Overview

The manuscript (i) reviews previous field studies of rapid subsurface storm flow, and concludes that when the total rainfall is large enough, the mechanism for stormflow production can be attributed to pressure propagation in a hydraulic domain; (ii) carries out a model-based similarity analysis to assess an instantaneous response function for pressure wave transmission of subsurface flood waves within steep hillslopes, and ascribes some of the properties of the response function to macropore flow; (iii) concludes that evolution of the soil layer, leading to development of complex spatial structures, is an important consideration for future research on storm runoff.

The paper combines an interesting array of field and modelling studies, but I found it a challenge to read. It is long, and attempts to tie together many threads, yet it seems to be specific to a particular environment which is not defined by the author (perhaps steep humid forested catchments?). At times the author reaches intermediate conclusions which are not obvious to me, for example on interpreting streamflow response as being a result of pressure propagation. The paper would be more widely understood if it was shorter, and focussed more on what I believe are the author's main points: (i) evidence for pressure propagation; (ii) relationship of the pressure propagation mechanism to conceptual hydrological models; (iii) consequences of the pressure propagation mechanism for further development of hillslope hydrology.

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

I intended to focus the geography on an active tectonic region with large-magnitudee 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.

I have to explain a removal about using a term ‘pressure propagation’ in this revision. 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.

Main Points

1. 7046L1 “Soil layers on hillslopes acts as systems in quasi-steady states generating rainfall-stormflow responses that are controlled by pressure propagation in a hydraulic continuum established when the rainfall volume is sufficiently large.” This statement needs qualifying (perhaps with adjectives such as steep, forested, humid, permeable); it is not relevant in arid and semi-arid settings where infiltration excess surface runoff is frequently the dominant runoff-generation mechanism for large storms.

The abstract will be changed in the revised manuscript to specify the geographical conditions and fixed stormflow-contribution area.

2. 7046L15 A major point that the author makes in the introduction is that sub-surface properties may control storm response. The important additional point the author does not make (except for the phrase “especially in active tectonic regions”) is that under- ground pathways are not always dominant (in contrast to 7047L1). They may well be dominant everywhere in the landscapes that the author considers, but they are not necessarily dominant in, for example, arid or semi-arid landscapes. Dunne’s diagram of physical controls on runoff generation mechanisms illustrates this well (e.g. Figure 1 in T. Dunne, The relation of field studies and modeling in the prediction of storm runoff, J. Hydrology, 65, 25-48, 1983)

The geographical condition addressed in this paper will be strictly described in the revised manuscript.

3. 7047L11 "This analogy may apply to the developers of distributed runoff models, who have built their models based on the surface topography" I think this is a bit unfair on the model developers. Most developers of models that rely on surface topography do so because there is a strong association between surface topographic attributes and the hydrological response of the study area they were considering when they built themodel. The(mis)application of those models to other environments suggests to me that the real problem is in the model user community.

I will remove the analogy. Thank you.

4. 7048L16 "Double or triple peaks are sometimes generated in small catchments, as mentioned before (Onda, 2001; Kosugi et al., 2011), but the responses of river flow to rainfall commonly contain a quick component of stormflow with a short half-life distinguished from the entire hydrograph. Although this question may be unique and not generally addressed, it is believed to provide important information on stormflow mechanisms and modelling." It is not clear what the author means by "this question". As a result, the message of this paragraph is unclear to me. This is important for this paper, because it seems that the author intended to present the main new idea of the paper in this paragraph, or perhaps present the motivation for the specific features of the paper which are presented in the next paragraph.

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.

5. 7049L10 "When the rainfall is small, the stormflow is low because most of the rainwater is stored in the soil layer by absorption within small pores with a low matric potential." This explanation for low stormflow volumes is presumably appropriate for the catchments to which *the authors is referring, but soil water storage is not the only mechanism for low runoff coefficients in small storms (e.g. storage of water on plant canopy, within the snowpack, in surface depressions, in a litter layer). During this introductory phase, the paper needs to make explicit the environmental context for which it is written.*

First of all, snow effects are not considered here. Other storage besides the soil storage will be mentioned in the revised manuscript.

6. *7053-4/Section 2.4 It is interesting that the same exponent, p=0.3, was found for all three case studies. I would like to see the author at least briefly explore whether there is any connection to the theory of aquifer drainage (see e.g. Brutsaert and Nieber, 1977, Rupp and Selker, 2006).*

I think no clear reason for the value of p and the value is only empirically obtained because the it is a result from the complicated flow mechanisms including vertical unsaturated flow and downslope saturated flow. This will be stated in the revised manuscript.

7. 7054L6-16 The connection from the tank modelling to pressure propagation is not made clear. It is apparent that the author sees a clear connection between the two, but this is not made explicit for the readers. For readers less familiar with the field sites where the model is applied, there are many possible interpretations of the flow recession behaviour of different model parameters, and it is not clear why variable source areas or pressure propagation are discussed, but, for example, the heterogeneity of soil and aquifer material properties (e.g. Harman et al 2009) and the location and nature of their interface (Tromp-van Meerveld and McDonnell, 2006) is not discussed.

