

## ***Interactive comment on “Searching for the Holy Grail of Scientific Hydrology: $Q_t = H(SR)A$ as closure” by K. Beven***

**K. Beven**

Received and published: 25 June 2006

Response to Comments by Erwin Zehe

This would appear to be a perfect example of the HESS-D approach to open commentary and discussion on submitted papers being very valuable. The discussion in my paper and the comments by Erwin Zehe encapsulate an on-going discussion that will, in fact, run for the foreseeable future because it has to do as much with a matter of beliefs as with the science. I would very much hope that hydrologists and hydrological modellers who have not been invited to review the paper will contribute to the discussion.

The fundamental issue here arises immediately when Erwin suggests that the title is not appropriate. I totally agree that closure is normal in other disciplines and should

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

be the basis for hydrological modelling. However, it should also be pointed out, that we also have some rather tenuous sub-element parameterisations in common, though there is always resort to the argument that these will be improved in the future. But my argument is precisely that the difficulty of characterising the surface and subsurface flow domains will make any such improvements difficult (and perhaps impossible) in hydrology. Subsurface hydrology is not a self-organising dynamic system in the same way as the atmosphere or oceans (or only at local pore scales); they are dominated by the boundary conditions of the media of the flow domain, the characteristics of which are unknowable with current and foreseeable measurement techniques. The phrase “Holy Grail” was chosen carefully, and is appropriate, though as I point out this does not mean that the search is not worthwhile. I have argued both in this paper and previously (e.g Beven, 2002) that looking for closure schemes is PRECISELY the way that hydrological modelling should be developing in future.

Erwin’s second point is about averaging. He suggests that techniques analogous to those used in the treatment of turbulence, treating variation in velocity, for example, as a Reynolds’ decomposition, might be useful in the characterising variability in hydrology and that correlation scales provide an intrinsic way of separating scales of representation. Well, in some simple saturated media in large scale flow domains they might be. In general, at hillslope scales, this will be not the case (though there are those that argue that hillslope systems, including percolines and preferential flows, are self-organising over long periods of time to maintain stability - I remain dubious because some of the causes of preferential flow are quite independent of flow). It is not actually an average velocity that we are interested in here - it is the mass flux as an integral of all the point velocities orthogonal to an element boundary. While clearly a subsurface mass flux can always be represented as an average velocity, it will, in general, be dominated by a flux in a small part of the pore size distribution. The extremes will be important (and, as for any extremes, will be most difficult to estimate from the “statistics” of the velocity distribution). Although there is again a knowability problem here - how is it intended that the sample of velocities to determine such a distribution might be measured in the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

subsurface in the general case?). This is made worse by the fact that the quantities that Erwin wants to treat as scale average quantities (capillary pressure or head) will be nonlinearly related to the closure flux and that the FORM of the velocity distribution (as well as magnitudes) may change over time with system state.

In his third major point, Erwin seems to think I have isolated closure from the balance equations. This is not the case, and he seems to have misunderstood the purpose of the equation for the drainage flux. It is not intended that this should replace the REW approach. It is, rather, only intended to represent ONE of the closure terms of the balance equation - a boundary drainage flux in response to the complex internal states (and history) of the element. There would certainly be an additional closure relationship for the integral of actual evapotranspiration fluxes from the element that would need to reflect the space-time variability (and history) of states of soil and vegetation in the element.

So, what we have here is essentially is a difference in belief as to how far the variability in closure fluxes can be treated as a theoretical problem, with all the associated methodological tools of hypothetical simulations and finding solutions based on continuum mechanics in heterogeneous domains. Erwin is a strong believer in this approach and has the justification that it is, after all, a “scientific” approach. I am (currently) a strong sceptic and would like to learn more about the closure problem more empirically, for cases where the fluxes can actually be measured, precisely because of the difficulty of applying the theoretical approach to any PARTICULAR flow domain as an REW. This is not, I would hope, just a question of my own limitations in being able to envisage or develop an adequate theoretical approach. It is much more to do with the practical issues of characterising actual rather than hypothetical flow domains. It is worth noting in this respect, that ALL small experimental catchment (or at least small impermeable catchments in which the discharges are really a good representation of the outflows) are good examples of REWs in this respect but clearly have complicated variability and downslope flow history issues which is why, after decades of theorising and modelling,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

we do not already have an accepted and satisfactory theory of catchment discharge fluxes as a function of catchment state - essentially the closure problem for drainage flux as set out here - rather than a wide variety of competing models of different levels of complexity.

While I concentrated on drainage fluxes in the presentation, evapotranspiration fluxes are also an interesting case for study. Here again we mostly have point measurements (or rather approximate line integral estimates in the case of scintillometers) that in some way reflect a complex space time pattern of fluxes but do not give the integral flux over an REW directly.

Let me repeat the emphasis at the end of my paper. The scale dependent mass balance approach formalised by the REW concepts should be the future of distributed hydrological modelling. For this to be useful, we need closure schemes for real systems. To achieve this we will need both theoretical and empirical studies to understand the problem. But, as noted in my paper, this is still only the second most important problem in hydrology for the future. I would hope that learning more about the nature of the closure problem might then lead on to an agreement on the definition of measurement technologies we need in the future, in the same way as a satellite design process involves a large community. It may be that, as often with satellite design, the physicists/engineers will tell us that what we need is not yet possible but suggest a compromise that is possible but more approximate. We see this already, for example, in defining the input fluxes to a catchment scale REW using radar-rainfall estimation, with all its limitations. We need to approach the problem of closure for the different boundary fluxes in this way as a better way of structuring a community attack on the problem of representing hydrological processes but it is unlikely that the subsurface characterisation problem will be resolved for a long time, if ever. Searching for a solution is, indeed, a Holy Grail. But, let the discussion continue.

References

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Beven, K. J., 2002, Towards an alternative blueprint for a physically-based digitally simulated hydrologic response modelling system, *Hydrol. Process.*, 16(2), 189-206.

---

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 3, 769, 2006.

**HESD**

3, S339–S343, 2006

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper