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Hydrology and Earth System Sciences Discussions

Interactive comment on "Dye staining and excavation of a lateral preferential flow network" *by* A. E. Anderson et al.

Anonymous Referee #2

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The authors present an interesting and illustrative descriptive field study examining the connectivity of preferential flow paths and the effect of contributing upslope area on preferential flow based on a large-scale staining test using Brilliant Blue FCF dye. The uniqueness of this study relates to the scale at which it was conducted. This scale, and the limited number of excavations that were possible, also complicate the interpretations of the dye patterns as will be discussed later. Nevertheless the authors have presented and interesting and valuable piece of research that after addressing the technical comments and polishing up the writing should make a decent contribution to *Hydrology and Earth System Sciences*. My comments are offered in the spirit of improving this paper and bringing it forward to publication. Major general technical comments that need to be addressed by the authors:

1. **Frequency of hillslope excavations and the limitations these impose on interpreting lateral subsurface flow paths and connections:** Based on my experience, a spacing of $\geq$1 m in adjacent trenches is not sufficient to draw meaningful conclusions related to the complex interconnectivity of the hillslope scale preferential flow network. Noguchi et al. (1999; *Soil Sci. Soc. Am. J.*) noted that few individual segments of macropores were $>$50 cm in length and demonstrated complex interactions amongst decayed root channels, loose soils, buried organic matter pockets, and bedrock fractures over scales of $<$1 m (also see Sidle et al., 2000 & 2001 *Hydrol. Process.*). Recent work at other forest hillslopes in Japan is confirming such small scale interactions. Thus, in the discussion of their results, the authors need to recognize and note that the $>$1 m spaced slices that they used may not reflect the complex connectivity of preferential flow processes at the hillslope scale. This is briefly acknowledged on pg. 7, in the 2nd sentence of the “Results", but from there on the authors do not consider this important limitation. Thus, when discussing the dye cross-sections (i.e., Fig. 3) there needs to be a recognition that other connective “features" (i.e., not mapped in the excavated trenches) could have strongly influenced the downslope staining patterns. In fact there is little evidence in the dye patterns that pipes continued for any considerable length within the hillslope. As such, a theme emphasized in this paper (and one that has been inappropriately perpetuated by others) that a “rising water table" is the primary means of facilitating the connectivity of preferential flow networks is not supported by the dye pattern data – see statement by the authors on pg. 10 (last paragraph) continuing to pg. 11. This either needs to be removed or put into the context of the limitations of these data. Some of the references cited to support the concept of a rising water table connecting preferential flow paths (bottom pg. 10; Tsuboyama et al., 1994 *WRR*; Sidle et al., 2000 *HP*) did NOT support this concept, rather they noted discontinuities in saturated zones in the soil and proposed that “connectivity" of preferential flow networks was supported by increasing antecedent soil moisture. This misquotation must be clarified.

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2. **Use of Brilliant Blue dye and its application:** Although the limitations of visualizing Brilliant Blue dye in dark forest soils has been recognized by other researchers and by the authors in this study (2nd sentence of section 2.2; pg. 2nd paragraph), it was employed to articulate connectivity and extent of preferential flow paths. Nowhere in the results and discussion are such limitations noted. Certain pathways and interconnections in dark portions of the soil could have been missed. Also, what about possible exchange of flow into and out of the bedrock or underlying till (in fact there is NO mention at all of these substrate characteristics – **a major omission**)? Also, the method of applying the dye in solution in a trench (i.e., ponded water) actually encourages water to enter preferential flow paths and can augment preferential flow above levels that would otherwise be experienced during natural rainfall or snowmelt. This limitation should be noted. On pg. 5 (9th line from the bottom) you cited the Noguchi et al. (1999; Soil Sci. Soc. Am. J.) paper and implied they used the same methods as you did – Noguchi et al. did not use a trench with ponded water, rather they sprinkled white paint solution along a line source (quite different application at a realistic rainfall rate). Please correct this.

