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Hydrology and Earth System Sciences Discussions

Interactive comment on "Geostatistical modeling of spatial variability of water retention curves" *by* H. Saito et al.

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This is a very interesting and well-executed paper that definitely deserves attention and comes to an interesting conclusion with some practical implications. However, its title is misleading and its conceptual basis is fundamentally flawed.

The gist of this paper is this: For the geostatistical (spatial) estimation of a water retention curve at a location (x1, y1) from several nearby locations, where soil samples have been taken and water retention functions have been measured, which of the following two approaches work better:

1. Method: fit retention curves to the datapoints, then run a geostatistical extrapolation

of the retention curve parameters to the point of interest, then regenerate a water retention curve from the estimated parameters for that point. (parametric method or P method)

2. Method: for each of several tension-values at which water content has been measured, run a geostatistical extrapolation of the water content to the point of interest, then fit a retention curve to these data. (non-parametric method or NP method)

By using an empirical approach (application to a well-characterized field site with a very dense dataset), the paper concludes, that especially for the VG model (and to a lesser degree for the BC and LN-Kosugi model), the second option works better than the first in a majority of cases. This is significant, argue the authors, as the first method is commonly employed in stochastic analysis.

Prior to publication, I suggest that the authors address several critical points currently omitted in the manuscript:

* the NP method chosen is arbitrary. For example, an obvious alternative NP method would be to pick 10 saturation values and run a geostatistical extrapolation of the soil water tension to the point of interest, then reconstruct the water retention curve. To which degree does the conclusion depend on the choice of the "NP method"? Also, why use kriging as an interpolation method? How would other interpolation methods fair with either the P or NP method? (see comment below).

* This is presumably a geostatistical paper. Yet, nowhere in the manuscript do the authors discuss the distribution of either the saturation values or the water retention curve parameters. Ordinary kriging is a best linear unbiased estimator, IF the underlying distribution of the estimated parameter is GAUSSIAN (NORMAL). What is the distribution of the water content at various tension values? What is the distribution of the water retention parameters? It is well documented that retention curve parameters such as saturated hydraulic conductivity, air-entry value, and shape parameter tend to have strongly skewed distributions (usually log-linear), while water content tends to

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be closer to normal distributed. Has this been accounted for in the application of the "P-method"? If the water retention curve parameters in this dataset are skewed, while water content is normally distributed, this may explain why the NP method (interpolation of water content data) works better than the P method (interpolation of retention curve parameters).

* Note that - geostatistically - the kriged value merely represents the mean of a normal distribution, which is further characterized by the kriging variance, representing a random variable at location (x,y). That said, there is a fundamental mathematical flaw in the NP-method. Let me explain this by going back to the P-method: in the P-method, we fit a water retention function to a set of data measured on a soil core, taken from location (x,y). The parameters of that function (e.g., alpha, n, theta-s, theta-r) are exact at(x,y) (assuming a good fit). In the context of a stochastic analysis, each parameter is a random space function with an underlying (second order stationary) distribution. At (x,y), we have a measurement of these parameters. The uncertainty about the value of the parameters at (x,y) is zero (we neglect measurement error and errors in assuming a certain model - VG, BC, etc - for the retention curve). In kriging these parameter values at another location (x1, y1) from known values in the vicinity of (x1,y1), we really estimate the DISTRIBUTION of the water retention parameters: the kriged value is the mean and the kriging variance is the variance of that (joint-normal) distribution. We don't know the exact water retention curve at (x1,y1), but we know something about the possible shapes it can take on by looking at the distribution of the parameters obtained from kriging. The important point is: in the geostatistical framework, there is nothing certain about the water retention curve at (x1, y1).

Now, in the NP method presented in this paper, the water content (or saturation) at specific soil water tension values is the random space function. The NP method estimates the distribution of that water content (at a specific tension value) for location (x1,y1) from the known water content values (at that specific tension value) at locations in the vicinity of (x1, y1). The result of the kriging is again an estimation of the normal distri-

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bution of that water content (at that specific tension value) at (x1,y1), characterized by the kriged value (mean) and the kriging variance.

The flaw in the development of the NP method is this: The authors assume that the kriged water content value is the actual value of the water content at that location, then fit a retention curve to these water content estimates (kriged separately at various soil water tensions). They neglect that the kriged water content value represents the mean of a distribution. And the resulting water retention function has no distribution associated with it (unlike in the P method). In fact, in this manuscript, a geostatistical method is reduced to a deterministic interpolation method.

The correct geostatistical approach would be to plot the distribution of water content against the soil water tension values, where the distribution is defined by the kriging mean and variance. And THEN fit a family of water retention curves to those distributions. From that family of retention curves we would obtain the distribution of the water retention curve parameters at (x1,y1). But how would one do that fitting procedure? That is exactly the question that would need to be answered to correctly address the initial question posed in the introduction of the manuscript.

What the authors present is a very practical interpolation method that works in practice but is inconsistent with geostatistical theory.

More importantly, the described NP method does not give the stochastic modeler her/his essential tool: a random space function distribution for the parameters in the unsaturated flow equation. The proposed approach here would only work for a deterministic simulation using the deterministic water retention function found by the NP method for (x1, y1). That is, the NP method is a practical approach, where one is merely interested in simulating a scenario represented by a single, somewhat likely function for the water retention curve at (x1,y1).

Why does it SEEM to work? Because water content is nearly normal distributed, and because the dataset spatial density is very high. Hence the estimation of the water content (at given soil water tension) at (x1, y1)is, on average, relatively good - kriging works well as an interpolation method. Then, the reconstructed water retention curve matches well with a measured curve (cross-validation or jack-kniving method).

At the very least, the paper needs to be retitled (and have a new introduction) that omits the word geostatistical and the word spatial-variability. I suggest: Alternative Deterministic Method for the Spatial Interpolation of Water Retention Curves.

By the way, kriging is only one interpolation method applicable for this problem - P or NP method. Others include linear interpolation, minimum curvature interpolation, inverse distance to a power, etc. Perhaps these should be tried as well.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2491, 2008.