*Pg. 90, line 6: "lithologically". Also, I would imagine that this method (as with many others) is only capable of identifying tectonic influences if lithology is uniform.*

The text was corrected. Regarding the uniformity of lithology, this method is not *only* capable of identifying tectonic influences if lithology is uniform, but once you cross the interpretation of base-level maps with geological maps you may discard the obvious lithological differences, so the method will provide you with a good guide as to where there might be tectonic structures.

*Pg. 90, line 9 and 12: how was a regional scale defined? The topographic data are in the form of a digital raster with a pixel resolution of 90m. Whether this can be represented as a map of 1:50K or 1:1Million is more a question of computing power than cartographic definitions. Please clarify all data sources. Is it even fair to say that this method works at the regional-scale when the topographic base is generated from a much higher resolution dataset?*

The source dataset used was the SRTM30\_PLUS, which has a spatial resolution of 30-arcsecond, or approximately 900 meters at the Equator. The data is indeed derived from higher-resolution SRTM at 3-arcsecond (aprox. 90m), but we worked with the 30-second data. Our experience shows that the elevation data at 30-second is compatible with the information found in topographic maps at 1:1 million (contours, streams), and that was the factor used to define 'regional' scale.

*Pg. 90, line 11: ". . .used as a topographic base. . ."*

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The text was corrected.

*Pg. 90, line 17: "presents"*

The text was corrected.

*Pg. 91, line 3-7: This section is too general. The way it is currently written, it sounds like a reiteration of Davisian concepts of young and mature landscapes. I am guessing that the authors are refereeing to the headward erosion of sequential stream base-level perturbations.*

We believe this section is important to the concept of 'isobase'.

*Pg. 91, line 14: "lithologically"*

The text was corrected.

*Pg. 91, lines 15-20: I agree completely. It is good to clean up confusing terminology.*

Thank you.

*Pg. 91, lines 21-27: I do not understand this paragraph. Please give concrete examples of how tectonic strain, lithologic boundaries, etc. are expressed on the base-level*

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map.

This paragraph was modified in order to explain better how to interpret base-level maps in section 2.1 'Construction and interpretation'. We highlight the following:

Interpretation of base-level maps and identification of base-level lineaments is not an exact science, and experience plays an important role here. One must look for patterns in the base-level isolines, such as aligned and elongated features, compression or spreading and sharp deviations in contours which can be indicative of structures associated to tectonic movements, extreme lithological changes or important geomorphological features. Working with contours overlaid over the base-level surface map is easier than working only with a colored or greyscale map. Relief shading might be very useful, but one must consider the effects of illumination over linear terrain features (Grohmann, 2004; Smith and Wise, 2007). In the example of Fig. 4, the inflexion is marked by a compression of base-level lines, while in Fig. 3 the base-level anomaly is given by the abrupt deviation of contours. These areas would be interpreted as the most probable targets for field investigation and possible confirmation of structures whose recent tectonic activity left signals in landscape.

Therefore, results from a base-level map interpretation should be taken as *qualitative* ones (not quantitative) in the sense that they will not predict the exact location of tectonic structures, but would be used to guide and plan *in situ* investigations. It is particularly useful to optimize an otherwise costly, time-consuming field survey in large, heavily vegetated areas of poor accessibility, such as the study area.

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*Pg. 91, line 26: "provides"*

The text was corrected.

*Pg. 92, lines 13-16: What contour interval is chosen here? Or is this just a holdover terminology from the days of paper maps? Please clarify.*

The contour interval is taken from a topographic map at the same scale. This is not just a terminology from paper maps, but also an important factor when interpolating the base-level surface in a GIS environment. The following was included in the new section 2.1 'Construction and interpretation' in the text:

Construction of base-level maps in a GIS environment is fairly simple. From a DEM of the area of interest, one can derive the drainage network using several available algorithms, depending on the characteristics of the landscape and the spatial resolution of data source (e.g., Tarboton and Bras, 1991; Orlandini et al., 2003; Grimaldi et al., 2007; Nardi et al., 2008; Grimaldi et al., 2010). This drainage can be classified and the elevation points used to interpolate the base-level surface can be extracted by overlaying the desired stream orders with contours derived from the DEM. Using contours as a source for elevation (resembling the manual method) instead of using all the elevation values along the stream lines might sound as an unnecessary complication, but our experience shows it provides better results. As with any interpolation procedure, one must avoid clusters of data points and use, whenever possible, points with a uniform distribution over the target area in order to prevent interpolation artifacts (Chaplot et al., 2006; Berry, 1997; Yilmaz, 2007; Yue et al., 2007). Extracting the elevation

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from the intersection of streams and contours yields a smaller number of points to be interpolated, without clusters along streams.

*Pg. 93, lines 5-7: This seems a very roundabout way of identifying a normal fault. Simple stream profile analysis or even topographic swath profiles would give the same information (and might even allow the quantification of knickpoint retreat rates and fault throw).*

Please note that figure 4 is just an *example* on how the base-level surface would be. Similar block diagrams could be constructed for reverse or strike-slip faults as well.

*Pg. 93, lines 25-27: If the automatic processes are equivalent, then I would deem them more advantageous (and less subjective).*

Yes.

*Pg. 94, line 26 - Pg. 95, line 2: This seems like a very good application of the method.*

Thank you.

*Pg. 95, line 20-21: Sentence fragment. Also, please identify the source of the 1:1Million scale map.*

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The sentence was changed (below). The 1:1million maps are official Brazilian topographic maps.

The extracted drainage network were visually compared with available topographic maps and was considered to be compatible with a 1:1,000,000 scale.

*Pg. 96, line 6-10: If I understand this properly, one needs to know what the structures are before deciding which stream order to use. Above in the applications, the 3rd order map is considered the best.*

The idea of the passage (and figure 5) is to show that, in our opinion, maps produced with 2nd+3rd order streams provide the best results.

*Pg. 96, line 17: Be very cautious here. This method may be able to identify lineations, but it is certainly not capable of distinguishing between thrusts from normal faults.*

Yes, the method can identify lineaments (not lineations) but cannot distinguish between normal faults and thrusts. We mention the structure as a thrust from its correspondence with the geological map.

*Pg. 96, line18-19: This could equally be a difference in the rock properties.*

Yes it could.

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*Pg. 96-97, Results and Discussion: It is generally difficult to follow this text. References to a NNW-SSE trending thrust or NE-SW and NW-SE trends are helpful, but I would prefer to see these structures numbered on the figures and precisely identified in the text.*

We agree. We added numbers to the lineaments in the figures. Thank you for this suggestion.

*Pg. 97, line 21: Does this refer to the magnetic or gravity anomaly map?*

It refers to the lineament interpret from the base-level map.

*Pg. 97, line 21-25: I would hardly say that this has little topographic expression. The elevations from the original SRTM data jump by 400 meters from a flat plane to the northeast. If anything, feature is most obvious in the original topography (Fig 5A).*

If you consider the whole are, it might seem that way, but the topographic expression of this lineament is actually subtle. We changed figure 5A to a shaded relief image of the topography and it should give a better view of the landscape.

*Pg. 98, line 6-7: The identification of lineaments is rather woolly. The method used to draw the lines is not given and the lines themselves (Fig 6A) only partially correspond to known structures. If the goal of the paper is to prove the applicability of the method, then it seems to me that it should be capable of recreating the structures on the simplified geologic map (Fig 6B).*

The goal of the method is *not* to recreate structures already present in a geological map. Please refer back to the comment about *Pg. 92, lines13-16*: for a detailed explanation.

*Pg. 98, line7-9: More recent than what? This statement requires a brief introduction to the tectonic of the area.*

Agreed. A section on local geology and tectonics was included.

