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Discussions

Interactive comment on "Surface water and groundwater: Unifying conceptualization and quantification of the two "water worlds"" *by* Brian Berkowitz and Erwin Zehe

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We sincerely thank Anonymous Referee 2 (AR2) for his/her comments on our manuscript. We respond below to the individual points.

AR2: The authors present a contribution stressing the observation that surface and subsurface systems should be described by a single model, since both systems are "a manifestation of self-organization". While (of course) I agree with the obvious observation that surface and subsurface systems are governed by the same physical principles (conservation of mass, momentum and energy) I do not agree (in general) with the observation that a "single model" can efficiently and with the same level of accuracy

capture the behavior of all systems. Taken to the extreme: we all know that the Navier-Stokes (NS) equations can describe incompressible fluid flow in (simple and complex) systems. However, direct solution of the NS equations is typically not feasible (in general) for turbulent flow or flow in large aquifers. This is why, in various disciplines and with reference to specific topics, diverse (simplified) models/approaches have been developed, both with reference to surface and subsurface flow conditions.

RESPONSE: We respectfully disagree with the principal claim in this comment, namely the referee's assessment that we stress "surface and subsurface systems should be described by a single model". We make no such argument anywhere in the manuscript. Predictive modelling of a hydrological system requires essentially solving the mass, energy, momentum and entropy balance equations. We agree with the reviewer that one might use, e.g., approximations of the Navier-Stokes equations for the momentum balance in turbulent open channel or overland flow that are different from those for porous media flow. Indeed, at the outset (second paragraph of section 2), we pointed out that surface flow velocity is proportional to the square root of the potential energy difference, while it is proportional to the head/potential energy difference in the subsurface. In the revised manuscript, we will further expand the text to consider this point more fully.

Hydrological modelling (and hydrological theory) attempts to predict how processes described by equations evolve in and interact with a structured heterogeneous domain (i.e., hydrological landscape). Thus, our key observation that both systems are a manifestation of self-organization does not imply proposed use of a single model. Rather, we argue that similar conceptualizations and methods of quantifications – whether related to preferential flow paths, dynamics and patterning of chemical transport and reactivity, and characterization in terms of energy dissipation and entropy, for example – can and should be applied to both surface and subsurface systems, to the benefit of both research communities. In the revised manuscript, we will ensure that this point is clear throughout the text, particularly in the introduction and concluding sections.

AR2: Then, the authors stress the ability of Continuous Time Random Walk (CTRW) to describe non-Fickian transport in heterogeneous (surface and subsurface) systems. CTRW has a long history (it was originally introduced by Montroll and Weiss (1965), to the best of my knowledge). It has been widely used by the subsurface hydrology community and it allows including the impact of heterogeneity on transport. The model fully depends on the pdf of transition time (i.e., the weighting time in between two jumps). This pdf is an input function in CTRW; it has to be known "a priori" and it is (usually) modeled as a truncated power law, thus embedding fitting parameters. Here, the authors argue that CTRW could be used also to simulate transport in surface systems. Indeed, several studies along this line have already been presented in the literature, as also acknowledged by the authors, albeit not at the catchment- scale.

RESPONSE: With regard to our discussion of the CTRW — First, the history of CTRW is reviewed in detail by Berkowitz et al. (Reviews of Geophysics, 2006), as cited in the manuscript; since the referee comments on this, we state here that a random walk with continuous time was introduced by Montroll and Weiss (1965), but the generalization of the formalism to a joint pdf in space and time, labeled "CTRW", and with the physical application to transport, was first given by Scher and Lax, 1973). As noted by the referee, CTRW (introduced to the field of hydrology by Berkowitz and Scher, 1995, 1997, 1998) is now used widely by the subsurface hydrology community, as it accounts well for the impact of heterogeneity on transport. The referee is correct in stating that the governing pdf is an input function in CTRW, which requires fitting parameters. We do not see this as a "criticism", given that every model requires fitting parameters. Ideally, these parameters can be determined from various types of a priori information; indeed, such analyses are incorporated in many types of modelling studies, and they have also been implemented to determine the relevant CTRW parameters.

The referee then continues by (correctly) noting our argument that CTRW could be used also to simulate transport in surface systems. We agree (and cite relevant literature) that several studies along this line have already been presented in the literature, albeit not at the catchment scale. Again, we do not see this as a "criticism" of the manuscript.

If the referee intends that these comments are an indication of lack of novelty, we address this concern in the next point.

AR2: In summary, I do not clearly see the novelty of the present contribution. No original works/results are presented. The manuscript looks like an "opinion" paper where the authors present an overview of previous work in surface and subsurface systems (with particular emphasis to CTRW approach). The "novelty" should be the suggestion of future works where CTRW could be applied to simulate the transport feature at the catchment scale. This observation appears not at all surprising to me. CTRW is a tools allowing to embed the effect of the heterogeneity of the system on transport features via the use the transition time distribution, regardless the system considered (surface of subsurface). The drawback of this approach is that the pdf of the transition time must be known "a priori" as well as its parameters (that are fitting parameters and must be estimated via available data).

In conclusion, given the flavor of the study (at least the way it is perceived through my analysis), my suggestion would be to reconsider the scope of this contribution. This can be achieved it by framing it in the context of a review or, probably better, an opinion paper. This is the spirit with which I would recommend a set of major revisions.

RESPONSE: We (the two co-authors) discussed this point repeatedly – "opinion paper" or "research paper" – and concluded that while this evaluation is somewhat subjective, the manuscript is best characterized as a research paper. We are gratified, too, that referee 1 and the author of the "short comment" were evidently satisfied with classification of the manuscript as a "research paper". Throughout the manuscript, we synthesize literature, conceptualizations and understanding from the generally separate surface water and groundwater communities. This is much more than an "opinion" or straightforward "review". In doing so, we provide new insights of "commonalities" in the two water worlds, in terms of preferential paths, entropy, and energy dissipation. In terms of the discussion of CTRW, the manuscript offers new analysis and insight showing that one definition of the CTRW power law form of the pdf is actually an inverse Gamma distribution, which meshes beautifully with previous work showing use of an inverse Gamma rather than a Gamma distribution to characterize transition times and the resulting travel time distributions of chemical tracers. (We address the comment regarding "fitting parameters" in the previous "Comment/Response", and do not repeat here.) Moreover, in terms of the CTRW framework, we stress that the work of Edery et al. (2014) clearly reveals a link between the power law exponent and the underlying medium characteristics. This implies that the fitted parameters are a macroscale fingerprint of spatial media characteristics that determine the temporal arrival of chemical species. While we do not expect that this relation is unique, it does imply that "fitted" parameters have a physical meaning that can be used to constrain characteristics of the domain (i.e., the hydrological landscape mentioned above) in a spatially distributed model. This goes clearly beyond the mere fitting of meaningless/empirical parameters. In the revised manuscript, we will also include a specific (and unpublished) entropy calculation to quantify the preferential paths that control tracer migration. Thus, while our manuscript is not a "traditional" research article ("Methods and Materials, Results, Discussion"), it (i) synthesizes an array of diverse methods for quantification, (ii) offers new insights, (iii) presents new quantitative analysis, and (iv) sets a framework for future research.

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