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Hydrology and Earth System Sciences
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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In the manuscript "Surface water and groundwater: unifying conceptualization and quantification of the wo water worlds" by Berkowitz and Zehe, the authors aim to raise awareness for the unjustified and unnecessary distinction between surface and groundwater hydrology. They make the case that both systems are controlled by largely the same principles – one of these principles being the energetically necessary development of preferential flow paths.

The manuscript touches a highly interesting topic and it has, without doubt, the potential to become an important reference for future studies. Although I really like and

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appreciate the overall intention and objective of the manuscript, I also feel that the manuscript could strongly benefit from the authors taking a step back to re-reflect some parts of their work.

(1) Throughout the manuscript it remains somewhat unclear what the authors want to convey. While some parts of the text read like a very interesting, yet general reflection on the structural similarity of surface and groundwater systems and the general principles behind that, other parts are very technical descriptions of one specific aspect (i.e. power law transition distributions) of groundwater and potentially surface water systems. Both parts are very interesting, but there is little clear coherence between them in the text. It therefore remains somewhat vague, how and if power law transition distributions are in detail linked to the formation, evolution and function of preferential flow paths in surface water systems and how and if they can be expected to improve representation thereof in models.

(2) Linked to comment (1), it is difficult to discern from the text what the original and novel contribution of this manuscript is. In other words, it remains unclear if the manuscript is intended to provide a review of the state-of-the-art together with guidance towards promising future research avenues or if the thermodynamic considerations and the proposed power law transition distributions are a novel development that is introduced here for the first time (which I do not suspect). I therefore strongly encourage the authors to more clearly distinguish between existing literature and potential novel contributions.

(3) I strongly agree with the authors that eventually the surface- and groundwater communities need to converge towards “unified” conceptualizations – we are talking about the same physical system after all. However, and in contrast to the authors, I believe that different modelling approaches are not mutually exclusive. Rather, they should all be embraced and exploited to their fullest to learn about the system. In other words, I think, different modelling approaches are complementary in what can be learned from them. Here, the authors provide a beautiful example of how theoretical considerations

and “physically-based” models can teach us about real world systems. Their findings that power law transition distributions may be more suitable than other, currently used distributions can be of considerable interest for other modelling approaches. I think it would therefore be very helpful to emphasize this complementary aspect. Now large parts of the manuscript read like as if typical top-down model approaches cannot deal with the celerity-velocity dualism, nor that these models could resolve the incomplete mixing. I strongly disagree with this notion. While it is true that simple, lumped convolution integral approaches have limited use, they are quite outdated and cannot be seen as state-of-the-art anymore, for reasons also highlighted by the authors in this manuscript. That they are still widely used in the community is a different problem. On the other hand, the authors claim that conceptual multi-box models similarly, cannot represent the system nor reproduce its dynamics in terms of both, water and chemistry. I disagree with this opinion in the strongest terms, as there are many papers, in particular over the last 5-10 years, in which the opposite was shown. I agree that standard conceptual box models cannot simultaneously reproduce water flows and solute concentrations (and thus water/solute age distributions). However, as already shown almost 20 years ago by Seibert et al. (2003; HP), the addition of “mixing” assumptions and hydraulically “passive” mixing volumes has demonstrated great ability in resolving this problem (and thereby the “old water paradox” – which is not a paradox anyway, really). Of course, many of these papers (e.g. Shaw et al., 2007, JoH; Fenicia et al., 2008, WRR; Birkel et al., 2010, HP; Dunn et al., 2007, WRR; McMillan et al., 2012, WRR) started with the assumption of complete mixing in the individual reservoirs. This was done not out of conviction but rather in absence of more detailed information. However, soon it was realized that complete mixing in the presence of preferential flowpaths is an unsuitable assumption. Subsequent work therefore adopted the use of incomplete mixing at least for the unsaturated root-zone, and which could either be time-invariant (e.g. Hrachowitz et al., 2015, HP) or time-varying as a function of wetness conditions (e.g. Hrachowitz et al., 2013; HESS). In a parallel development, similar considerations were made using the SAS-function approach (e.g. van der Velde et al.,

