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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #1

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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 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 confounded 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 = KQˆmSˆn (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 Sr = S*(Ar/A)ˆ-m/n (Sklar and Dietrich, 1998, or the channel steepness ks where S = ksAˆ-θ (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 perturbations; 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 identifiably, but not how. I would like to see a section describing this in detail. Further detailed comments can be found below.

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.

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?

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

Pg. 90, line 17: "presents"

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.

Pg. 91, line 14: "lithologically"

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

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 map.

Pg. 91, line 26: "provides"

Pg. 92, lines13-16: What contour interval is chosen here? Or is this just a holdover terminology from the days of paper maps? Please clarify.

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).

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

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

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

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.

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.

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

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.

Pg. 97, line 21: Does this refer to the magnetic or gravity anomaly 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 $\sim$400 meters from a flat plane to the northeast. If anything, feature is most obvious in the original topography (Fig 5A).

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).

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

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).

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 89, 2011.