*Pg. 98, line 11-13: (and above in the Discussion section): Firstly, this method has not identified structures with little topographic expression (see above). Secondly, as a non-expert, I do not see the “good correlation” mentioned here. I would prefer to see some quantitative proof of correlation (e.g. correlation coefficients or a similar statistic).*

First, it has (see above). Second, we must say again that the lineaments interpreted do not mean to replicate those present in geological maps, but to provide one with a fair guide as to where there might be feature, so that one can plan a better field survey. There is absolutely no point in a statistical correlation about the ‘accuracy’ of such lineaments.

*Figure 6: Again, I am no expert in this field, but I struggle to see how the lines in Figure 6A were identified. If I were asked to draw lineaments on Fig 5B, I would end up with a somewhat different map. This step of the method needs to be better explained.*

We expanded the section on interpretation of base-level maps. Please refer back to

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the comment about *Pg. 91, lines 21-27* for a detailed explanation.

## J.-D. Champagnac (Referee)

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*Received and published: 20 January 2011*

*Grohmann et al. explain in this paper a method (that I was not aware of), namely base level mapping, that would help to extract geomorphic signature of existing tectonic structures, rock type contrasts, or any other morphological contrast. They explain clearly the method and some of its limitations, briefly show some examples of successful use of this method, and apply the method in northern Brazil, where, I guess, field mapping of active structure is not easy. I personally think that topography s.l. contains a lot of information, and I am always happy to see methods that aim to extract this information out of noise. However, without a detailed discussion on the tectonic regime and the age(s) of the deformation(s), coupled with a more detailed analysis of the “structures” and “anomalies” observed, it is hard for the reader familiar with fault mapping to 1) really believe that the mapping is univocal, and 2) to understand -in this specific case study- what are the insights given by the method (the “structures” detected are either already known, either not discussed in details.*

*S. Castelltort in his comment, suggests that the tectonics is active there. I don't know much about South America, but I doubt that any seismic or geodetic activity is recorded in this area. This raise two points: Authors should precise the tectonic activity of the region for reader not familiar with this area, and 2) if tectonic activity is slow / null, what is/are the possible origin(s) of the observed structures?*

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A new section on the geologic and tectonic context of the target area was included in the manuscript. This text gives a general idea of regional structure and explains that some of the structures recognized in base-level maps were previously known and some not yet. Thus base-level map allowed to enhance structures resulting either from fault reactivation or previously unreported or newly generated ones with recent tectonic activity along them.

*Detailed comments: Abstract The abstract present the findings described in the paper, but may (should?) be written with more details, avoiding general statements and vague formulations.*

The abstract was rewritten.

*Line15: why anomalies with no mapped structure would be more recent? Is the large scale tectonic setting supporting “more recent” (and how recent?) tectonic activity with surface expression?*

The concept of neotectonics only started being used in Brazil in the mid-1990's. Before that, geological mapping was focused more on Precambrian faults and shear zones. Therefore, if a lineament is interpreted and it does not have a correspondence with a mapped structure, there is a great probability that it represents a brittle, younger structure. We included this information on the text.

It is worth noting that until the mid-1990's, the concept of neotectonics was new to Brazilian geologists, and the continental shield was considered to be stable (Riccomini and Assumpção, 1999). The geological mapping of

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the whole country carried out at a 1:1,000,000 scale during the 1970's and 1980's (Radambrasil Project) paid more attention to Precambrian shear zones, and did not consider much the younger brittle structures in its maps. Thus, a lineament or structure interpreted on a morphostructural map that does not have a correspondence with a Precambrian shear zone is likely to be of neotectonic origin.

*Line 17-18: this sentence is too general. What kind of geophysical anomalies? What is a geophysical anomaly? How "good" is the correlation with the geophysical anomalies?*

The text was changed to indicate that the anomalies are considered as alignments of high or low values, or sudden changes in values. The correlation between the lineaments and the geophysical data is visual.