I will have decided to remove Section 2.3 for the easier understanding because this paper should focus on large-magnitude storms when the contribution area is almost extended to the whole catchment.

8. 7055L7 At the end of the modelling/interpretation sub-sections 2.3-2.5 I am left with the impression that the author has an important story to tell about pressure propagation, but that it has not been conveyed with sufficient evidence to make it convincing. The author did not present any data on pressure, and the modelling did not explicitly represent soil water pressures.
9. 7055L10 "The observation results presented in the previous section suggest that the stormflow responses were created through pressure propagation" I do not agree. The stormflow responses could have been created by pressure propagation, but they could also have been created by other mechanisms. I think the author needs to make this point more clearly, before proceeding.

For the items of 8 and 9, please see the end of the review response to the overview about avoiding the term of ‘pressure propagation’ in the revised manuscript. Thank you.

10. 7055L13“Such a tank can be generally regarded as a “quasi-steady-state system””. This is a true statement about the tank model, but it is not necessarily a true statement about pressure status in a real catchment (until more evidence is presented).

As mentioned in the end of the review response to the overview, I think the pressure propagation itself is consistently valid for the water movement. However, the important point is whether the fluctuating waveform of rainfall is directly transmitted to that of outflow from the soil layer or not.

11. 7057L1-14 It appears that the author has linearised the nonlinear storage- discharge relationship around f=f_{m}*, and then derived the time-constant. If this is a linearistaion, then it would be useful for the author to show this connection, and if it is not, then more explanation is needed.*

My target of the sensitivity analysis using f_{m} is to compare the RBPI values in response to any topographical and soil properties each other when a steady-state inflow (=outflow) rate is given. The RBPI is calculated by

$$\mathrm{RBPI}\Big|_{f_{\mathrm{m}}} \equiv \frac{\mathrm{d}V}{\mathrm{d}f}\Big|_{f_{\mathrm{m}}} = \lim_{\Delta f \to 0} \frac{V(f_m + \Delta f) - V(f_{\mathrm{m}} - \Delta f)}{2\Delta f}$$

For this target, the rainfall hyetograph is first decomposed into the average and the fluctuation around it, and the waveform transmission system is regarded as fluctuations around the average outflow rate f_{m}. Please imagine a case for a tank with a drainage hole as a typical QSS system. When the input fluctuates around f_{m}, the storage will also fluctuate around the steady-state water level and the output will fluctuate f_{m} with some delay. This transmission process can be also accepted for a sloping soil layer as far as it is a hydraulic continuum under QSS.

In the recession stage after the input stops, the same recession process of outflow occurs also for the soil layer and the tank, and the storage/outflow relationship in a steady state continues. Hence, The gradient of the recession at any outflow rate f in the recession stage is represented by the following equation.

$$\frac{\mathrm{d}f}{\mathrm{d}t} = \frac{-f}{dV/df} = \frac{-f}{\mathrm{RBPI}\big|_f}$$

Equation (6) used f_{m} may make a confusing, and I will use f for the revised manuscript. As a result, the recession hydrograph is obtained from the integration of this equation.

12. 7058-7069 There now follows a comprehensive similarity analysis of s 2-dRichards equation hillslope model, which could comprise a paper in its own right. The key points need to be summarised in the main body of the paper, and the rest of the detail re- moved to an appendix, or another paper.

Thank you for your suggestion. I will describe the key points in the main body and the rest of the detail in the appendix.

13. 7072-3 Section 5.2 This section on the relationship between tectonic activity, erosion and soil drainage systems seems quite distinct from the similarity analysis, and it is not clear why they even belong together. It seems to me that most of the analysis from 7058-7071 (sections 3.3-4.2) could be omitted without losing what I understand to be the main point of the paper.

As mentioned in the top of this response to the reviewer, this section is needed to consider why the large drainage capacity has to be generally evolved. The connection of logic flow will be improved.

14. 7073L18 "Thus, we can conclude that the development of efficient drainage systems along a hollow are inevitably associated with the evolution of the soil layer" There are too many assumptions to justify the use of the word "inevitably"; more cautious wording would be more appropriate. Minor Points

I will use 'naturally' instead of 'inevitably'.

Minor points

15. 7048L10 " "previous studies could not demonstrate why water movement within a soil layer resulted in the production of stormflow" This statement confuses me, because it conflicts with the author's previous statement "many well-designed observations were conducted to explain the production of stormflow by soil water movement (Mosley et al.,1979; Pearce et al., 1986; McDonnell, 1990)."

