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Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial distribution of solute leaching with snowmelt and irrigation: measurements and simulations" *by* D. Schotanus et al.

Anonymous Referee #1

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Review of HESS 9-13451-2012 Field-scale leaching of a conservative tracer and an organic chemical was measured using a multi-compartment sampling device. Solute breakthrough was measured at a high spatial resolution for different flow regimes (snowmelt versus irrigation). The solute transport experiment was then calculated with a 2D variably-saturated water flow and solute transport model; random fields of a lognormally distributed scaling factor were used to mimic spatial variability in hydraulic properties. By trial and error the geostatistical parameters for hydraulic functions best describing the leaching data were identified. Additional flow boundary conditions were imposed during modeling to explore the effects of flow conditions on solute leaching.

This paper presents an interesting data set and associated calculations on a subject of broad interest to HESS readers. However, the experimental and numerical analysis are not really new as another paper on the same subject was recently published by the same authors (Schotanus et al. 2012) – also, similar work has been reported using wick samplers (eg Seuntjens et al. 2001 JContHydrol 51:13-39) with the main difference that the spatial resolution of their MCS is a bit higher than that of others (suction regulation with wick samplers is also possible). The main limitation of the current paper is its lack of detail in explanations and descriptions of assumptions, hypotheses, etc. Indeed, many statements are insufficiently underpinned: for example, comparison between flow heterogeneity for snowmelt and irrigation is hampered by incomplete description of the experimental setup, insufficient [basic] knowledge of soil hydraulic properties. I recommend to study solute leaching in each collector separately and try to calculate the pore-water velocity from moment analysis (Veffective); if this is compared with the application rate divided by an average water content (v0), the ratio Veffective/v0 is an indication of the degree of preferential flow (see eg. Mallants et al. 1996 – Hydrological Processes 10:55-70). The paper further lacks clarity and transparency in describing assumptions and hypotheses. Many of the conclusions are not supported by comprehensive data or model output. It is not clear how a difference in soil hydraulic properties can be the reason for a different flow regime when the same experimental setup is used. There is mentioning of uncomplete BTCs; what does that mean exactly? It needs quantification in terms of solute mass balance. When a single realization approach is used, inference of statistical moments of the distribution of relevant variables requires a flow domain that is sufficiently large compared with the correlation scales of the pertinent formation properties. In addition, in order to preserve details of the spatial structure of the formation properties, the size of the numerical cells must be small compared with the characteristic length scale of the heterogeneity of the relevant formation properties. Based on the criterion of at least four nodes per correlation length suggested by Ababou [1], the horizontal discretisation is too large compared to the value of the horizontal correlation length. Furthermore, the domain size should be at least 10 to 50 times the correlation length; the vertical domain size is too small (1.5 m versus a required 6 m if vertical correlation length is 0,6 m). There are many more comments on the annotated pdf-version of the manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C6415/2013/hessd-9-C6415-2013-supplement.pdf

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Interactive Comment