2010,2012; WRR; Benettin et al., 2015a,b, WRR; Harman, 2015; WRR; Rinaldo et al., 2015, WRR and many others). Here, please note that the SAS-function approach is functionally and even mathematically (!!) equivalent to the mixing coefficient approach above, as described in detail in Hrachowitz et al. (2016; Wires). The only difference is in the choice of the SAS-function (piece-wise linear for mixing coefficient approach and typically gamma or beta distributions for SAS approach) and in the way the involved processes are described semantically. It is true that many applications(!) of the SAS-function approach limit themselves to overly simplified representations of hydrological systems. However, this does not mean that the general concept behind it is invalid. The application of different individual(!) mixing coefficients (and thus SAS-functions) for different individual(!) system components has, on the contrary, already proven very useful (Fenicia et al., 2010, WRR; Hrachowitz et al. 2013, HESS; Hrachowitz et al. 2015, HP; Hrachowitz et al. 2016, Wires). With the new information provided in this manuscript, a logical next step should thus be to check if these types of models can reproduce power law transit time distributions and if power law distributions would be suitable as SAS-functions in different components of the system (e.g. in the unsaturated root zone for drainage and evaporation; in groundwater for drainage). In fact in our paper Hrachowitz et al. (2013; HESS), using incomplete, temporally varying mixing in the unsaturated zone, we found that the system overall transit time distributions (i.e. TTD of the modelled combined outflows) had longer than gamma distribution tails. An observation that we could not make too much sense of at that time. However, it could fit very well into the reasoning presented by the authors here. in that sense, this would be a beautiful example of how different modelling approaches could learn from each other. It would make the manuscript much stronger if the authors emphasized how their findings could be helpful for other modelling approaches and if the authors invested some more effort in being more accurate in their description of what different model approaches can do. In the current description the authors seem to equate what “is” done with these models with what “can” be done. This is not a valid assumption. In fact we can do much more than is typically done.

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Minor comments:

p.3,l.96: what are “uncorrelated noise pattern”?

p.4,l.133: Hrachowitz et al. (2016) would fit nicely here as reference

p.4,l.146: not sure why this is referred to as “weak interaction” here. Given its importance for flow velocities and sediment transport, should friction not cause a “strong interaction” between water and solids?

p.5,l.161 and elsewhere: why only chemicals? This is also true for the behaviour of individual water molecules. The difference is that we can normally not tag and distinguish them. But this does not make the general process valid for chemicals only. There is also imperfect mixing of waters of different ages/provenance. Please try to be more precise in these formulations. In addition, please note that this is exactly what incomplete mixing assumptions and/or non-uniform SAS-functions try to mimic.

p.5,l.178: also depending on the pre-storm wetness conditions

p.6,l.203-204: I disagree with this statement. While it is true that conceptual models are often applied in a careless way, there is a wide body of literature that describes the necessity and value of treating models as hypotheses that need to be tested (e.g. Fenicia et al., 2014; Kavetski et al., 2011; Clark et al., 2011;2015), which, in an iterative approach, allows us to learn about the system. In addition, in Hrachowitz and Clark (2017) we argue that ideally, these catchment-scale, effective parameters should reflect real world numbers. The actual challenge is to find these numbers, which is far from trivial for many parameters, but which was shown to be feasible for others, e.g. Master-Recession Curve (Lamb and Beven, 1997) or the storage capacity in the unsaturated root zone (e.g. deBoer-Euser et al., 2016; Nijzink et al., 2016)

p.6,l.210: I also disagree here. these relations remain specific for the spectrum of environments they have been developed for. If they relations were developed using a wide range of different landscapes, as done for example for the determination of global

parameters in the mhM model (e.g. Samaniego et al., 2010) than they will also give us a more general picture.

p.6,l.219: No, I strongly disagree! Why should a parameter that describes the aggregated effects of heterogeneity be an abstract quantity? I think it rather provides the macroscale perspective and it can in some instances already be independently observed/estimated (see comment above about Master-Recession-Curve and storage capacity in root zone)

p.6,l.223-224: no, although there is without doubt some tendency to believe that parameter search can help, I do not share this. I rather think that, for all the uncertainties involved, we need to start with step-by-step limiting the feasible parameter space by identifying and eliminating solutions that are inconsistent with our data and our understanding of the system. That is essentially the opposite of finding the "optimal" parameter set and relatively independent of parameter search algorithms.

