Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-409-RC2, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "A history of TOPMODEL" *by* Keith J. Beven et al.

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Received and published: 10 September 2020

I enjoyed reading this paper and found it both interesting and informative. It is different from the papers that I am used to reading because it reads more as a careful and balanced reflection on a model rather than a report of new findings. But I think it is valuable and will be a useful resource to those who use or are considering using Topmodel in the future as well as those who must make sense of its predictions. I have four major comments, none of which are critical to publication but all of which I feel would improve the paper. There are then minor comments and suggestions most of which are either typos or suggested rewording in the attached pdf.

Major comments

Assumption A1

The assumption that "that the storage for any given value of Sbar is configured as if it was at a steady state with a steady homogeneous recharge rate (L88)", and its implications comes up in three different places within the article. It is an important point because it relates to a central assumption and one of the primary perceived weaknesses of Topmodel. I found this discussion particularly helpful in my thinking on Topmodel but I also found it confusing in places.

On my first read through I felt the first discussion of A1 on L88 didn't give enough detail. In particular I was confused by the language around "configurations" and "configured as if". I didn't understand how Sbar could be configured as if it was at steady state (L88) nor how configurations are dependent on storage (L89) nor how the two ideas related to one another. Did this mean that Sbar is varying only slowly in time? How slowly does it need to be? What controls the sensitivity of Sbar to rainfall and what is the sensitivity of the saturated zones to Sbar?

The later treatment of the assumption (L320) is more detailed and I understood this section better. It might be enough just to point to the later section at L89 for more detail. In this L320 paragraph I still struggled to understand what you meant by configuration. I understood it to mean that: 'the two-dimensional phreatic surface over the flow strip is that which would result from steady recharge over that flow strip'. However I wasn't confident in my understanding so clarifying this would be helpful. The main outstanding question for me at the end of the paragraph was: how close to 'as if' is near enough? You mention this with reference to Kirkby (1997) but a more complete restatement of his examination and findings would be useful here.

Assumption A1 is revisited on L755, and I found this the clearest expression of the steady state assumption within the paper. It may be that the other sections had laid the groundwork but I think you should consider re-stating this expression earlier in the paper.

Assumption A2

Topmodel uses tan(beta) to calculate lateral subsurface flux (L49 and equation 1). Others, usually modelling steep landscapes, have used sin(beta) to make the same calculation (e.g. Montgomery and Dietrich, 1994; 2002; Borga et al., 2002; Chirico et al., 2003). In some cases they explicitly claim that there is a choice between “the original ln(A/tan(beta)) or the more physically correct ln(A/sin(beta))” (Montgomery and Dietrich, 2002, p2). It might be helpful to respond to this claim, perhaps explaining why the difference, whether you consider one more physically correct than the other and if so what the implications are for situations in which they can or should be applied.

Assumption A3

It would be useful to have a longer discussion of whether the exponential transmissivity function is an assumption introduced by the authors (as is suggested L91-2) or one that is required within the derivation (as Kirkby (1997) seems to suggest). There are clearly advantages to being able to use alternative transmissivity functions, so it would be useful to know more about any possible disadvantages. It would be particularly useful to comment on how this might impact the validity of other model assumptions (e.g. L413) and the sensitivity to these assumptions (e.g. L360-2)

You do touch on this at L413 “might also preclude. . .” however, you say might rather than would and I am not clear what you mean by “implicit redistribution of subsurface storage”. Do you mean that A1 would not be consistent with non-exponential transmissivity functions? Kirkby (1997) seems to argue that the choice of an exponential transmissivity function is required to satisfy the integration (though I could have misunderstood Kirkby here). Do the authors of this paper find that argument convincing? If so what does it mean for the alternative profiles (e.g. Ambrose et al., 1996)? If not then where do you differ from Kirkby (1997)?

Connectivity and run on

The argument that small channels may connect apparently disconnected saturated areas (L350) is not clear to me. In particular mention of small channels at the start of

the sentence seems to contradict the end of the sentence. If I understand what you mean here, I think it might be clearer to talk about geomorphic / landscape evolution controls on where channels begin (e.g. Montgomery and Dietrich, 1988). The places where this will break down are those where some other landscape property gets in the way e.g. lithology and rock strength in parts of the Yorkshire Dales. If instead this is a suggestion that the majority of run-on passes from patch to patch and reaches the river as overland flow, then I think more support for the argument is needed. I haven’t seen anyone demonstrate this.

References

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Please also note the supplement to this comment:
https://hess.copernicus.org/preprints/hess-2020-409/hess-2020-409-RC2-supplement.pdf

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