Hydrol. Earth Syst. Sci. Discuss., 8, C4357–C4359, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C4357/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Extended power-law scaling of air permeabilities measured on a block of tuff" *by* M. Siena et al.

Anonymous Referee #1

Received and published: 10 October 2011

General comments

This paper represents a substantial contribution to the understanding of the often-observed power-law scaling of porous media (soil, aquifers, . . .) properties and specifically hydrogeologic variables such as permeability and porosity. The authors used the methods of moments and of the so called extended power-law scaling to identify power law scaling of log air permeability data collected (using four measurement support scales) on the faces of a laboratory-scale cube of tuff and published by Tidwell and Wilson in 1999. The methodology used for analyzing the data are presented in a clear-cut manner and the discussion/conclusions are original bringing a new insights into the derivation and significance of these power-law scaling (and range of occurrence) observation. The explanation of the advantage of ESS for analyzing the power-law scaling is particularly interesting.

Specific comments

Although the paper contains sufficient information and original works to be published as it is there are some issues that could be discussed (or at least noted):

1) This paper focus on a structure function defined using measurement increment which supports the comparison with variogram analysis. Yet, such kind of function and moment approach (eq. 1) and the occurrence of power-law scaling have been largely applied (Bird at al., 2006, J. of Hydrology; Dathe et al., 2006; Geoderma) to the measurement itself (in this case the size of the support volume is used instead of the lag) and conclude that the nonlinear variation of the scaling exponent with q denotes multifractality. I believe it would be useful to comment on the use of measurement increment versus measurement (as well as the possible use of negative values for q).

2) What could explain that the log permeability distribution of this rock is similar to a tfBm (and why it is not the case along direction z).

3) Could we extend the conclusions by saying that : if the signal measured is not a tfBm distribution, then ESS should not perform better than the standard moment method ?

Technical corrections

- The use of “s” for denoting the lag is sometimes confusing (while the structure function is noted “S”) and makes typos consequential such as in equation 2 ($S \sim s\hat{}f(q)$).

- Why "N" depends on "s" (it is not always the case isn’t it) ?

- I believe that a couple of sentences (in the introduction) explaining for non-specialists what is “a signal derived from additive processes subordinated to a truncated version (tfBm) of additive, self-affine fractional Brownian motion (fBm)” would make the paper more attractive to most of the hydrologists !

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Interactive Comment

- Line 24, page 7814 “to H =0.74ri =1.27 cm” should be “to H =0.74 for ri =1.27 cm”

- (some of the) stationary variograms of Fig. 14 could be added in figure A1 for direct comparison.

- explain notation “!!” in A2 and A8

In conclusion, i support the publication of this paper because 1)it addresses relevant scientific questions with applications to a very large number of domains; 2) the concepts and mathematical tools are presented in details, making this paper very didactic and original; 3) substantial conclusions are reached and reopen the discussion on the significance of the observed power-law scaling in the frame of fractal and multi-fractal models.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 7805, 2011.