E-mail discussion between Keith Beven and Makoto Tani on Tani’ s paper ‘’Reevaluating the runoff mechanisms of small mountainous catchments by applying a new storm-runoff model based on the vertical unsaturated flow.

(1) Tani- > Beven: February 2, 2024

Dear Keith,

This is Tani., Kyoto Univ. Thank you for your comments on my paper in Tani et al., (2020).

Your point might be that soil layer and weathered bedrock in natural slopes have various kinds of pathways with unique hydraulic properties and that it is unacceptable to represent them based on water movement in the soil matrix described by Darcy's law.

I did not agree with that idea, so I developed a runoff model based on the issue discussed in Tani et al. (2020) that the physical basis for a runoff-storage relationship model is vertical unsaturated flow. The original of my new paper was in Japanese, so I translated it into English myself (Tani, 2023). As I wrote in the introduction of this paper, I believe that the history of runoff process development in hydrology has been a process of seeking consistency between the complex runoff mechanism and the simple runoff model represented by a tank for example.

In the mountainous headwaters of Japan, where rainfall is heavy, the magnitude of storm runoff is frequently comparable to the magnitude of rainfall. In such cases, the entire catchment, including the ridge area, has to become a stormflow-contribution area, and all the various hydrologic pathways contribute to the storm runoff. It is a question why the runoff response to rainfall can be well simulated with runoff-storage relationship model. That is my question that I have deeply considered for long years. The hints are included in the paper. I hope you will reconsider the consistency between runoff models and runoff mechanisms.

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(2) Beven -> Tani: February 7, 2024

Dear Tani,

I have read through the papers and I really do not disagree with your perceptual analysis except in some points of detail and you give a nice summary pf the history from a Japanese perspective (some of that still poorly known further east).

In fact, the perceptual model does not differ that much from the kinematic wave solutions for the vertical unsaturated flow (VUF) and downslope saturated flow (DSF) that I provide in Beven (1981) and Beven (1982), in that case for a simple impermeable slope to allow analytical solutions. Those papers also allowed the timescales of VUF and DSF to be evaluated (and the timescales for DSF can be significant).

A few specific points

1. You cite Hewlett and the Horton and Hawkins paper. What Hewlett did not remark on in his use

of those results is that the Horton and Hawkins wetting front took 3 weeks to travel through their column. The extension to storm responses was not therefore really justified.

2. But we do think that something like that happens – even if we do not expect complete displacement of all stored water to form the leading edge of a wetting front. But the celerity of VUF is limited by that wetting front – any response ahead of it can only be small in volume (and as noted above the VUF arriving at depth still needs to move downslope – even if downslope celerities will be fast in your complete wet condition since storage deficits above any saturated zone(s) will be small – actually celerities might then DECREASE if the soil becomes saturated and there is overland flow) (McDonnell and Beven, .2014).
3. Your equations (1) and (2) are a kinematic wave model – which has the advantage that you do not actually have to specify anything about the pore space and flow pathways, or assume Richards-type flows). Clearly, the deeper the system, the more important the time scale of the VUF will be relative to the DSF (and it there is some form of confinement of the saturation at depth the celerities can be very fast indeed, possibly the case in the fractures of the Coos Bay Oregon which I have visited)
4. As expressed in the Beven and Germann (2013) paper, I do not find the dual continuum Richards approach at all satisfactory. Richards itself is based on the wrong experiment by excluding the possibility of flow naturally in larger pores, and macropores really do not act as coarse matrix. The important thing that is missing in your analysis is that the macropores are not generally continuous in forest soils. This has an important consequence that there will be a build-up of saturation at any dead-ends in wet conditions, leading to displacement of stored water into macropores below or downslope (it also means that dye tracing might not indicate all macropore pathways). Pressure propagation can still be fast in that case. This mechanism has not been sufficiently studied but we were able to mimic such behaviour in the multiple interacting pathways (MIPs) kinematic model (e.g. Davies et al., 2013)
5. In fact, you approach is not so different to a variable soil depth Topmodel in its original topographic index form which assumed time step by time step integration of the vertical fluxes (albeit based on a storage function rather than Richards) and resulting distribution of saturated areas (there was also a version that allowed K_s to vary as a statistical distribution). That rapid time step redistribution of the saturated zone relies on the rapid celerities expected in wet soils, of course – but is not really supported by the time scales of the kinematic solutions mentioned earlier.

Thanks for sending the paper – it was good to think about hillslope hydrology again!!

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(3) Tani- > Beven: February 2, 2024

Dear Keith,

Thank you very much for your prompt reading of my paper and your kind comments.

First of all, I apologize for publishing my paper in a format that puts it on my personal website because the original was written in Japanese. I am in the process of receiving confirmation from the copyright holder and I will be happy if you inform colleague and friends of the paper on my website after the permission is obtained. This is because I believe the discussion on my paper could be useful both for runoff-process studies and for modelling studies.

1. As shown in Takeshi Ohta's artificial rainfall experiment in Figure 12 in Ohta (1983), when the soil layer is completely wetted, the time variability of the rainfall is quickly transmitted to the slope outlet. I think this kind of result is found all in experiment on a natural slope with heterogeneous soil, experiment on an artificial homogeneous soil slope, and calculation using the Richards equation.