3. **Quantification of dyed “slices" of soil:** The approach used to “quantify" Brilliant Blue dye in the soil could have been more quantitative. No evidence of concentrations are presented. The only graphic presentation of data appears in Fig. 3 which is a rather inadequate representation of these important data. Also, there are no supporting photographs to show the preferential flow pathways and their linkages – this would have been very useful. I suggest that the authors rework the data included in Fig. 3 and present this in a more quantitative, spatially explicit manner which attempts to establish the links between various sections (slices) with a concurrent discussion of the potential problems of this method whereby slices are taken at 1-m intervals (see previous comments). As it stands, this “analysis" is not so convincing, but I think that the authors have the

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data to make this far more quantitative and explicit. Maybe these data need to be presented in two separate figures: one that provides an overview of the entire slope “system" (similar to Fig. 3, but with more detail and better coordinates, and possibly on “one line" – i.e., continuously from the top to bottom of the slope).

4. **Influence of microtopography:** I think the authors somewhat discount the influence of microtopography related to their staining results and instead put most emphasis on contributing area – certainly the two are a bit interrelated. However, in many forest hillslopes, I have seen relatively large pipes emerge in the longitudinal axes of hollows or depressions. These have been reported in other studies (e.g., Lin, 2007 *Vadose Zone J.*; Negishi et al., 2007 *HP*; Terajima & Sakura, 1993 *Trans. Japanese Geomorph. Union*; Terajima et al., 1997, *Earth Surf. Process. & Landforms*). Often times such pipes are NOT connected over long slope distances, rather they arise from contributions of a network of preferential flow paths that converge in these hollows or concave portions of hillslopes. Your data actually does not support the idea of long continuous pipes and may be better explained by this concept of a converging network of preferential flow paths (see Sidle et al., 2000, 2001 both in *HP*). In any event, topography appears to play a major role in your findings, yet it is understated in your paper (in my opinion). Places in the manuscript where you could have referred to the influence of topography include: pg. 3, 12th lien from the bottom; pg. 9, last complete sentence; pg. first half of this page; and more specific emphasis in the Conclusions on pg. 13, 2nd last sentence.

5. **Interpretations of pipeflow:** In my opinion the authors place the wrong focus on interpreting the emergence of dye from a soil pipe in x-section 18 near the application source. This very interesting observation is first noted in the first paragraph on pg. 9 and later in the following places: pg. 10, last part of 2nd paragraph; and the first half of pg. 12. Finally in the last paragraph of pg. 12 it is noted that preferential flow paths could be “blocked", but this idea is not incorporated into

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other discussions related to this interesting preferential flow observation. This seems to be a major reason why many of the even larger macropores are not continuous over long slope distances. Also, earlier in the paper (pg. 3, middle paragraph) there is no mention of the importance of "dead-end" preferential flow paths related to slope stability. A number of papers and books have noted the importance of these: e.g., Brand et al., 1986 *Quart. J. Eng. Geol.*; Tsutsumi et al., 2005 *WRR*; Uchida et al., 2001 *JOH*; Sidle and Ochiai, 2006, *AGU Water Resources Mono. 18*); such important factors (i.e., dead-end pipes) should be mentioned and emphasized related to your findings in trench 18.

Other specific issues that need to be addressed by the authors:

- I disagree with the statement made in the Abstract the "no studies have determined how individual features are hydraulically connected at a hillslope scale"; this is exactly what Sidle et al. (2001, *HP*) propose. The unique feature of your study is that you collected data over a longer hillslope.
- Pg. 2, line 6: The Uchida (2004) reference is a very poor choice; this is a "commentary. Please cite some of the wealth of primary research papers.
- Pg. 2, line 7: If you wish to site earlier studies of the importance of preferential flow in forest soils, I suggest adding the following important studies that have been ignored in soil hydrology, but which were some of the earliest studies noting the importance of preferential flow (in addition to Mosely's paper): Aubertin, G.M. (1971) Nature and extent of macropores in forest soils and their influence on subsurface water movement. USDA Forest service Res. Paper NE-192, Northeastern Forest Exp. Sta., Upper Darby, PA, 33 p. and Sidle R.C. and L.T. Kardos (1977) Transport of heavy metals in a sludge-treated forested area. J. Environ. Qual. 6: 431-437 and Sidle, R.C., L.T. Kardos, and M.Th. van Genuchten (1977) Heavy metals transport model in a sludge-treated soil. J. Environ. Qual. 6: 438-443.

