Hydrol. Earth Syst. Sci. Discuss., 10, C3562–C3568, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C3562/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "A paradigm shift in predicting stormflow responses in an active tectonic region through a similarity analysis of pressure propagation in a hydraulic continuum" *by* Makoto Tani

Anonymous Referee #3

Received and published: 29 July 2013

Review

The question this manuscript promises to answer is interesting and important for understanding hydrologic catchment response. What controls stormflow response and can we use our knowledge about these controls to predict stormflow responses? Unfortunately, the author never really provides a clear answer to these questions (other than that it may be related to soil evolution processes) and the reader becomes increasingly confused.

The manuscript is lacking a consistent story. Oftentimes, the author seems to jump from one interesting fact in one paragraph to a completely different topic in the next without any transition or an explanation how these different topics are related to one another or to the general research question. A better structure is necessary to connect the individual sections.

I agree with the first reviewer that the details of the sensitivity analysis should be moved to an appendix or another paper while the main points should be summarized in this paper.

In the abstract the author concludes that:

a) 'Complex and heterogeneous catchment properties are poorly related to simple stormflow responses'

b) 'Simple stormflow responses may be mainly determined by soil evolution processes'

I would argue that many of the complex and heterogeneous catchment properties (like hydraulic conductivity, soil depth, slope, vegetation, etc.) are also mainly determined by soil evolution processes.

So maybe the author just did not look closely enough at the catchment properties to find stormflow controlling parameters. Or maybe the dynamic nature of the controls complicated things too much. In fact, these parameters can change in dominance over time (e.g. with certain wetness conditions or precipitation event conditions).

I do agree with the statement that the knowledge of soil evolution processes can help significantly in improving the prediction of stormflow responses, however, I would not disregard other catchment properties as potential predictors. I mean in order to predict stormflow responses you will need a physical parameter value (or a combination of them) to relate your responses to.

The complex distribution of catchment properties can be explained by soil evolution processes. That means that eventually, hydrologic response can be predicted if we

look at soil evolution processes, but in this article the author does not tell us how we can relate soil evolution processes to stormflow responses. The author needs to add that or be very clear about the fact that he does not do that.

Main Comments:

Title: The title might be problematic in a couple of ways:

a) A 'paradigm shift' because stormflow responses 'may' be mainly determined by soil evolution processes? Is that enough to call it a paradigm shift?

b) What is done 'through a similarity analysis'? The paradigm shift? The stormflow response prediction? Neither of the two makes sense. The stormflow response may be predicted by certain parameters that were found to control it (found by means of a sensitivity analysis).

c) The 'active tectonic region' confuses more than it helps in the title. The author could have also included the 'heavy storms' that are apparently important for the processes he describes - but he didn't do that.

Abstract:

The abstract could be written in a more structured and concise way. Please state the intention of your research at the beginning ('we wanted to investigate what controls stormflow responses. . .') and introduce your methods ('sensitivity analysis') and findings ('development of effective drainage systems') afterwards.

P.7047, L.6: Do not confuse hydrologic response time and residence time. They are fundamentally different and potentially controlled by fundamentally different parameters. Therefore they should not be compared in this way.

P.7047, L.8 to 11: This analogy is not necessary. Afterwards the author restates what it is supposed to mean. Maybe the author can remove it for the sake of brevity and conciseness.

P.7047, L.23: Saturation-excess overland flow is still considered a potential source of stormflow response.

P.7048, L.10 to 21: This whole paragraph is confusing. How is the question the author is introducing related to inflection points, recession limbs and triple peaks?

P.7049, L.10: This is simply not true. If the catchment soils are saturated, even a small event will cause lots of stormflow. The author cites Tromp-van Meerveld 2006 but does not mention what they say about antecedent moisture content.

P.7049, L.20: Figure 1 shows what? It sounds like it would show two flow duration curves. But it does not. Again, no mention of antecedent moisture.

P.7050, L.5 to P.7051, L.19: It is not necessary to describe this experiment in such detail. A comprehensive summary of the results is sufficient. It would be good to provide better context of how the cited study is related to the current study.