   All I am trying to say is only that "it is obvious that there are various runoff pathways of hydraulic mechanisms within the natural soil layer, but even so, once the soil layer is completely wetted, changes in rainfall intensity are quickly transmitted to deeper positions in the soil layer, and the results are apparently little different from those calculated using the Richards equation.
2. Another thing I would like to point out is that one of the key objectives of physically-based runoff model development is traditionally to evaluate the effect of catchment properties on runoff, not just rainfall conditions, and that is why your Topmodel also played an important role toward that end, As shown in Figure 3, my model structure ignores DSF and is comically simple, but it claims that soil layer thickness and soil physical properties (ψ-θ and ψ-K) affect storm runoff responses. As a case in point, this paper attempts to explain the differences in runoff characteristics between KT and MN in TY by differences in soil layer thickness. The effects of soil physical properties shown in Figure 7 and Table 2 are confirmed by the simulation results of hydrographs in TY, MR, and KI.

I understand that it is very difficult to assess the sensitivities of each of the catchment properties on the runoff responses. This is because the spatial distribution of each is so heterogeneous that I am not sure how to treat them appropriately. However, I believe that it is important to study the effects of catchment conditions on runoff responses. Certainly, it is very difficult to ensure consistency between the runoff model and the runoff mechanism. That is why I believe that hydrologists must make every effort to approach that consistency.

Again, thank you for your valuable comments. I really hope young hydrologists will understand our discussion.

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(4) Beven -> Tani: February 8, 2024

Thanks Tani

I have no problem with rapid transmission of pressures under wet conditions. That is when the celerities should be at their fastest and the result should be the displacement of stored water. I also have no problems about the importance of the distributions of soil depths and bedrock permeabilities in shaping the response under very wet conditions. As I said, our perceptual models are very similar.

The question of whether the Richards equation is appropriate under such conditions at profile or hillslope is still, to my mind, more problematic. That is because under wet conditions any soil characteristics will be at their most nonlinear, even if the equilibration of pressures (as needed for a continuum approach) will be faster. But if there is underlying heterogeneity, then the gradients and conductivities do not average linearly – which is to say that some more complex equation that allows for the heterogeneity should be used. That applies even if Richards applies locally (and again I think that it was derived from the wrong experiment, so that is not the correct physics - I made this point in Beven (1989).

But, of course, that does not mean that a Richards equation model cannot mimic the behaviour seen in the field – as a "lumped conceptual" model structure) even if the physics is not correct. It could be one of many ways of mimicking the behaviour (see again our MIPs model) that will perhaps converge as the soil gets close to saturation and the fast celerities mean that outputs follow inputs.

My PhD back in 1975 was all about trying to use a finite element Richards equation model to evaluate the effects of catchment properties of runoff. In that case it failed when compared with field data, in part because it ignored the heterogeneities and preferential flows observed in the field (a story told in Beven, 2001: Dalton lecture paper). That experience certainly shaped my later work and made me think about the nature of the physics more deeply.

I think the critical question to think about is how to assess the distribution of celerities in the system (for both heterogeneous soils on convergent/divergent hillslopes with bedrocks that are not necessarily impermeable. Assessing velocities is already difficult (tracers help of course) but we have not really looked at all at how celerities change with wetting and drying. They will be implied by the hysteresis in the response in different conditions and I have tried to get a number of groups to look at how hysteresis changes with antecedent catchments in small zero/first order catchments using machine learning to try to understand the controls. But nobody has yet taken the idea seriously.

Do you know of any data sets in Japan where there is both good hydrological AND high frequency isotope data? It would be good to have more data sets to work with but the isotope time series are rare!

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(4) Tani -> Beven: February 9, 2024

Thank you again for your comments.

I think the issue is becoming more solid. From the looks of it, I thought we had similar ideas in terms of the rapidity of celerity in wet soils and the importance about the influence of the distribution of water and sparing on the runoff response of soil layer and weathered bedrock.

However, you argue that the physical mechanisms that occur both in homogeneous permeable media and heterogeneous permeable media with various flow paths cannot be described by the Richards equation. On the other hand, I have a pragmatic view that the various properties contained in the subsurface structure of the slope must affect the runoff response, and as long as these influences can be evaluated, the physical mechanism does not matter much. I have always thought that the sensitivities of watershed conditions such as soil layer thickness, soil physical properties, etc. on runoff response can be evaluated (Tani, 2008; Tani, 2013). I fear that my goal would be negated by your assertion.

I believe that the topics discussed here are of interest to many hydrologists. Therefore, as you stated at the beginning of your response, I would like to post this on my personal website. Therefore, I would like to organize the text for you to review and, if necessary, revise it to create the content for publication. Also, you would like to have both hydrological and isotopic data, but first, I will inform the Japanese observational researchers of our discussion and your wish. After then, if they are interested, I hope you can proceed to the next step.

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(5) Beven -> Tani: February 9, 2024

We are indeed mostly in agreement – my only point is that we should not promote the Richards equation when it is evidently the wrong physics (did I send you the 2018 Century of Denial paper – if not attached here) (Beven, 2018).

That is not to say that I have a better theory to suggest MIPs tracking will not be good under dry conditions) but is something worth encouraging the younger generation of hydrologists to address (since they are still generally taught that Richards is THE physics).

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