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.

Thank you very much for your valuable comments, the response to them are as follows.

The answer for the subject 'what controls the stormflow responses to rainfall' depends on geographical conditions first of all, and the categorization by geography has been pursued by many hydrologists including field and modelling scientists. Already in my original manuscript, I intended to focus the geography on an active tectonic region with massive storms covered with forests, but this intention has not been well delivered to the reviewers. This was caused by my insufficient explanation and the geographical restriction will be clearly described in the revised manuscript.

Another important limitation of the condition in this paper is a spatially-fixed area producing stormflow responses. The stormflow-contribution area is generally variable, but, from the water-balance point of view, the contribution area may be almost fixed in the case when most of the rainfall is allocated to stormflow because the contribution area is extended to the entire area. We concentrate the target of mechanism consideration on this case. I noted 'enough wet condition' but the 'spatially fixed condition' was not clearly stated in the old manuscript. This will be clearly explained in the revised manuscript. To avoid any confusion and to focus on this limitation, I have removed the Section of 2.4 'Insensitivity of the stormflow response to storm magnitude' with Fig. 5.

The main concern from all the reviewers was a problem of consistent story connecting a review of stormflow process in Section 2, a similarity analysis in Sections 3 and 4, and a discussion on the soil evolution effect in Section 5. I will explain the interrelation in the abstract and the introduction as well as the connection passages between the sections in my revised manuscript, and also summarize it below. As suggested by all the reviewers, I will move the method of sensitivity analysis and similarity framework to the appendices.

Observations on hillslope hydrology showed that the stormflow responses from the fixed stormflow-contribution area were simply represented by a single tank with a drainage hole in the bottom (this will be called TANK in the revised paper). In addition to this, TANK was used for many runoff models as a stormflow component although the contribution area is not fixed but the effective rainfall has to be separated from the observed one. This suggested the input/output transformation by TANK may commonly characterize the stormflow responses at least for a simple condition where the contribution area spatially invariable. However, this characteristic was only empirically obtained from TANK and without an enough physical base.

Why can the stormflow responses generally represented by TANK? This is the subject of our sensitivity analysis in Sections 3 and 4. First of all in this analysis, the characteristic of TANK was explained where both the delay of inflow/outflow waveform transformation and the recession gradient are controlled by the differential coefficient of storage with respect to steady flow rate. In the sensitivity analysis, a two-dimensional sloping soil layer with homogeneous topographic and soil properties was selected as a domain for the analysis.

In this connection between the observation review and sensitivity analysis, a hydraulic continuum under a quasi-steady state (this will be called QSS in the revised paper) was employed as a key common characteristic to generalize the individual observation results. Consider a conversion system from input to output composed of very many heterogeneous components produces a very similar output to that produced by another system with a few homogeneous components with a clear physical background. One of the methodology for understanding core conditions in the former producing a similar output may be a detailed investigation for the characteristics of the latter. In the case here, the latter contains still many unknown characteristics though the physical background itself is clear. This is the very reason why the sensitivity analysis with a newly-developed similarity framework for the generalization was made after the observation review. The conditions obtained were: the combination of the saturated and unsaturated flows and a large drainage capacity of downslope flow.

Why does the large drainage capacity have to be generally found in an active tectonic region? Indeed, future field investigations will be needed for generalizing this idea, and now we can do only a discussion in reference to the previous studies as described in Section 5. The main suggestion is: for an environment with strong erosion forces, because overland flow is a main cause for landslide initiations, to *confine* the stormflow within soil layer does play a key role in the soil layer evolution. It s suggested that the soil-layer cannot be evolved unless the creation of a large drainage capacity is accompanied. I never neglect the occurrences of infiltration-excess and saturation-excess overland flows. However, they may have a relationship to the surface-erosion and landslide-initiation processes controlling the soil-layer evolution. When all the subsurface flow and these overland flow involve these processes are taken into consideration, effects of the heterogeneous topographic and soil properties inside of the soil layer on the stormflow responses will be able to assessed quantitatively. This is a duty of both the future observational and modelling studies.

I believe the logic flow is consistent even though three portions of this paper may appear to be different subjects. The reviewers and readers might wonder the different papers should be made for these subjects. However, I have to emphasize that one integrated paper can only explain the logic comprehensively.

In addition, 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.

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.

Obviously, various catchment properties like topography, soil depth, soil hydraulic properties, and vegetation covers give large influences on stormflow responses besides the soil evolution processes. As a forest hydrologist studying runoff processes from the field observations for over 30 years, I think I have considered all the influences. However, specifying the effect of each property on the runoff responses is quite difficult on the runoff analysis based on the hillslope observations. This conclusion is obtained not only from my experiences but from the history of hillslope hydrology since 1960s. In the revised introduction, this difficulty will be described more clearly.

