Hydrol. Earth Syst. Sci. Discuss., 11, C1872–C1878, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C1872/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Stable water isotope tracing through hydrological models for disentangling runoff generation processes at the hillslope scale" *by* D. Windhorstet al.

M. Hrachowitz (Referee)

m.hrachowitz@tudelft.nl

Received and published: 16 June 2014

This is a very interesting and well developed paper. The only real concerns I am having is that the limitations of the Richards equation based model is only discussed very late in the manuscript and that the referencing could be more detailed, as there are quite some rather recent studies that address and discuss exactly these limitations of the Richards equation (i.e. the assumption of a continuum and thus the omission of preferential flow paths), using different modelling strategies. Apart from that I would be very happy to see this manuscript eventually published. Please find below my detailed comments.

1) P.5181, l.7 and elsewhere: in science (except for mathematics) verification is extremely problematic. The most that can usually be done is to test hypotheses and falsify them. Thus please avoid terms such as "verify".

2) P.5181, l.9: unfortunately we are not yet far enough to be able to predict mixing processes. Please rephrase

3) P.5181, l.25-29: what about Kirchner et al. (2001)? They could plausibly show that advection-dispersion processes can be responsible for observed tracer signals along hillslopes.

4) p.5182, l.17: what do you mean by a "constrained model"?

5) p.5182, l.17-20: perhaps include McMillan et al. (2012) and Hrachowitz et al. (2013) as they did exactly that on the catchment scale.

6) p.5183, l.6: please define "presaturated"

7) p. 5183, l.9: is the "organic horizon flow" matrix or preferential flow dominated?

8) P.5183, l.15: how was the saturated hydraulic conductivity measured? If it was from a soil core sample (i.e. relatively small size) that it should be noted that it is likely not to represent the effective hydraulic conductivity that would also to some degree account for preferential flow (as the sample would be too small to get a representative distribution of these flow paths, e.g. macropores) but rather merely the matrix conductivity.

9) P.5183, l.28-29: 2.1 and 3.9 years are by no means "long" mean transit times. I suppose you wanted to say that they are longer than the MTT of faster flow paths. Please tone down.

10) P.5184, l.8-11: I do not agree with this statement as you essentially only exchange one information for another one. In other words, here the model is calibrated to observed soil water composition while in many other modelling attempts, models are calibrated to some measures of flow or other observed storage changes. Here you do

not have much indication if the model actually reproduces flow in a meaningful way. One could only be more confident about the model if it was able to reproduce both, flow and tracers. Thus please omit this statement

11) P.5184, l.27 and elsewhere: I suppose you refer to Hortonian overland flow here (as opposed to saturation overland flow). Please clarify.

12) I found the introduction in general a bit too long and too detailed. Some parts of it would better fit into study area or methods sections. I would thus encourage you to shorten and revise this section.

13) Methods section: no mention is made of the precipitation sampling strategy. Thus please add sampling method, sampling frequency and length of sampling period. In addition, was fractionation considered in the precipitation samples?

14) Section 2.6.1: I think it would be beneficial for the manuscript if you already here included a statement of the limitations of the Richards equation (i.e. preferential flow not accounted for) because this has also consequences for the interpretation as a completely mixed assumption will not hold for most systems where preferential flow is relevant (i.e. in a simplistic way the problem could be approached as a "Dual flow domain") as preferential flow is not a continuous process and thus alters the mixing process according to antecedent wetness. Thus please include Brooks et al. (2009) and Hrachowitz et al. (2013) who show the importance of temporally varying mixing processes. In addition, it would be good to bring this into wider context with previous work (e.g. Weiler and Naef, 2003; Van Schaik et al., 2008; Vogel et al., 2008; Legout et al., 2009; Koeniger et al., 2010; Stumpp and Maloszewski, 2010; McDonnell and Beven, 2014)

15) P.5191, l.3-5; Table 4: are these the prior parameter distributions? Are they uniformly distributed? Please clarify

16) P.5191, l.17: please specify more clearly "best" parameter sets. Are these all

retained as behavioural? Or just a sub-set?

17) P.5191, l.25; Figure 3: is "level" here equivalent to depth below ground? Please clarify. Are these really confidence intervals or merely the 2.5/97.5 percentiles of all values from the behavioural parameters? If yes, how were the confidence intervals calculated? Was some kind of weight or likelihood measure applied (e.g. Freer et al., 1996)?

18) P.5192, l.9ff: I think it may be a good idea to tone the discussion of hypothesis I down a bit, as the given interpretation remains a bit speculative. For example, the effect of different mixing processes in the soil together with the presence of preferential flow paths could have introduced compensatory errors in the model, which may also propagate into the representation of evaporation.

19) P.5193, l.24: Figure 4: not sure why A1, A2 and A3 are each represented in 2 panels. Please clarify. Also, how were the box plots constructed (see comment 17)

20) P.5194, l.4-6: this does not really come as a surprise as there is typically very little fluctuation in the groundwater isotopic composition due to elevated storage capacites and therefore low turnover. The model can thus not fully discriminate between different parameter values (e.g. Dunn et al., 2008; Hrachowitz et al., 2009)

21) P.5194, l.6-10: why? What is exactly happening at 2m? It seems a bit peculiar to me that at exactly 2m you start getting behavioural models. Please clarify.

22) P.5194, l.16-18: I am a bit concerned that this can be justified as no information on flow is available to be compared to and the model was only calibrated on tracers (see also comment 10). Please tone this statement a bit down and clearly state the limitation.

23) P.5194, l.22: it will depend on the wetness state of the soil and on the question if the macropores are filled. In other words, this statement may hold for wet conditions, when there is actually water in the macropores. Under dry conditions most of the water

will be stored in the matrix and thus also sampled from the matrix. Please reconsider this statement.

24) P.5194, l. 23-26: This is pretty well established in literature. Thus please include some more recent and relevant references here (McDonnell et al., 2007; Brooks et al., 2009; Beven, 2010; Hrachowitz et al., 2013; McDonnell and Beven, 2014).

25) P.5195, l. 8-10: please also include some more recent references here (e.g. Vogel et al., 2008; Stumpp and Maloszewski, 2010).

26) P.5195, l.22-25; Figure 5: not sure how the standard deviation was computed. Were likelihood weighted values used? See also comment 17

27) P.5196, l.9: see comment 9

28) P.5196, l.13 and elsewhere: I am not sure what is menat by "subsurface flow" here. please define: is it shallow groundwater (i.e. Darcy; celerity and pressure head driven) or is it rather understood as preferential flow (velocity and elevation head driven)?

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