*Introduction The first part of the introduction acknowledges (a lot of) previous work, back in the past. The last part (beginning line 15 page 91) suggest a new formulation, and at line 21 (page 91), authors develop some general suggestion about how should be interpreted base level map. This section should be removed from the intro, and dropped into section 2. A quick introduction about the geomorphological / geological / tectonic setting of the studied area is missing in the Introduction.*

A section on the geological and tectonic context was included and the part on construction and interpretation of base-level maps was moved to section 2. Thank you for this suggestion, it improved the organization of the text.

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2. *base level maps - Author should acknowledge that stream order depends on the scale of the map DEM used.*

A sentence was added:

It should be noted that stream classification depends on the resolution of the DEM, that is, with higher spatial resolutions the drainage network will tend to be denser, with more low-order streams than with a lower resolution, so trunk rivers are more likely to present a higher Strahler order.

*section 2.2 application is a suite of several (7) studies that are using similar or approaching method. Either each subsection (each example) is too long or too short. Too long, I think, and the entire section could fit with a few concise sentences, or each example should be developed with a critical view of the detail of the method used, limitations, and results. I think that a section of more modern morphometric analysis is missing.*

Thank you for this suggestion, but we believe that this section is important and we prefer to keep it as it is.

3. *Method I am not sure that a single researcher on earth still uses manual detection of rivers and Strahler ordering. The first sentence is therefore probably not needed. However, SRTM resolution should be corrected (resolution is more ~900m than 1km). Reviewer#1 say that authors have used SRTM90m, which I think is not the case. This suggests that authors where not clear enough on this point. I do not understand the*

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sentence “The extracted drainage network is compatible with a 1:100000” (line 20 page 95). What compatibility the authors are talking about?

We have changed the text in order to make it more clear:

All data processing was carried out with GRASS-GIS version 6.4 (Neteler and Mitasova, 2008; GRASS Development Team, 2009). As topographic base, we used SRTM30\_PLUS V3 DEMs (Becker and Sandwell, 2007), with spatial resolution of 0°0' 30" (~900 m). Drainages were extracted using an A<sup>T</sup> least-cost search algorithm designed to minimize the impact of DEM data errors (Ehlschlaeger, 1989). This algorithm provides more accurate results in areas of low slope and also on DEMs where canopy top might be mistaken as ground elevation, such as SRTM (Kinner et al., 2005). Water flow was calculated using a multiple flow direction (MFD) method, where the water flow is distributed to all neighboring cells with lower elevation using slope towards these cells as a weighing factor for proportional distribution, a convergence factor of 5 as recommended by Holmgren (1994) and a minimum size of an exterior watershed basin of 25 cells. The extracted drainage network were visually compared with available topographic maps and was considered to be compatible with a 1:1,000,000 scale.

*4. Results and discussion Line 8 page 96, Authors should be more precise when they are talking about “structures” or “morphostructures”. What kind if structures? Faults? Folds? Bumps? Lithological contratsts? Etc. Again “anomalies” should be detailed /précised (height, wavelength, orientation etc.)*

In this case, “structures” are used in a loose way, since the method only indicates where

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there might be a structure worth investigating in the field. In the same way, “anomalies” are considered as alignments of high or low values, or sudden changes in values. We also changed the text so “lineaments” are used instead of “structures”.

*Line 16 (page 96). What is the “recent” tectonic activity of the studied area? How recent is “recent”? Line 20 (page 96). You should help the reader not familiar with the Barzilian geology, and precise the age of the Parnaiba Sedimentary Province, since some “structure” seem to be younger than these sediments.*

A section on the local geology and tectonics was included. We hope it will provide the necessary information to readers.

*Line 9 page 97, authors write that observed anomalies “can be interpreted as a SW dipping normal fault”, without discussion of regional tectonic and / or stress field. Moreover, this finding is not related to the method described in the paper (base level maps), but well-known and quite straightforward swath profile.*

The swath profile was used to illustrate the topographic pattern of the area where we interpreted a fault, not to identify the fault. It adds to the interpretation. An explanation on the stress field and structural style of the region was included:

Topographic swath profiles (or projected profiles, Baulig, 1926; Tricart and Cailleux, 1957) are those where intersections of contours with equally spaced profile lines are marked within a swath, or band. This kind of profile can provide a broader view of altimetric behavior, and help to determine inclination of large topographic features (Meis et al., 1982). Figure 7 shows a

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N-S swath profile constructed in a band of  $2^\circ$  with a 10' interval between individual profiles. There is a general trend of lowering the elevation towards north, which is interrupted at about halfway the profile length by a strong increase in elevation and subsequent gradual decrease.

