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Hydrology and Earth System Sciences Discussions

Interactive comment on "Curvature distribution within hillslopes and catchments and its effect on the hydrological response" *by* P. W. Bogaart and P. A. Troch

Anonymous Referee #1

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Review of Curvature distribution within hillslopes and catchments and its effect on the hydrological response by P. W. Bogaart, P. A. Troch

This is an interesting paper. The main contribution to me is the clever way to compute flowpath lengths. Another interesting point is the convergence paradaox (although this has been illustrated more clearly on the authors'; poster at the EGU meeting). Below I list some comments which I hope can improve the paper.

The two contributions mentioned above seem not be linked in the paper. What I am missing is something like a correlation between flowpath length (distributions) and hill-slope shapes as quantified by curvature.

p.1082 ff. I agree that computing the full distribution of flow pathways is not possible for large DEMs. On the other hand, using just the mean (first moment) might provide limited information. I would recommend the authors to compute the real distributions for a limited number of cases and to investigate the shape of the distributions and whether this shape is similar for different topographic settings.

On multiple-flow-direction routing methods: p. 1082, 2nd paragraph: The algorithm suggested by Tarboton (1997) is actually no multiple flow algorithm. One steepest direction is computed and the flow is then divided among the two cells around this one steepest direction. This also means that this algorithm is as divergent on on divergent hillslopes as on convergent hillslopes. p. 1082, 3rd paragraph: "The proven advantages ", can we really say that the advantages are proven? These methods certainly make more sense than the simple D8, but there is hardly any 'proof'; in the literature. Sörensen et al. (2006, HESS, www.hydrol-earth-syst-sci.net/10/101/2006/) recently tested different algorithms against field observations but results were somewhat ambiguous.

On the effect of divergent/convergent hillslopes: The results on pages 1089/90 and figures 8&9 are a bit surprisning and, I have to say, also disappointing. From these results one might draw the conclusion that hillope shape does hardly matter, convergent and divergent hillslopes differ somewhat but when averaging the results are the same as for straight hillslopes. These results should be discussed more as they question much of what has been said in the paper before. I would like to agree with the authors that shapes matter, but this has to be shown! One suggestion would be to look on shorter events. I would also assume that differences might show up more clearly if one looks on where the runoff is coming from and residence times.

How has the channel network been determined? Is it a real network or one derived from DEM? Results might depend on the method used for this.

The section discussion & conclusion is mainly a summary. I do not think such a sum-

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mary is needed here; rather I would like to see a real discussion and an outlook on how the results might be used in the future.

There are a few typos in the text; more important I feel that the language could be improved to make the text more readable. I, thus, would recommend the authors to seek help from a native speaker.

A few minor comments:

p. 1073, third paragraph, ”As a result ...”. Even with these constrains other runoff generation processes than saturation excess are possible.

p.1077, Eq(4). No correct equation, Should be something like V = U L. While it says volume in the line above, the units are Lˆ2/T (i.e. no volume)

p. 1082, last line. The approach to sort all cells will get inefficient for large DEMs

Figure 9. in the text 1800 should be 180

PS: I apologize for my late comment to this interesting paper

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1071, 2006.