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Interactive Comment

Interactive comment on "Searching for the Holy Grail of Scientific Hydrology: $Q_t = H(\overleftarrow{S}\overleftarrow{R})A$ as closure" *by* K. Beven

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When I started my research career by trying to model a hillslope and small catchment using a finite element model based on Darcian theory, I chose that direction because (as a young, naïve and optimistic graduate student) I wanted to be objective about my hydrological science. REW theory is also an attempt to do just that, to provide a foundation for theorising in hydrology that goes beyond the use of point scale theory (that resulted in my thesis study being a failure, see Beven, 2001). I am therefore optimistic about this approach, but as noted by the anonymous Referee #1, it is important to remain realistic about what might be possible.

In that sense, it is a pity that my paper was available in HESS-D well before the papers by Lee et al. (HESSD-2006-3-1627), and Zehe et al. (HESSD-2006-3-1667) for which it was intended as a commentary (having been written after I was sent early comments

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of those papers by the authors for comment). There is no doubt that some proponents of the REW approach have been over-optimistic about their closure schemes, ignoring some of the concerns and constraints over what is possible given current measurement technologies, and being over-optimistic about what might be possible using remote sensing. Sequential data assimilation (Referee #1) is not the answer, it is only a way of sequentially compensating for model structural error. Gravity anomaly measurements of storage (Savenije and Fenicia) are not the answer either, the scales are far too large to learn much about the way in which hydrological systems and processes actually function. As yet, other types of remote sensing have also not provided much in the way of useful hydrological information because of the significant uncertainties associated with their interpretation both before and after the digital numbers are received by the hydrologist (see for example, the analysis in Bashford et al., 2002). This may, of course, change in the future.

So my conclusion was that we need a different type of approach and I tried to analyse some of the requirements for the downslope flux closure. Referee #1 does not seem to have taken the point of my paper on board in this respect. The suggestion that the resistor network analogy used by Reggiani and Rientjes is a way to resolving how mass and momentum flux closure can be solved directly at the scale of interest by applying mass and piezometric head conservation along closed loops is (in their usage) exactly the type of approach I am arguing against. What is this “head” at the scale of a heterogeneous REW control volume in practice? How do the momentum losses get accounted for at the control volume scale in a complex heterogeneous nonlinear domain? Savenije and Fenicia suggest that my hysteretic equation is not the answer - but any useful form of closure must represent that hysteretic response. They point out themselves that this can be done with simple storage schemes - they should also have noted that I had already (at the end of section 3 of my paper) suggested that a simple transfer function will have a similar effect. It may well be that we overemphasise some of the complexities involved and that a simple representation of the dominant modes of response may be adequate within the limits of uncertainty with which we can characterise other boundary fluxes and system parameters. The issue is then to find and justify the right sort of dominant mode representation, while allowing for the fact that this may not be possible unambiguously and for the inevitable uncertainties in prediction.

Finally, Erwin Zehe questions the dimensions of the proposed relationship between the REW area, the past trajectories of storage and rainfalls, and the downslope flux. This also seems a little odd, given that H() is noted to be some (as yet undefined) hysteretic nonlinear relationship, the dimensions of which can be arranged to match the required dimensions of bulk downslope flux. The much more important question is how to develop techniques to take account of the past hydrological trajectories of the system. Is either the transfer function approach, or the Savenije and Fenicia approach adequate as an approximation in the face of inevitable uncertainties? Is a formal representation of hysteresis, as suggested by the papers by Flynn et al. and O'Kane cited, identifiable? Or is some different approach needed? These questions will provide material for debate and thought for many years to come.

References

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