Most of hydrologists focusing on the modelling have not enough understood how difficult we could detect the relationships of runoff responses to each catchment property. It would be quite difficult for example how to express the spatial distribution of soil depth even though a detailed field investigations using the cone penetrometers were conducted. Nevertheless, we have to quantify a sensitivity of runoff responses to each catchment property. This is a very reason why we have to consider the historical mediation by the soil-layer evolution process between catchment properties and runoff responses.

However, this paper focuses the target on stormflow responses in a specific geographical condition as a high tectonic activity, and the responses only in very simple conditions with a spatially-invariable contribution area due to enough volume of storm rainfall supply was selected for the sensitivity analysis. A hydraulic continuum in QSS is only established in such a special simple condition and can produce essential findings on the sensitivities.

A question 'how we can relate soil evolution processes to stormflow responses' in your comment cannot be answered right now because my paper has only provided the possibility for the relationship between the response and evolution by reviewing the previous studies on severe erosion processes in an active tectonic region in Section 5. I intended to make clearer descriptions for the relation.

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?

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.

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.

Thank you for your comment and I have changed the title into: A paradigm shift in stormflow predictions in an active tectonic region with large-magnitude storms
- Through a generalization of hillslope observations by a sensitivity analysis and an insight into the soil-layer evolution -

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.

Thank you. I will modify the abstract according to your suggestion.

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.

I see. To avoid the confusion, I will remove this.

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.*

I will remove this analogy.

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

In this paragraph, it is enough to tell Dunne's concept that quick stormflow responses could not be explained if overland flow was generated. So I will modify the sentence.

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?

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.

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.

The saturated condition before the storm is quite unrealistic even in wet climate region. You mentioned 'lots of stormflow', but I argue the growth of stormflow in this paragraph.

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.

I am sorry but I can't perfectly understand what you mean. However, to avoid a confusion, I have removed the figure for Kiryu. Only KT will be shown in the revised manuscript. I will explain the meaning of this figure. The total stormflow volumes in response to the total rainfall volumes in storm events were plotted, and they controlled the relations clearly as indicated by the wetness index using the runoff rate before the storm event. The figure shows the threshold of rainfall volume over which the stormflow volume largely increased and most of the increase of rainfall is allocated to the stormflow over the threshold.

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.

I think this needed for this paper the total results of which are closely related to the context in my paper.

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. . .

In general, water moves along the gradient of hydraulic head even if the flow is hydraulically described by any type of flow including pipe flow without water table, stream flow with water table and the groundwater flow. In the revised manuscript, I will not use the term 'pressure propagation' as mentioned in the beginning of this review response.

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).

I will modify the description in the revised manuscript: the total explanation for TANK and a hydraulic continuum under QSS is made in 2.3 to understand their role in the inflow/outflow waveform transmission.

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

Thank you, I will follow this.

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

The only one parameter *k* is optimized and such results in Fig. 4 has been obtained. I think my description 'extremely close agreement' is acceptable from the illustrations in this figure and not influenced by a selection of the objective function.

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.

Thank you for your comments. As mentioned before, I will remove this section 2.4, because this paper should focus on large-magnitude storms when the contribution area is almost extended to the whole catchment.

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.

I see. I will make a better description.

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.

Thank you and I am very sorry for this mistake. I will make more careful descriptions.

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?

'The speed of the flow rate' simply represents df/dt, so this term will be inserted after the phrase.

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?

Please see the right panel of Fig.5 (Fig. 3 in the revised manuscript) for example. The storage increase for k=40 is larger than k=10, and this reflects that the left panel where the flow-rate change for k=40 is gentler than k=10. In general, this difference is derived from the storage increase in response to an increase of outflow rate. The RBP represents the equalization of flow-rate increasing and decreasing.

Calculate the storage $V(f_a)$ in response to a steady-state outflow rate f_a and calculate the storage $V(f_b)$ in response to another steady-state outflow rate f_b larger than f_a. RBP = $V(f_b)$ - $V(f_a)$. When this value for a soil layer C is larger than another soil layer D with different topographic and/or soil properties, the storage increase is larger for C than D. As a result, the capacity of equalizing the flow rate is larger for C than B for the flow-rate increase from f_a to f_b.

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

Yes.

P.7061, L.14: What is 'ε'?

The sentence 'where the parameter *'ε'* represents a macropore effect.' will be inserted.