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- Pg. 2, line 13: I know that the scale of the Noguchi et al. (1999) study was 2 m (I am not certain about the others cited), so why do you say “less than 2 m"?
- Pg. 2, 3rd line from bottom: The McDonnell (1990) reference does not fit well here; he assumed a long, continuous pipe that was intersected by a rising water table. Now we know this did not happen. The other paper (Sidle et al., 2001) notes that smaller preferential flow paths are interconnected due to increasing soil moisture – this is quite different.
- Throughout the paper, I prefer the term “preferential flow paths" rather than “preferential features" (less descriptive).
- Pg. 3 middle paragraph – the Sidle and Ochiai (2006) AGU Water Resour. Mono. Provides a good summary of the role of preferential flow paths related to slope stability (please see pgs. 72-74).
- Pg. 3, line 19, may want to add “and more connected features **especially in concave topography**".
- Pg. 3, lines 19-21: Definitely the Sidle et al. (2001) study specifically identified such physical connectivity of preferential flow paths. Also see the applicability of this study related to the first sentence in the last paragraph on pg. 3.
- Pg. 4, 1st paragraph: A 30% slope is not steep in terms of slope stability!
- Pg. 4, 2nd paragraph: “herbal vegetation" is far too vague; be more specific.
- Pg. 5, 2nd sentence (and elsewhere): do not capitalize the common names of species (unless a proper name).
- Pg. 6, 1st paragraph: You need to better characterize the spatial distribution of the 16 x-sections.

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- Pg. 7, 2nd sentence of Results: This points out a big problem related to the >1 m spacing of trenches.
- Pg. 7, Section 3.1 (first sentence) Replace “generous" with “deep".
- Sections 3.1, 3.2, and 3.3: I do not see the need to present this information in separate sections. In fact if this was better written, it could it would help the reader visualize this setting and findings better.
- Pg. 8, Section 3.2: please reword the second last sentence.
- Pg. 9, 1st sentence: This is the evidence that this was a dead-end pipe!
- Pg. 9. Section 3.4 (last half): This points out some severe disadvantages of using Brilliant Blue dye in forest soils.
- Pg. 9 & 10, 1st paragraph of 4.1: The Uchida (2004) commentary is not a good choice here; please site original research instead of these. As such the first sentence in this section is very weak unless you can support it by some research study. In the 2nd sentence you should note that subsurface erosion involves the enlargement of pipes. In the second to the last sentence (pg. 10) you should contrast flat topography with the greater propensity for pipes in concave topography – i.e., not enough emphasis on the importance of concave topography.
- Pg. 10, lines 12-13: this sentence (“As features...") is very much supported by the Sidle et al. (2001) paper in *HP*.
- Pg. 10, lines 13-15: Such subsurface erosion is also know to extend channels and initiate gullies (by collapse).
- Pg. 10-11, 4.2 (first paragraph): I would say “as wetness increases" not as a water table rises – this is what the Tsuboyama et al., 1994 and Sidle et al., 2000

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papers state. Thus, I think you must restate the conceptual model accordingly related to your excavations. Also, I think that the – 1 m slices that you employed were probably not close enough to support your discussion of the “linkages" noted in the last part of this section (top pg. 11).

- Pg. 11, lines 7-10: Yet other studies have questioned this simplicity.
- Pg. 11 last paragraph, 2nd sentence: Delete the Uchida 2004 commentary and cite some of the papers suggested earlier in this review related to slope stability.
- Pg. 11 last paragraph, 3rd sentence: this may or may not be true – no proof.
- Pg. 12, 1st sentence: As I noted earlier, Sidle and Ochiai (2006) give a good review of effects of preferential flow related to pore pressures that can induce slope instability.
- Pg. 12, lines 4-11: Here is an example of where you need to invoke the idea of dead-end pipes related build up and dissipation of pore pressure. Your discussion leaves much to be desired.
- Pg. 12, 2nd paragraph, 1st sentence: replace Uchida (2004) with a primary reference.
- Pg. 12, near the bottom: You could also mention that a “likely outcome" could be return flow via a soil pipe that emerges at the surface.
- Pg. 13 – Last section heading should read “Summary and Conclusions"; actually the conclusions are a bit scant.
- Pg. 13 Conclusions 3rd to last sentence: you should specify “concave" topography.
- Pg. 13 3rd line from bottom: do you mean “anonymous"?

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Interactive Comment

I believe that once these issues outlined in this review are addressed, this can be a suitable contribution to *Hydrology and Earth System Sciences*. The paper will naturally require additional review by the Associate Editor and possibly an additional outside review. However, I think that the information presented in this paper is unique and important and should be published. I certainly encourage the authors to make the necessary changes to bring this important information to the attention of the vadose zone community.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1043, 2008.