The fact obtained from the many observations is the soil layer produces stormflow in an active tectonic region with large-magnitude storms. However, it has not well been understood why the water movement in the soil layer generally *have to* produce a quick response of stormflow. Overland flow drains so quickly that it is naturally distinguished from the baseflow with slow movement. This may be a reason why many runoff models assume the stormflow responses as a production of overland flow. In this logic flow, I have provided the following analyses in this paper:

The review in section 2 showed clear field evidences about the contribution of water movement to stormflow.

The sensitivity analysis in sections 3 and 4 was made to detect the general conditions necessary for this contribution.

The discussion in section 5 suggested why the contribution was created.

This logic flow will be clearly explained in the revised manuscript.

16. 7048L11 "A hydrograph generally has rising and falling inflection points." I do not see how this is connected to the previous sentence.

I will remove the description about the question.

17. 7049L10 "When the rainfall is small, the stormflow is low because ..." With the phrase "stormflow is low" the manuscript needs to be clear whether this is meant in absolute or relative terms, and whether small refers to rainfall volume or rainfall intensity or both. I suggest something like "When the rainfall volume is small, stormflow is a small proportion of rainfall because ..."

Yes, thank you for your suggestion. The reviewer #3 also mentioned various components involved in the stormflow increase and the loss decrease in response to the rainfall increase. I will make a clearer description for this.

18. 7052L23 "Equation (1) represents the water balance as a physical law" This is only a "law" if the assumptions behind the equation are accurate (e.g. negligible evaporation, negligible drainage to deep groundwater, negligible inflow of groundwater from adjacent catchments). Those assumptions may be empirically true in this setting, but they do not constitute a physical law.

This description was made only to emphasize an empirical character of the functional relationship between flow and storage in Eq. (2). The description will be improved.

19. 7054L25-27 "Practical stormflow analyses for flood management purposes in headwater catchments in Japan have provided examples of successful applications" These model successes show that the tank model works well, but they do not provide any evidence about whether pressure propagation was a dominant process in those catchments. Nonlinear storage-discharge relationships can succeed for many different reasons.

I will remove the term of 'pressure propagation' in the revised manuscript to avoid a confusing. In this connection, I only stated stormflow *could be empirically* simulated by a component of runoff models represented by a tank with a drainage hole. Any other components of runoff models also can do that of course.

20. 7055L17"This character of quasi-steady state systems can be hydraulically derived from pressure propagation under gravity. This is typically described as Darcy's law both in saturated and unsaturated zones in a permeable domain." These statements need a citation to support them.

The description here will be improved for the clearer understanding. I am explaining my intention however. The sentences mean that as the physical background of the quasi-steady state system, the water movement is generally controlled by the gradient of the hydraulic head consisting of pressure head and gravitational head and that the water movement *both* in the saturated and unsaturated zones are controlled by Darcy's law. I thought no citation for this context.

21. 7055L24-25 "We refer to this system as a "hydraulic continuum" for the production of stormflow" By the word "system", is the author referring to the tank model, or the real catchment?

This meant that a system producing a simple pressure propagation in a quasi-steady state. I will remove the pressure propagation and modify the description.

22. 7062L25 Why was B selected as the variable to combine with f_m in order to derive a non-dimensional length? There are many other ways to define a non-dimensional length scale while using f_m, so it is important to know why the author considers this choice advantageous.

As already mentioned before in the response of No. 11, my target of the sensitivity analysis using f_m is to compare the RBPI values in response to *any* topographical and soil properties each other when a steady-state flow rate is given. Only selecting *B* (estimated a constant $10^{0.4}$ by the dataset of soil hydraulic properties) allows the sensitivity analysis to compare RBPI for this comparison. Therefore, this selection is the most important for the nondimensionalization in this similarity framework. The selection of *l* only can give the length scale a character that is free from the any influences of topographic and soil physical properties of the soil layer, and we can assess the sensitivity to them for any soil layer in response to a given steady flow rate f_m. When any other length scale than *l* such as $-\psi_a$, $-\psi_m$, *D,* and *L* is selected, results of the analysis will be dependent on one of the topographical and soil properties in addition to f_m. This will be explained in the revised manuscript.

23. 7073L1 "the soil cannot be semi-eternally recovered" I could not understand this phrase.

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'.

24. 7073L6 "the drainage capacity of water" might be clearer if written as "the drainage of the hillslope"

'A large drainage capacity of the downslope flow within a soil layer' will be used in the revised manuscript.

25. 7073L10 "If a landslide does not occur during a storm event within a zero-order catchment, we can infer that the slope might have remained stable across the entire area." I could not follow the logic here; why do you say "might have remained stable"? What is the "entire area"? Since I did not understand this sentence, I could not follow the rest of the paragraph. References

Please see the logic flow at the beginning of this review response. An addition explanation is stated below: when a landslide occurs, the hydraulic continuum will be broken. As far as landslide does not occur on any portion of 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 as far as a landslide does not occur.