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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 #2

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The manuscript discusses the prediction of stormflow responses by integrating several key ideas that have been studied with different approaches. These include 1) field observations of stormflow, 2) a simple tank modeling and its relation to pressure propagations, 3) similarity analysis based on steady state approximation of Richard's equation with/without macropore effects, and 4) some discussions on soil-layer evolution for future modelling strategies. It is a very challenging task, which has not been done in many other studies. The author presented not only reviewing previous studies, but also added original perspectives.

Probably because of this challenging task, the author's message in the manuscript was sometimes not very clear. The followings are review comments for the possible improvement of the manuscript.

1. The title started with "A paradigm shift in predicting stormflow" but how the author recognizes the current paradigm and how should be shifted is unclear. According to the abstract, the author advocates the importance of "evolution process" considered, but it is commented only in the final discussion without mentioning what is the current and future paradigms.

2. 7046-L13 "the simple stormflow responses and complex and heterogeneous catchment properties are poorly related". What does this mean? The two approaches presented in the manuscript include "tank modeling" and "similarity analysis" based on simplified situations, and the author has successfully demonstrated how these simple approaches could provide useful insights. On the other hand, the introduction cited various field studies explaining how the nature is complex such as double peak hydrographs, which likely cannot be represented by a simple single storage-system with impermeable bedrock assumption. It is always true that the reality is very complex, while theory and models should be as simple as possible. However, my concern is that the author's key message of the introduction and the following main body is unclear, and therefore, the above sentence in L13 is left for readers without author's answer.

3. 7047-L16 "Many distributed runoff models still use the surface flow for their pathways". This kind of sentence appears in many literatures, especially in the field catchment sciences, but without enough evidence. Many of up-to-date distributed rainfall-runoff models consider both subsurface and surface flows. And the subsurface flow process is simulated not just for simulating "variable source area" (of course, some models do so intentionally). Instead, the quick lateral subsurface flow is simulated with a few order higher hydraulic conductivities to implicitly represent the macropore and other preferential flow effects. In fact the cited author's previous study (Tani (2008)) and the equation in (22) in this manuscript (K = e x Ks) are good examples for the

simple approximations.

3. 7048-L10 "Nevertheless, the new question has emerged: ..." is unclear. First, "previous studies could not demonstrate why water movement within a soil layer resulted in the production of stormflow" seems to be contradicting with a sentence in the above section, "many well-designed observations were conducted to explain the production of stormflow by soil water movement". Second, "inflection points" appears suddenly without strong connections with other parts.

4. Section 2.2 presents interesting examples of pressure propagations from field studies. In this discussion, the author mainly divided the dominant flow processes as "vertical plug flow in the unsaturated zone" and "a high-speed preferential downslope flow in the saturated zone". According to the following sentence "these processes should generally follow the hydraulics of water pressure propagation", the author recognizes the quick preferential downslope flow is also controlled as "pressure propagation". Then its mechanism is just explained as "the latter originated from the water table as the downslope flow rose and fell in response". Since this part is very important, related also to the next section, it should be explained more in detail with some evidences.

5. The keywords of the manuscript are "quasi steady state" and "pressure propagations". The example of the simple tank approach is used for explaining the importance. I have some confusions here what the author attempts to say with the tank approach: "any simple conceptual model including tank approach can represent the two important mechanisms" or "many existing models fail to represent pressure propagations". If it is the former case, do we still need the paradigm shift?

6. 7054-L1 "the recession flow calculated with p = 1 is plotted as a straight line". Is there any straight line in Fig. 3?

7. 7054-L19 Explain what “constant recession gradient” means.

8. Section 3.3-3.7 Part of the derivations of equation may be placed in appendix, so

that only the key points in these sections become clear and easy to read.

9. The result of the sensitivity analysis shown in sect. 4.1 is very interesting. But the last sentence in the section is unclear. "This indicates that RBPI* only follows dSe/dK* in the I zone in a domain with a negligible downslope flow effect in the U and S zones." I thought that the downslope flow is dominant in U and S zones and the vertical flow is dominant in I zone. But the above sentence seems to be opposite.

10. 7074-L7 "saturation-excess overland flow generally occurs unless the macropore effect inhibits the groundwater table." Is this sentence correct? Should it be like "saturation-excess overland flow generally does not occur because the macropore effects inhibits the rising of groundwater table"?

11. 7071-L23 "It is still an open question as to why macropores generally develop". "The reason may be ... as discussed below". In fact, the discussion in sections 5.2 and 5.3 are interesting ones. However, the conclusions of 5.2 and 5.3 seem to be “consequences” of macropores rather than the “reasons”. Slope is not a creature that it develops itself to increase its stability. Perhaps erosion and other weathering processes are the “reasons” for the development of macropores. This is not a review comment, but the paper would be more enhanced if author’s idea is clearly illustrated in this respect.

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