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.

Thank you very much for your valuable comment. My response to each of your comments are as follows.

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.

This word 'paradigm shift' is clearly related to a recognition of the severe difficulties on detecting sensitivity of stormflow responses to catchment properties obtained from the hillslope observational studies stated above. If you would think the sensitivity can be estimated by the present studies on observation and modelling, certainly this paper could not provide a paradigm shift. Most of the sophisticated hillslope observational studies have rejected the recognition. Many ideas such as 'main stormflow contribution by 'transmissivity feedback hypothesis' (Kendall et al., 1999), 'important role of pipeflow' (Uchida et al., 2005), and 'a concept of fill and spill' (Tromp-van Meerveld and McDonnel, 2006) have contributed to understanding a partial face of the runoff processes, but they have not provided a core concept necessary for parameterizing catchment properties in distributed runoff models. This sense may be stated in the analogy of 'a man searching his key in the light' in the introduction of my original manuscript, but a clearer description will be inserted in the introduction of my revised manuscript. As the conclusion of this paper, we have proposed two strategies for stormflow prediction: a parameterization of catchment properties in consideration of the historical soil-layer evolution, and comparative hydrology for detecting sensitive properties. This can be expressed as a paradigm shift in predicting stormflow responses in this geographical condition. This will be explained in the introduction in the revised manuscript.

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.*

Thank you for your suggestion. I will focus the citation of site observation on the stormflow processes at enough large magnitude, and remove Fig. 4 including the early stage of storm events with small magnitudes.

I mainly state in this paper that the stormflow processes at actual hillslopes are complex and heterogeneous, whereas the stormflow responses both at actual hillslopes and runoff models were commonly simple. Why the contrast was found between the processes and responses was mainly examined in the sensitivity analysis in sections 3 and 4, which concluded that the combination of the saturated and unsaturated flows and a large drainage capacity of downslope flow were generally needed. The following section 5 discussed on why this large drainage capacity was naturally created and suggested this was originated from the soil evolution process against the severe erosion forces in an active tectonic region with massive storms. Connecting the three portions in this paper will be much will be much clearly explained in the abstract, introduction and the transition paragraphs between the three in the revised manuscript.

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 ($K = \varepsilon \times Ks$) are good examples for the simple approximations.

I can clearly understand your point, but the motivation of this paper is why stormflow responses were *difficultly* predicted from the topographical and soil properties through distributed runoff models which have overland flow and/or subsurface flow as a component of the algorithm. Both the characteristics of each runoff model with such a component in it and the observation results on occurrences of overland and subsurface flows can never give good answers to the prediction difficulty above. However, the sensitivity analysis here showed that this difficulty was derived from a difficult causal relationship between the stormflow response and its processes: a combination of the saturated and unsaturated flows and a large drainage capacity of downslope flow within the soil layer creates a hydraulic continuum under a quasi-steady state causing a simplicity of the stormflow response although the soil layer is characterized by large heterogeneities.

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.

This question is why the stormflow is distinguished from the baseflow, that is, why the recession hydrograph has an inflection point from quick portion (the half life of less than one day) to slow portion (that of over several days). We have to consider at least two components for simulating the hydrograph recession, and a single tank model even with a nonlinear function cannot simulate the entire hydrograph recession. This strongly suggests the transformation of rainfall to runoff is produced from the plural domains such as the ground surface, soil layer, and weathered bedrock. In this paper, I have consistently focused on the soil layer as a possible system producing stormflow responses. I have removed the expression of ‘question’, but I will discuss this point in the section explaining a quasi-steady state as: we can extract a quasi-steady state system producing stormflow responses whereas the entire runoff responses are produced from a plural systems such as a serially-concatenated tank system.

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.

A term ‘pressure propagation’ used in my original manuscript will have been removed in the revised manuscript. I think this term is defined clearly by hydraulics, but an ambiguity may be included for actual uses. For water movement within soil matrix, water moves along the local gradient of hydraulic head, the total of pressure head and gravitational head. This is also valid for the pipe flow. However, the value of unsaturated hydraulic conductivity is very very small in a dry portion of the soil because large-sized pores does not function as water pathways, and the pressure propagation is considered almost negligible in this portion with a low matric potential. When the wetting front moves downward during a storm event, the dry zone between the wet transmission zone above the wetting front and capillary fringe near the groundwater table may behave as if this intercepts a pressure propagation from the upper transmission zone to the lower saturated zone. Therefore, the term of pressure propagation might have given readers an ambiguous impression. As a result, the term 'pressure propagation' will be removed from the revised paper to avoid any confusing.

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?

Please note a difference between a single tank with a hole and a runoff model like Sugawara's tank model, one of the serially-concatenated tank systems. The former was only used here for understanding the nature of a quasi-steady state, whereas the latter never shows this nature, as already stated in item No.3 above. The soil layer functions as a hydraulic continuum under a quasi-steady-state system only when a dry portion in it disappears by enough amount of rainfall supply and the waveform transmission from the inflow to the outflow is directly connected. A soil layer with the direct connection can give the same transmission as that of a tank with a hole because both systems are the same from the viewpoint of a quasi-steady-state system as explained for the onsite results in CB1 and TEF in Section 2.

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

No. This shows a character of the semi-logarithmic plot only. However, this figure will be removed because I focus only on a condition after enough large rainfall supply.

7. 7054-L19 Explain what "constant recession gradient" means.

This means a similar recession gradient regardless of the stormflow magnitude, but this section 2.4 will be removed by the same reason above (item No.6).

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.

They will be moved to the appendices and the main flow will remain in the body text.

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.*

RBPI*, that is, a delay of waveform transmission from input to output is controlled by a differential coefficient of storage with respect to steady flow rate ($\mathrm{d}V/\mathrm{d}f$: see Fig. 5 but Fig.3 in the revised manuscript). When the horizontal slope length is short, or, when the downslope flow is so fast by a large drainage capacity with a large ε value, $\mathrm{d}V/\mathrm{d}f$ is controlled mainly by the unsaturated vertical flow in the I zone because the downslope flow in the U and S zones is so quick that the storage fluctuation give little influence on $\mathrm{d}V/\mathrm{d}f$. This explanation will be inserted to the revised manuscript.

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"?

I think this sentence is true. The macropore effect can be recognized as the restriction of groundwater-table rising by a large drainage capacity of the saturated downslope flow (in the I zone) in this paper. Therefore, at least for a large-magnitude storm, saturation-excess overland flow occurs without the macropore effect, whereas it does not occur with a large effect. The contrast may give a connection between this effect and the soil-layer evolution against severe erosion forces in this geography.

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

Thank you very much for your suggestion. I will change the description according to it. The revised description will be that the development of an efficient drainage system has to be naturally associated with the evolution of the soil layer.

References

Kendall, K. A., Shanley, J.B., and McDonnell, J. J.: A hydrometric and geochemical approach to test the transmissivity feedback hypothesis during snowmelt, J. Hydrol., 219, 188-205, 1999

Tromp-van Meerveld H. J. and McDonnell, J. J.: Threshold relations in subsurface stormflow: 2. The fill and spill hypothesis, Water Resour. Res. 42, doi:10.1029/2004WR003800, 2006.

Uchida, T. Tromp-van Meerveld, I., and McDOnnell, J. J.: The role of lateral pipe flow in hillslope runoff response: an intercomparison of non-linear hillslope response, J. Hydrol., 311, 117-133, doi:10.1016/j.jhydrol.2005.01.012, 2005.