The general topographic pattern admits different interpretations: a southward-dipping normal fault; a northward dipping low-angle reverse fault or even an anticline growth in the northern portion of the profile. A reverse fault is unlikely since the elevated area in the central sector of the profile would have to be an abnormally-large preserved relief in the hanging wall. A compressive style of deformation is not expected in the intracratonic extensional regime of the Grajaú Basin. Therefore, the most likely interpretation for this structure is an E-W-oriented southward-dipping normal fault (lower right inset in Fig. 7), which allows a best fit to the observed boundary between the Grajaú Basin and the Araguaia Belt. This would imply in a N-S direction of extension, in accordance with available seismic data (Assumpção et al., 1985). Given that the abrupt change of elevation in the swath profile correspond to the E-W inflexion of the Tocantins river, this adds to the hypothesis of a major drainage capture in the lower Tocantins and of tectonic influence in the landform configuration of the study area.

*The end of this section briefly argue that anomalies observed in base level maps “correspond” to anomalies observed in geophysical maps, without a complete discussion. This is way too short.*

We expanded the discussion on the text.

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5. *conclusion This section is just an abstract of the section 4. I suggest merging discussion and conclusion, and using available space to develop points that are treated too quickly.*

Thank you for this suggestion, but we believe that the concluding remarks are valid and we prefer to keep them as they are.

*Figures: Figure 1: I suggest the use of a DEM (color figures are not expensive in a online-only paper...), that would give much more information than a simple hydro-graphic map.*

We agree. Thank you for this suggestion.

*Figure 3 is too small. It is a key figure of this manuscript, and should be easy to read.*

This is an important point that was also commented by others. In fact, all the figures were prepared considering the final publication (HESS) which is a page size of 8.27 x 10.91 inches. Figure 3, for instance is supposed to be a whole-page figure. The figures appear to be too small because the page size of HESSD is 8.50 x 6.26 inches, so the figure must be adjusted to this size. We would like to make a constructive criticism to HESSD here: following the example of other high-prestige journals, it would be better for referees if there was a link in the pdf file for downloading a high-resolution version of the figure. This would prevent any issues about figure size.

*Figure 6 : typo: 24nd instead of 2nd order in the caption. I have a major concern with*

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*this figure: I don't understand how these "structures" are deduced from fig 5B. Are they faults ? strike slipe? Why are they cross cutting to each other without offset? I feel that this figure is highly speculative, and I suggest that author give Fig5B to someone that don't know anything about Brazilian geology (a structural geology which has training in fault mapping, for instance), and ask him (her) to draw "structure" on fig5B or 5C. I doubt that he (she) will provide a map similar to Fig6A.*

We must say again that the lineaments interpreted do not mean to replicate those present in geological maps, but to provide one with a fair guide as to where there might be feature, so that one can plan a better field survey. Additionally, the base-level maps are presented in a very small scale and the lineaments are in thick lines. In such small scale, horizontal displacements under tens of kilometers cannot be properly represented. Please refer to previous comments on interpretation of base-level maps.

*Figure 7: the inset is too small (draw the swath on Fig 1 or Fig 5A. I fear that the interpretation by a normal fault is no univocal: the same swath profile may be obtained with other tectonic structures (N-dipping reverse fault, anticline growth north to the northern part of the profile...)*

Please see the comment on J.-D. Champagnac question about *Line 9 page 97* for a complete explanation. As for the inset map, we expanded it a bit.

