Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2017-36-RC4, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "HESS Opinions: The complementary merits of top-down and bottom-up modelling philosophies in hydrology" *by* Markus Hrachowitz and Martyn Clark

M. Bierkens (Referee)

bierkens@geo.uu.nl

Received and published: 14 February 2017

I started to read this opinion paper with great anticipation because I think there is a desperate need for joining top-down and bottom-up approaches to arrive at solid hydrological theories. The paper is generally well written and starts out with a promising small review about the nature of bottom-up and top-down approaches.

However, after reading the part thereafter, I have to admit I started to become a bit disappointed. The reason for this is that the second part of the paper becomes quite unbalanced and reads as an apologia for top-down modelling. What I miss is a section "Modelling myths or not" for bottom-up approaches. For example, statements as "Bottom up models are over-parameterized" can be elaborated on. After that I would

have liked to have a section to sketch a way forward to marry both approaches taking account of their complementarities. Shortening the "Modelling myths or not" to make room for similar sections on bottom-up approaches would make the paper much more balanced and interesting.

Also, I have some additional remarks about the paper.

First, the authors underpin the statement that "At the macroscale, which in the realm of organized complexity is frequently characterized by the emergence of relatively simple functional relationships... that integrate typically unobservable natural heterogeneity over the model domain", with a comparison with to statistical physics (e.g. gas laws). However, there is a big difference between an ideal gas and a hydrological system related to the assumption of ergodicity. In that context, this assumption loosely means that at all times all microstates are present when averaging over the volume. This assumption is valid for an ideal gas but not necessarily the case for hydrologic systems.

Second, I feel that a problem with the way top-down megascopic hydrological laws are derived (also in comparative hydrology) is that often only (signatures) of the output variables are used to assess the form of the Q = F(S) relationship. This can only be done if a certain form (often a power function) is assumed a priori. I think that to really assess the form of these relationships one needs to jointly measure the state (groundwater storage, soil moisture, snow water equivalent) and the output variables (discharge, evaporation). Very rarely these state observations are used or available in catchments used in comparative hydrology. So we should get away from the fixation with hydrographs only and start measuring states. To add to this: energy conservation is often added by checking if the found megascopic laws follow Budyko's hypothesis. This is only a weak check on energy conservation, because it only checks for very long times and doesn't guarantee energy conservation at any given time.

Third, once megascopic laws have been derived empirically, these laws' physical basis should be strengthened by also deriving them from upscaling from smaller-scale mechanics. A well-known example is Darcy's law. It was first established empirically - note that this was done by both observing states (heads or actually the head gradient) and fluxes. Later (much later), it was shown that it could be derived from the Navier-Stokes equations (by 1. neglecting quadratic inertia terms: laminar flow -> Stokes equations; 2. volume averaging by homogenization; 3. noting that drag forces are much larger than viscous forces). Obviously, heterogeneities in hillslopes and catchments are more complex than pore-scale heterogeneities in a REV. This makes simple homogenization not likely a suitable approach. However, hyper-resolution (cm-scale) modelling using simulated heterogeneities (including macropres etc) with 3D PDE-based models (e.g. Parflow, Hydrogeopshere, Cathy) and upscaling the results may be a way to derive megascopic laws from first principles.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-36, 2017.