p.6,l.231-234: with all appreciation for the authors, but this is too bold a statement, which I need to consider as plainly wrong. First, these models *can* resolve the celerity/velocity dualism, when implemented as described above (mixing coefficients/SAS-function). And this, second, has already been demonstrated with a plethora of articles on combined modelling of water flow and (non-)conservative solute concentrations (e.g. chloride, nitrate, silica, ANC, EC, DOC and many others; see above references and references therein)

p.7,l.247-263: while Darcy-Richards does indeed, and probably rightfully so, receive criticism, I think these problems can be fixed within the near future (as also suggested by the list of improvements given here by the authors). I rather consider another point why these models receive much criticism: the fact that the sheer number of parameters needed can never be observed at the spatial resolution and scale(!!) of interest. Either we then need to use our scarce, existing observations to inform our model, in which case we will upscale homogeneity (as the spatial correlation fields of our system prop-

erties are unknown!), or we will need to calibrate these models, in which case we will obviously run into the problem of equifinality and our inability to meaningfully constrain our models.

p.7,l.264-265: why “alternative”? should these tracers not also be very informative and helpful to implement and test the above models?

p.7,l.264-270: true, but mostly outdated, as these approaches are too simplistic.

p.7,l.271-283: in this paragraph the authors seem to be confusing different approaches. At least it reads in a rather incoherent way. for example, Rinaldo et al. (2015) and Botter et al. (2011) describe the same general concept. Further, the system overall SAS-function can also emerge from the choice of SAS functions from individual components, which can be a calibration parameter or informed by observations/theoretical considerations as demonstrated by the authors of this manuscript. Finally, yes, the gamma distribution is often used, but as often other choices, such as the beta-distribution (e.g. van der Velde et al., 2012) or some piece-wise linear distributions are made (Hrachowitz et al., 2013,2015). In any case, and as mentioned above, these choices are not necessarily made out of conviction but rather due to a lack of more information and can be easily adjusted. In particular, it will be very help to get a better understanding how the tails of the distributions should look like.

p.10,l.364-370: unclear in how far this is different to what mixing coefficient/SAS-functions do.

p.10,l.375-378: I believe there is quite a good understanding in the community that there is no binary distinction between separated pools of water. The “two water worlds” need to be understood, prosaic literature terms, as a hyperbole – an exaggerated analogy, i.e. a pointed description of a concept.

p.10,l.379-380: perhaps better “drainage and evaporative fluxes”

p.11,l.431 and elsewhere: I am not sure if the term “chemical transport” is the best

way to express what you want to express here. why not keeping it more general to "transport", which then indeed is further divided into physical, bio-physical and chemical components in the transport processes.

p.12,l.454: conservative transport?

p.14,l.500: where can I see this?

p.15,l.500-503: sure, but would be good to introduce what CTRW is before shown results.

p.15,l.550ff: as also demonstrated by Hrachowitz et al. (2015 – sorry to again bring in one of my own papers here) using a conceptual model with mixing coefficients, where we showed that conservative (!) chloride transport is slower than water transport (evapoconcentration)

p.17,l.594ff: I really like this analogies here, but a stronger explanation of and link between spatial and temporal heterogeneity needed. Why are they different? What is going on there?

p.17,l.632ff, section 3.3: in how far is this different to SAS? This does not clearly come out here. I think it would be very interesting for the reader to clarify this. Is there a fundamental difference or is it only the choice of the transition time distributions?

p.17,l.646 and elsewhere: I noted the use of "transition" times instead of what is standard use in surface water hydrology: "transit" times. Was this made on purpose? Is there a difference? If yes, please specify. If no, please also clarify.

p.17,l.670: water molecules, too, are subject to both! It is just that its more difficult for us to observe

p.21,l.802-804: because that was the best information we had so far.

p.22,l.821: not sure this is correct ". . .sum of a gamma function(which is also a gamma function). . .". Please check if this makes sense. Do you mean the scaled incomplete

gamma function? Should it not rather reader something like:"the sum of 2 random variables that follow a gamma distribution can also be described by a gamma distribution"?

p.22,l.824: well, no. Not if suitable local (i.e. individual for each system component) mixing coefficients/SAS-functions are chosen.

p.22,l.839-841: I am not sure that that reference is a good example. First, the NSE is a very pessimistic performance metric in cases there is a low signal-to-noise ratio in the target variable (which is the fact in the Weiherbach: the stream isotope composition is strongly damped and essentially plots close to a straight line – any small deviation from that – error or real process – causes strong effects on NSE). Second, the choice of model also only allows a rather rudimentary partitioning and routing of water fluxes, which will have a considerable effect on the tracer composition.

p.23,l.868-873: it is difficult to judge for the reader if the CTRW framework is more physically justified than other models. What also remains unclear: is CTRW necessary to model long tails? Or can suitable distributions be used in other (conceptual) models to reproduce similar results?

Best wishes, Markus Hrachowitz

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