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Interactive Comment

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

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C: The reviewer remarks that the ’convergence paradox’ topic is illustrated more clearly on our EGU 2006 poster.

R: We have checked the poster (available online: http://webdocs.dow.wur.nl/internet/hwm/patrick/EGU2006/convergence-poster.pdf) but could not find material that is not in the paper.

C: The reviewer misses a correlation between flowpath length (distribution) and hillslope shapes as quantified by curvature.

R: The reviewer is correct that no explicit link between both measures of flow convergence was made. We have now inserted a paragraph that discusses this matter into the Discussion part of the revised manuscript: "It should be noted that topographic convergence/divergence ... beyond the scope of this paper."

C: The reviewer recommends computing the full distribution of flow path length.

H: In the original manuscript, we wrote that computing flow path length variability as obtained by our multiple flow direction algorithm is ‘infeasible’ (p. 1082). Since we wrote this, we found an alternative way for computing the full distribution, which is demanding but doable for the case where flow distributed among only two lower neighbours (what we labeled as ‘MFD-T’, but still infeasible for the case where flow is distributed along all lower neighbours (our ‘MFD-Q’). Preliminary analysis of these results, although interesting, yielded no conclusions that are pertinent to the current study. Therefore, in the revised manuscript, we now changed ’infeasible’ to ’highly demanding’, and choose to not discuss the higher moments. Probably, that will be the subject of a dedicated paper.

C: The reviewer comments that the Tarboton algorithm is actually not a multiple-flow algorithm in the strict sense. The reviewer further doubts if the superiority of multidirectional redistribution methods the unidirectional method is ’proven’.

R: We agree that this is the case, although there is some proof (a powerpoint presentation) that David Tarboton himself labels his own algorithm as a special kind of "multiple flow direction". special kind). In the revised manuscript, we have replaced "multiple flow direction" by "multidirectional flow redistribution".

We have also inserted a number of references to papers in which the superiority of the MFR methods is shown. We have, however replaced ’proven’ by a weaker predicate.

C: The reviewer remarks that our figures 8 and 9 suggest that "hillslope shape does hardly matter, convergent and divergent hillslopes differ somewhat but when averaging the results are the same as for straight hillslopes", and that "These results [...] question much of what has been said in the paper before".

R: The reviewer is right in that convergent and divergent hillslopes combined behave like an equivalent straight hillslope. In the original manuscript, we have discussed this result mainly in terms of intra-hillslope convergence variability. However, in the very last paragraph of the paper, we did mention an extrapolation of this effect to the catchment scale.

In the revised manuscript, we have now rewritten this part of the discussion, emphasizing the effect of compensation versus catchment-scale net shape.

C: The reviewer wonders how the channel network has been determined.

R: In the revised manuscript, we have now included our definition: channels are those DEM pixels with an upstream contributing area of $>$ 10,000 mˆ3, based on published maps and our own slope-area analysis.

C: The reviewer remarks that our discussion/conclusions section is really a summary. The reviewer would like to see a more extended discussion here.

R: In the revised manuscript, we have added or rewritten several paragraphs of the discussion section.

C: The reviewer mentions that our constraints (p. 1073) do leave room for other runoff processes.

R: The reviewer does not mention any particular type of alternative runoff process, making it difficult to give a proper reply here. In the revised manuscript, we have now explicitly mentioned infiltration excess runoff, since this is alongside with saturation excess and subsurface stormflow one of the 'classic' runoff mechanisms.

C: The reviewer mentions that Equation 4 is incorrect.

R: In the revised manuscript, this is corrected.

C: The reviewer mentions that sorting DEM grid cells by elevation will be inefficient for large DEMs.

R: We do not agree with the reviewer on this issue. There is no reason to assume that sorting will be 'inefficient'. Sorting a 1000 * 1000 grid typically takes less than 1 second. On the contrary: sorting is the key to the speed of the algorithm, because the 'solution' spreads uphill from the channel network. In the revised manuscript we have now stated this explicitly.

C: The number "1800" in the text should be "180".

R: In the revised manuscript, we have now fixed this.

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