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Hydrology and Earth System Sciences Discussions

Interactive comment on "The significance and lag-time of deep throughflow: an example from a small, ephemeral catchment with contrasting soil types in the Adelaide Hills, South Australia" *by* E. Bestland et al.

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The authors describe a study in which isotope and chemical data were used to improve the conceptualization of how an ephemeral catchment re-wets from autumn to winter through a series of rain events. The authors have some interesting observations about flow paths and the contrasting hydrologic roles of clay-dominated vs. sand-dominated soils. In particular, I believe that the observations about the role of different regolith materials are the most novel data presented and justify publishing this paper in HESS. However, I do have some concerns with this paper as currently presented. First, some

of the presentation would benefit from a clearer focus. For example, a conceptualization of the regolith and throughflow is referred to in the abstract, but never adequately described in the paper. Secondly, the authors present data on the timing of changes in Al and Fe concentrations. I disagree with their interpretation of the source of these peaks in metal concentrations. I detail my concerns below beginning with those of major concern, followed by some minor concerns, and finally just a few typographical errors at the end.

Improving the focus of the paper –

In the abstract on lines 18-19, the authors say that the flow system has been re-thought based on these new data. It is important in the main text to describe how the flow system was viewed (and what previous studies or data support this view) before this study began to provide some perspective to the reader. And I do note that there is some information in this regard in the "Background" section, but some additional explanation is needed. In particular, describing the previous beliefs related to the hydrologic roles of the different regolith types is an important item to discuss here.

I believe that the paper would be improved by removing some material that is superfluous to bring better focus to the most important and interesting data related to the lags in throughflow and the relative roles of sand vs. clay. This could be accomplished by eliminating Tables 1 and 2 (and possibly 3 as well) since these data are already shown in figures. Additionally, some of the discussion of mineral weathering and colloid sources could be reduced since this paper is not really an exploration of the geochemical sources of these elements but rather the use of the elements as hydrologic tracers.

The authors need to describe more clearly early in the results section exactly what they mean by Phase I, II, and III of the catchment re-wetting. These phases are shown in many of the figures, but never really explained and conceptualized in the text.

Many of the relationships the author show with chemical constituents would be more

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easily visualized if molar rather than mass concentration units were used. For example, in Fig. 6A the 1:1 relationship expected from NaCl and deviations from this line would be more clearly evident in molar space.

The cause of the peaks in Al and Fe concentrations – The authors show these peaks in Fig. 7, and discuss these results on pages 2612 and 2613 of the text. They hypothesize that these peaks result from colloid transport, and that the colloids were not removed by the filters used in this study. I tend to agree with this explanation, but not the authors' hypothesis that these colloids were most likely flushed from sandy soil horizons. This explanation is not consistent with the lack of peaks in Fe and Al during the April and May rainstorms. A far more likely explanation, and one that would be consistent with the lack of peaks in April and May, is that these colloids are mobilized within the stream channel itself as flow rises in the channel to heights that were not reached in the previous several months. This is a common observation in streams, seems a far simpler and more likely explanation than a source in shallow sandy soil horizons, and is consistent with all of the data collected in this study. In contrast, the peaks in DOC and K are more likely to originate from shallow subsurface flow paths, consistent with the offset of these peaks from those of Al and Fe.

The source of pre-event water –

I wanted to respond to a concern raised by the anonymous reviewer whose comments appear on pages C500–C507 as to whether the authors have convinced the reader that the pre-event water is largely from the earlier April-May storms and not largely from storage that is much older. I shared this concern when first reading the paper, but believe that Fig. 9 can be used to dismiss this concern. If a substantial amount of the pre-event water originated from vadose water stored from previous winters, I would think that a strong deviation from the meteoric water line reflecting a heavily evaporated signal would be evident, and I do not see strong evidence of evaporation in this figure. Therefore, I feel there is good support for the authors' assertion of the importance of the earlier April – May storm water in the later June-July storm.

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Interactive Comment

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Items of minor scope and concern –

Abstract – The number and sequence of rain events as described in the abstract is confusing. June and July rain events are suddenly discussed without any prior reference. A simple statement at the beginning of the abstract about the rain events and sequence that were sampled would help the reader.

Page 2602, lines 3 and 4 – The assumptions regarding the negligible role of vadose zone water as described in the Sklash and Farvolden work of the 1980s were later questioned and challenged by the work of J.J. McDonnell in the 1990s in New Zealand and much work since then. I would say that a more accurate portrayal of thinking since the 1990s is that the vadose zone generally holds a large store of moisture that can be mobilized by various mechanisms, and is viewed today as a major contributor to storm runoff.

Page 2603, lines 1- 2 – Doesn't lateral subsurface flow move mainly by advection and not diffusion through soil? Page 2604, line 8 – Could the authors briefly describe the principal vegetation types?

Page 2614, line 25 – Doesn't this also reflect the lower saturated hydraulic conductivity of the clay soil to a greater extent than just the "higher saturation" of the clay?

Page 2616, line 12 – Should add DOC here as well as potassium.

Fig. 1 - I share the other reviewer's concerns about the clarity of Fig. 1. In particular, could the figure be colored to show clay vs. sand-dominated regolith in a more straightforward manner?

Fig. 4 – Should define the lines in the caption as in the other figures. Don't need to repeat "EC" before A and B since it is mentioned at beginning of caption. Grammatical and typographic errors –

Page 2604, lines 23-24 – The sentence that begins with This site is... needs to be re-written.

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Page 2606, lines 8 and 9 – I do not know what a McCartney bottle is, but want to make sure it is a glass bottle that does not allow evaporation during storage. Please use a descriptor for the bottle as opposed to a brand name.

Page 2609, line 2 and other places – Since chlorine is usually in the chloride ion form in waters, the term chloride is preferred to chlorine, which implies the dissolved gaseous form, which was not measured.

Page 2609, line 2 and elsewhere – The word “trends” is used, which typically implies that a temporal change is being evaluated. I think the authors ought to use a more appropriate and less confusing term such as “patterns” or “distribution”.

Page 2610, line 4 – Change “And” at beginning of sentence to “An”.

Page 2615, line 17 – The words “able to be” can be eliminated.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2599, 2009.