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. . .

I will correct this because f is defined as the steady outflow rate per unit slope length. Thank you.

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

As mentioned at the beginning of this review response, I will modify the description for the connection of sensitivity analysis with discussion.

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

This means that makes the effect of the downslope flow on RBPI relatively small because the effect of vertical flow on it governs for the case of a large drainage capacity with a large ε value. The description will be modified for easier understanding.

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

I used theses terms: high tectonic activity generally covers Japan and tectonic uplift generally covers mountains in Japan whereas tectonic subsidence generally covers the basin-shaped valley.

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

This means a repetition of landslide occurrences and the evolutions of soil layer. The description will be improved.

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

The gradual increase of elevation difference between the uplifting ridge line suffering from small erosion forces and the riverbed with also uplifting but suffering from strong erosion forces occurs consistently in this geography. Therefore, this phenomenon continuously causes decreasing the stability of soil on a slope between the ridge line and the riverbed. The description will be improved.

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

For the case of small denuded areas produced a landslide occurrence mainly within the hollow on a zero-order basin, the soil layer with vegetation may naturally be recovered by soil supply from the surrounded areas with vegetation cover. For a denuded slope in a granite mountain created by a long-term human disturbances (over several hundred years) at a wide landscape scale, however, the observational studies (Fukushima, 2006) demonstrated the soil layer could not evolve because soil particles from the surface of deeply weathered bedrock was quickly eroded by the overland flow within one year and there is no surrounding are with soil and vegetation. Therefore, the recovery of soil on a steep slope depends on delicate conditions consisting of topography, size of denuded area, soil characteristics, supply of vegetation seeds and climate. The description will be modified about 'semi-eternally'.

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

Thank you for your question. This will be replaced to another description using a large drainage capacity.

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?

When a landslide occurs, the hydraulic continuum will be broken, but when no landslide occurs on a slope within a catchment, the soil layer will be consistently function as a hydraulic continuum producing the input/output response of a quasi-steady-state system.

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.

In response to this comment, I will modify the sentences for possible readers to easily understand the consistent story on 'the relationship of soil-layer evolution to stormflow responses', as already described at the beginning of this review response.

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

The description of 'simple characteristic' will be changed to 'the simple characteristic of stormflow response simulated by a single tank with a drainage hole'.

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.*

When a landslide occurs, the hydraulic continuum will be broken, and the water in the soil layer will be released from the soil matrix causing the initiation of debris flow (Takahashi, 1978). As far as landslide does not occur on any hillslope within a zero-order catchment, the soil layer will be consistently function as a hydraulic continuum producing the input/output response of a quasi-steady-state system because water is confined within the soil matrix. As a result, the stormflow response from this catchment can be characterized by a simple characteristic unless a landslide does not occur. However, I agree with your comment about a logic jump, and description will be modified. Thank you.

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.

Yes, I will replace 'derived by' to 'accompanied with'. Thank you for your valuable suggestion.

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

How to develop such a drainage system with a large drainage capacity has been discussed only in a few studies (Tsukamoto and Ohta, 1988), but it may be suggested that a production of soil block reinforced by a vegetation root system and an erosion process of fine soil particles under the ground would simultaneously progress together, resulting in an expansion of the heterogeneities in permeability within the soil layer. This paragraph will be inserted in the revised manuscript. Of course, further studies are needed for the validation.

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

As explained near the beginning of this review response in response to your comment of the title, this point of model performances is clearly related to a recognition of the serious difficulties on detecting sensitivity of stormflow responses to catchment properties. I will make the explanation clear in the revised manuscript.

Technical Corrections:

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

The sentence will be changed.

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

I intended that 'that' means 'the half-life of a recession limb'. The explanation will be modified.

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

The comment is not perfectly understood, but the description will be modified.

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

Yes, this will be modified.

P.7062, L.8: Sometimes?

This will be replaced to eoften f.

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

This will be replaced to ’might not agree with’.

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.

Yes, I will modify them.

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.

This figure will be removed. Thank you.

References

Fukushima, Y.: The role of forest on the hydrology on headwater wetlands, in: Environmental Role of Wetlands in Headwaters edited by Krecek, J., and Haigh, M., Springer, Dordrecht, ISBN 1-4020-4226-4, 17–47, 2006.

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

Takahashi T.: Mechanical characteristics of debris flow. J. Hydraul. Div., ASCE 1048, 1153–1169, 1978.

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.

Tsukamoto, Y., and Ohta, T.: Runoff process on a steep forested slope, J. Hydrol., 102, 165-178, 1988.

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.