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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

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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

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 func-

tion 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.

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.

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 underground 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)

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 the model. The (mis)application of those models to other environments suggests to me that the real problem is in the model user community.

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.

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.

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).

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.

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.

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).

11. 7057L1-14 It appears that the author has linearised the nonlinear storage-discharge relationship around f=fm, 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.

12. 7058-7069 There now follows a comprehensive similarity analysis of s 2-d Richards 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 removed to an appendix, or another paper.

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.

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

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).”

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

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 ...”

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.

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.

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.

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?

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

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

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

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

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