Hydrol. Earth Syst. Sci. Discuss., 3, S333–S336, 2006
www.hydrol-earth-syst-sci-discuss.net/3/S333/2006/

Hydrology and Earth System Sciences Discussions

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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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Received and published: 23 June 2006

Short summary The presented paper is due to my impression a mixture between a comment and research paper and therefore difficult to evaluate. The author is correct with his view that “closure” means essentially that we find relations for describing boundary fluxes between different zones with an REW, or different REWs, and that the applicability of the REW approach to real world basins stands and falls with the assessment of appropriate closure relations. Later on the author present the experimental and model evidence that discharge saturation relations derived from observed data in three catchments exhibit considerable hysteresis, which is again valuable and interesting. The author argues that such discharge-saturation are something like the ultimative closure relation and suggests finally a way to assess these relations by testing multiple competing hypothesis.

Evaluation Parts of the argumentation presented within section 1 and especially the data analysis in section 3 is very valuable. Nevertheless, the paper is not acceptable in the present form and the author should address the following major and minor points within a revised manuscript.

Major points: - The title is not appropriate. Closure is important, but I strongly disagree with the tenor of the paper that closure is something mystical like a holy grail. Closure is something normal in other disciplines e.g. boundary layer meteorology (Mellor and Yamady, 1972) and even discussed on the textbook level (Arya, 2003). Closure in this context means essentially to parametrize turbulent fluxes, that come into the equation of motion after a Reynolds decomposition. E.g. first order closure assumes that those fluxes are proportional to the gradient of macro scale average quantities such as the gradient of the average temperature. Turbulent diffusion coefficients have to be determined experimentally.

- In section 2 the author argues that the REW approach will not result in a representation that is consistent with continuum mechanics at any scale. In this context it appears to me that the author did not think what averaging means! In a probabilistic sense it means that we calculate the mean i.e. we do an ensemble average. It is well know from basic statistics that we need a sample that contains statistically independent values for calculating a representative mean. In REW theory we substitute the ensemble average by a spatial average, and this is essentially based on the ergodic assumption. This assures that we include enough statistically independent values into the averaging process. We can only end up with REW scale average quantities such as capillary pressure, saturation or pressure head, that are representative for a soil or an aquifer, if the averaging volume is large enough to satistify the ergodic assumption. This means that the volume must be much larger than the correlation length of soil hydraulic properties and other relevant properties to the third power. Otherwise the averaging does not make sense, because the averaged values will depend on the averaging volume!

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In this sense the REW approach cannot be scale independent, but is a means to separate scales! There is a minimum averaging scale, that is determined by the correlation length of relevant fields. An REW smaller than that scale is not representative! Hence, the REW approach is essentially based on assumptions from continuum mechanics and the appropriate size of an REW depends on the landscape! Similar, the mass balance equation for e.g. the average water saturation in the unsaturated zone of an REW is an ODE based on continuum mechanics, because it describes an averaged quantity! However, I agree with the author that simply describing the average dynamics in the unsaturated zone at the REW scale might be not sufficient e.g. to address transport problems. In this case the subscale variation of residence times in the u-zone could be parametrised by combining the REW approach with a stochastical approach.

- Closure is nothing that stands alone as stated by the author! It is always related to a balance equation and (as explained above) to a concrete REW which represents something like a functional unit of a catchment. E.g. exchange fluxes between the unsaturated zone and the atmosphere (which has to be parametrised as closure) include of course ET. The equation introduced in section 3 is by far not universal, as claimed by the author, as it does not cover ET! Furthermore, as H is claimed to be a non linear, hysteretic function, the suggested equation is not consistent with respect to the dimensions. R is rainfall, (length/time), S saturation (dimensionless) H is a non linear function, A is Area (length2) so the right hand sight will never get the dimension (volume/time) which is the dimension of discharge.

Minor Points - Unfortunately the abstract does not sufficiently reflect the content of the paper - Unfortunately the author does not refer to relevant work that deals with scaling problems that are very close to the closure problem such as Vogel and Roth 2003, JoH or Duffy (WRR 1996) - The calculation of the relative storage in section 3 is, as far as I understand, done using the water balance and potential evaporation in case of actual evaporation. It would be interesting to get an estimate of the related uncertainty. - It would be helpful not to give just the line plots in the discharge - saturation relations but

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to add symbols for the data points

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