*Figure 8: It is hard to see on this figure where are the structures mapped in Fig6A. I suggest a composite figure that would help the reader to better understand your points.*

Yes, thank you for pointing this. We have numbered the interpreted lineaments in figure

6 and we split figure 8 into two figures, one for the magnetic data (fig.8) and one for the gravimetric data (fig.9) In both figure 8 and 9 we provide one sub-figure with a overlay of the base-level contours over the geophysical data and one with the lineaments.

### Anonymous Referee #3

*Received and published: 30 January 2011*

*This short paper has for objective the "evaluation of the method's applicability (i.e. base level maps) in regional-scale analysis (e.g., 1:250 000 or smaller)" and uses a test area in Northern Brazil. The point of this method is simply to "filter" topographic maps in order to detect, if possible, features otherwise hidden by the complexity of the topography. I think that the papers style is clearly to communicate, attract attention towards, an already existing methodology made easier by recent GIS developments. Nothing more. The authors never intend to resolve the problem of how strain is recorded in drainage networks, nor do they intend to explain brazilian geology through their analysis. Overall the paper is well written and does a good job in calling attention to this method. Therefore I think the paper can be published almost as is, only after minor technical corrections of typos and answering to some of the precisions required by reviewers#1 and 2 with regard to the origin of maps or the scale of the DEM used and so on.*

The comments of the referee correspond to the central points of our paper. Thank you for generous comments. The questions of referees #1 and #2 are answered in the respective sections.

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*Received and published: 2 February 2011*

*The manuscript describes a base-level map definition method based on Strahler classification of stream network. The manuscript is pleasant to read and the topic is interesting. This comment is limited to the Digital Elevation Model pre-processing section, that, in my opinion, is not enough described and discussed by the authors. Indeed, in section 3, authors spend only few lines to explain the used approach to extract the drainage network. Precisely they mention:- the Ehlschlaeger's method for the drainage extraction- the MFD as flowdirection method. Since the base level method applied in the paper is related to the DEM pre-processing procedure I would suggest to include more details on this point and to justify why some specific methods are preferred to other ones. For instance, the MFD, in my experience, is more appropriate for hillslope areas and not for channel identification. In literature there are many contributions that investigate on flowdirection methods, flat areas issue and drainage network extraction, and it is clear that the choice of a combination of these methods could affect also the watershed Strahler classification.*

*As an example in: Nardi F., Grimaldi S., Santini M., Petroselli A., Ubertini L., Hydrogeomorphic properties of simulated drainage patterns using digital elevation models: the flat area issue, Hydrological Science Journal, 53 (6), 1176-1193, 2008, the table 4 (page 11) shows a comparison among three approach combinations. The result is that using different combinations either the drainage network shape and the watershed maximum order could vary. In our experience the best procedure for drainage network extraction is the following*

[1] :

- 1) application of PEM4PIT [2] for artificial depressions and flat areas removal;
- 2) estimation of the flow directions using the D8-LTD algorithm [3];

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3) *stream network automatic extraction using the curvature-based scheme [4], in conjunction with the automated constant drop analysis algorithm for the identification of the channel initiation threshold [5];*

[1] Grimaldi S., Petroselli A., Alonso G., Nardi F., *Flow time estimation with variable hill-slope velocity in ungauged basins, Advances in Water Resources, 33 (10), 216-1223, 2010.*

[2] Grimaldi, S., Nardi, F., Di Benedetto, F., Istanbuluoglu, E., Bras, R. L., 2007. *A physically based method for removing pits in digital elevation models. Advances in Water Resources 30, 345 2151-2158.*

[3] Orlandini, S., Moretti, G., Franchini, M., Aldighieri, B., Testa, B., 2003. *Path-based methods for the determination of nondispersive drainage directions in grid-based digital elevation models. Water Resour. Res. 39(6), 1144, doi:10.1029/2002WR001639.*

[4] Tarboton, D. G., Ames, D. P., 2001. *Advances in the mapping of flow networks from digital elevation data. In: World Water and Environmental Resources Congress, Orlando, Florida, May 20-24, ASCE.*

[5] Tarboton, D. G., Bras, R. L., Rodriguez-Iturbe, I., 1991. *On the extraction of channel networks from digital elevation data. Hydrol. Processes 5(1), 81-100.*

*So, since the manuscript results could be affected by the applied pre-processing method, authors should mention that there is a well developed literature about this step and explain why they preferred the chosen approach.*

We have been using GRASS (and the  $A^T$  method) for a long time and our experience shows good results for drainage extraction in Brazil. The literature cited is indeed very interesting and we included a passage about the existence of several methods for drainage extraction in the text, and that the user should choose among them depending on the DEM available and the characteristics of the study area. We are not sure, though, on how different (if any) the drainage network would be at this scale, even with such diverse methods.

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Please refer to previous comments about *Pg. 92, lines 13-16*:

## M. Jaboyedoff (Referee)

micHEL.jaboyedoff@unil.ch

*Received and published: 12 February 2011*

*This paper is dedicated to the concept of the base level used to extract the tectonic feature from landscape using DEM. The proposed method is applied to a large region of Brazil. The changes suggested by all reviewers and myself will greatly improve the paper, but I recommended the authors add more info on some references, which will make the text easier to read.*

*General comments The paper is well written. The authors make a good overview of the base level concept in the first part of the paper, but they do not really go into detail about the method used to perform a base level using DEM. In the method section more detail have to be given about how they extract the base level surface even if there is a reference. For instance AT least-cost algorithm must be described shortly way in the paper.*

We expand the text about the construction of base-level maps, in order to make it clearer. Thank you for pointing that out. As for the drainage extraction algorithm, we feel that this is not the place to discuss it, given that there is a extensive literature on drainage extraction. Also refer to the response to S. Grimaldi.

*On the whole, I am convinced by the base level concept used to analyze topography, but I am not convinced with the treatment of small scale (DEM 1km), because abrupt*

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*changes are not captured. In that case you will get more mantle effect on topography than really faults. I prefer local use for base level to analyze topography, as it is suggested in the last figure. In addition, Strahler is not the good classification for morphometry. I cannot go further because I have a paper on that topic, which has been rejected for 5 years.*

We have been using base-level maps for several years, and we are convinced of its applicability on small scale. We are sorry to hear about your paper, because it sure would be an interesting contribution, but we cannot comment on it.

*The main issues are linked to the discussion's section, because the figures do not really support the text. Each map in figure 5 must contain the river courses, in order to see the link between base level and the surface.*

Thank you for this suggestion. We included the drainages in the maps and it improved their readability.

*Figure 6 can probably be improved by overlapping both the 2nd (and not 24nd as displayed in the caption) and 3rd contours with a hillshade draping the geology, this will improve the readability of the maps.*

The typo was corrected. Although the suggestion is interesting, we prefer to keep the figure without the shaded geology.

*Figure 8 can be transformed in a pseudo 3D profile using the 3D representation using*

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sections; this will permit to see the fault scarp, if it exists, maybe it is more a limit of the extension zone, and more an effect of the mantle than a unique fault. Figure 8 needs more explanation.

We believe you are referring to figure 7 (swath profile). We expanded the text about this figure to make it clearer. Please refer to previous comments on this issue.

*Specific and technical comments I have no direct specific comments because my comments are requests for adding information. In addition, the reviewer (RC, C5) has already made good detailed comments.*

Thank you very much for your critics and suggestions.

## **Anonymous Referee #5**

*Received and published: 7 March 2011*

*GENERAL COMMENTS. Overall, article is interesting, with a new use of previous development methodologies in the morphometric analysis of landscape. Structure of the article must be modified for a better use of the paper extension. A significant reduction in the extension of chapters 1 and 2 should be useful for a better development of further chapters. In general, figures are small, so the visualization of data results are not very easy. General evaluation is accepted with major revisions.*

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Please refer to previous comments about the figures issue.