P.7052, L.8: 'May be caused by the mechanisms of water pressure propagation'. Please be more specific: Which mechanisms? How do they cause stormflow characteristics? This is too vague. . .

P.7052, L.11: There is a general lack of connection between the individual sections. For example when the tank model approach is introduced in section 2.3, there is no explanation that it is introduced because it was used to model the observed responses. Only later we learn that this was the case (L.25).

P.7052, L.19: It would be more intuitive to label 'r' as 'I' (for inflow) and 'f' as 'O' (for outflow).

P.7053, L.7: Can the author provide an objective function value to illustrate the 'extremely close agreement'?

P.7053, L.23: 'Insensitivity of stormflow response' is not correct. What the author means is the 'insensitivity of the stormflow recession', not the total response.

P.7054, L.6 to 19: Confusing mix of explanations: The author writes that 'it could be explained by the variable-source area concept' (but no explanation on what that entails), but two mechanisms may be possible. Then the author explains one of those mechanisms, then another one, then another possible mechanism. The author refers to this idea and then goes back to the two mechanisms. I got lost there. Please add some structure to this paragraph and clean up all the possible mechanisms.

P.7055, L.14 to 17: This is unclear. Dynamic equilibrium, inflow stops and outflow decreases, functional relationship of storage and outflow... I know what the author wants to say, but it could be written in a clearer way.

P.7056, L.6: '...when f > r, f DEcreases...'. I do not claim that I understand all of the equations on the next couple of pages. But when I find an error in the simplest one at the beginning, it does not give me great confidence that all the other equations are correct.

P.7056, L.20: What does the author mean by 'the speed of the flow rate'? Maybe how fast the flow rate changes in response to rainfall fluctuations?

P.7056, L.25: Is RBP always the same no matter which runoff rates are taken into account? If not which RBP is the right one?

P.7058, L.16: One can only eliminate infiltration-excess OF by setting 'r' lower than the saturated hydraulic conductivity, no?

P.7061, L.14: What is 'ε'?

P.7063, L.6: So what exactly is f? The author sometimes refers to it as flow rate, now he says it is rainfall intensity...

P.7071, L.12: The author only mentions one remaining question, the question why macropores develop.

P.7071, L.24: What is 'the effect of the downslope flow'?

P.7072, L.5: Does the author mean 'tectonic uplift' when he says 'tectonic activity'? This is too general in many places.

P.7072, L.17: What is this 'dynamic cycle of soil evolution processes'?

P.7072, L.20: 'strong erosion forces from tectonic activity' sounds earthquake-related. But that is not what is meant, or is it?

P.7073, L.1: What is 'semi-eternally'?

P.7073, L.6: What is the 'drainage capacity of water'? Do you mean how fast the soils can drain water?

P.7073, L.10: If a landslide does not occur during a storm event within a zero-order catchment you can be 100% certain that the slope remained stable across the entire area. So what does the author want to express with this statement?

P.7072, L.3 to P.7073, L.20: This section is so disconnected from the other sections. The author needs to explain how the soil evolution possibly relates to stormflow responses.

P.7073, L.22: A name for this 'simple characteristic' would be helpful.

P.7074, L.2: How can stormflow responses from soil layers provide simple characteristics as a result of collapsed soil fluidization? This sequence of sentences does not make sense.

P.7074, L.12: This is a typical case of co-evolution of hydrology and soils. I would not say that one is derived from the other. Both evolve simultaneously.

P.7074, L.21: Any idea how these drainage pathways develop according to your theory?

P.7074, L.15 to L.28: This whole section is too vague.

Technical Corrections:

P.7046, L.2: '. . .act. . . . . .state. . .'

P.7048, L.15: 'that'?

P.7053, L.1: '. . .was direct input to the tank'.

P.7058, L.2: This is a one author paper. So 'we' is not necessary.

P.7062, L.8: Sometimes?

P.7074, L.23: . . .might not be follow. . .?

Figures:

Figure 3: The o symbols for the observed runoff do not work so well. Maybe use a bold black line for this and a dashed or dotted white line for the simulated runoff rate.

Figure 4: Why is the long-term recession curve split up in two disconnected parts?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7045, 2013.