*SPECIFIC COMMENTS. Chapter 1 - Introduction. Chapter should be resumed. In Example, paragraph from line 15 to 20 do not provide essential information for the paper understanding. In the same way, paragraph from line 28 to 33 can be significantly resumed.*

Thank you for your suggestion but we prefer to keep the text as it is.

*Chapter 2.1 – Construction. This chapter should be resumed. First paragraph from line 7 to 12 could be replaced for a reference.*

We actually expanded this section, but thank you for your suggestion.

*Chapter 2.2 – Applications. In order to the total length of the article, the space used for this chapter is out of order. The systematic ordering of studies where this kind of methodology has been used could be useful in a methodologist focused paper. Not in this case. After all, all this information may be useful, so a list of references with an introduction in the way of: “This methodology has been used with optimal results in many different areas, like . . .”.*

Thank you for your suggestion but we prefer to keep the text as it is. We believe this section is important for a better understanding on how the method has been applied recently.

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*Chapter 3 – Methods. First paragraph from line 4 to 8 may be deleted.*

We do not agree. We believe that the paragraph indicates the advantages of the GIS-based method.

*Chapter 4 – Results and discussion. At first time, this section should have a longer extension, because is the focus of the paper, and any further discussion or idea obtained from the paper should be well developed and referenced at this chapter.*

We expanded this section. Please refer to previous comments for a full explanation.

*In addition, a short explanation of the base-level shape used in the lineaments identification and delineation will be useful. To support the first paragraph, from line 6 to 10, a figure of the DTM derived drainage network should be showed; Strahler s ordering of the network will be useful information to.*

The section on interpretation of base-level maps was expanded to make it more clear. We considered including the figure of the DEM-derived drainage but we think it is not necessary, but thank you for your suggestion.

*In third paragraph, from line 16 to 20, authors write: “Although none of these mapped structures have ever been connected to recent tectonic events, we must note that some of them, such as the NNW-SSE-trending thrust north of the rivers major inflexion (Fig. 6a),”. These lineaments are not present in figure 6a, or only one, so the direction of this sentence may be doubt.*

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That particular structure is the thrust at the contact between the Grajaú Basin and the Archean basement. We numbered all lineaments and now the text has references to those number, so now it is easier to know to which lineament the text refers to.

*In paragraph from line 21 to 26, authors talk about fluvial capture based on the orientation of the base-level lines. The affirmation may be true, and other studies over the area are mentioned in the text. Anyway, more data about the area, and new studies should be carried out for the verification of the proposed idea. In fact, the Grajau river development is more important that the Gurupí ones, so it is possible too, that the fluvial capture of Tocantins fluvial system (flowing before to the Grajau river complex) promote the development of Gurupí. As I mentioned before, new studies area necessary in that area.*

We agree completely that more studies are necessary in the area. The idea that the inflexion of the rivers are due to a capture (caused by the E-W structure?) is exactly that, an idea, an hypothesis that still needs confirmation.

*In paragraph from line 13 to 20 (page 97), authors use a new data (geophysical data) that have not been mentioned before. The use of this geophysical data must be mentioned previously. Anyway, results interpretations of the authors are very difficult to check, because the overlap of the data results is absolutely essential. If this overlap process is not makes, all interpretations are difficult to evaluate. To solve this disadvantage the size of the figures does not have a positive effect. Finally, author's affirmations of last paragraph (from line 21 to 25) have the same disadvantage pointed in the previous paragraph, which is pointed out the importance of the data results overlapping.*

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We agree. We included a mention about the geophysical data in the text and changed the layout of figure 8 into two new figures. Please see the last comment from J.-D. Champagnac about this same issue.

*Chapter 5 – Conclusions. All this chapter is structured on the base of the interpretation of results of the base-level analysis, but in the referee opinion, all these interpretations will be easy to follow, if the authors make a overlap of data. Lineaments vs. Geology and lineaments vs. Geophysical data are essential figures.*

Thank you very much for you suggestions, they helped to greatly improve the paper.

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