

Interactive comment on “Sensitivity analysis of point and parametric pedotransfer functions for estimating of soil water retention” by Sami Touil et al.

S. Barontini (Referee)

stefano.barontini@unibs.it

Received and published: 4 July 2016

1 General comments

The article proposed by Touil and coauthors addresses an important issue, when pedotransfer functions (PTFs) are used to characterize the soil water retention curve (SWRC), that is the sensitivity of the predicted soil water contents to the predictor variables. The goal is to provide an insight on which are the most reliable predictors, among the most common ones (bulk density, grain size distribution, organic matter content).

C1

In order to do so, the Authors investigated a sample of more than two hundred soils with poor organic matter content, splitted into two subsamples, one for interpolation (189 soil samples) and one for validation (53 soil samples), and compared the obtained results with those predicted by means of Rosetta database.

PTFs were approached both point by point and in parametric form. Multiple linear regression (MLR) was used to find a relationship between soil water contents and the explanatory variables, and the multiple nonlinear regression (MNLR) was used to find a relationship between the parameters of the van Genuchten (1980) SWRC and the explanatory variables.

Then the global sensitivity analyses (GSA) method was used to perform the sensitivity analysis of the predicted variables against the explanatory ones.

The database is rich, the methodology and the results can be interesting, so that I recommend this article for publication in Soil provided that the Authors fix some issues of major importance and some structural aspects of the presentation.

My major concerns are about the parametric approach to PTFs.

In lines 106 to 113 it is said that the RETC code was used to fit the van Genuchten SWRC to the experimental data, but at line 102 it is reported that the experimental data were determined at two tensiometer–pressure potentials, that are -33 and -1500 kPa. Therefore it seems that the four parameters of the SWRCs (θ_r , θ_s , α , n , while m is constrained to $m = 1 - 1/n$) are fitted by means of two experimental points only for, each curve.

If this is the case, and no other constraints were introduced, the set of parameters is not univocally identified for each soil, and the further analyses on the parametric approach loose their significance.

I therefore recommend that either (1) the Authors better detail the followed procedure for this approach, so that it is clear how main experimental points the procedure is

C2

based on or whether there were other constraints to univocally identify the fitted parameter set; or (2) they remove the part about the parametric approach and better develop that about the point approach.

Moreover (3) I encourage the Authors to explicitly present the PTFs they obtained for the investigated sample of soils.

In the following lines, detailed comments and some technical notes will be provided.

2 Detailed comments and technical notes

II.37—38 Explain whether it refers to the hydrological state of the soil or to the characterization of the hydrological properties;

I.47 $\Theta \rightarrow \theta$ (in all the paper);

I.51 Uniform all the paper to the version "van Genuchten" (or to "Van Genuchten");

I.59 "different environments from which they were derived for"

I.63 "and hydraulic conductivity as well";

II.93—96 Check the percentages, or probably better explain the consistency of the whole database;

I.103 "moisture" $\rightarrow$ "water content". Field capacity or soil saturation? Samples in Richards apparatus are usually saturated. Moreover field capacity (regarded to as the soil water content which remains in the soil after abundant imbibition and when percolation is materially decreased) can be quite a small water content, even smaller than the water content at 33 kPa;

I.106 "defended" $\rightarrow$ "defined";

C3

I.119 Add something like "the following measures of the errors", or something else, to make the article more readable;

I.131 Check equation (4), I think that there should not be $\frac{1}{n}$;

I.145 Title not necessary;

I.161 Explicit what does the constraint X_{i*} stand for;

I.177 Table 2 is not cited before Table 3. This is a good point to explicitly provide the formulae of the obtained PTFs;

II.199—203 I agree with this sentence, but in this case it can also be due to the undetermination of the interpolated parameters (see the General Comments);

I.233 Avoid referring to the conductivity as the framework of the article seems to be based on Mualem's predictive approach to the relative conductivity function (as it follows from the constraint on m);

I.244 and followings Consider the idea of collecting all the analyses regarding the texture in one paragraph only, thus restructuring the paragraphs regarding sand, sil and clay. This can strongly help the readability of the discussion. Many analyses of previous Authors are reported: I suggest to explicitly detail whether your results are according or discording to previous ones;

I.291 "They increase in organic matter" with... ?

I.306 and followings Typically clay is very important at characterising the water retention, even if it can loose sensitivity for great values of clay content: in which sense does it sound the statement of line 317?

I.353 I agree with the conclusion but it seems to be quite in contrast to what observed after the reported analyses and the last conclusion: I suggest to better detail this point or remove it.

C4

Further minor comments: (i) correct some typos, (ii) check the consistency of the references list and alphabetically order it, (iii) change the colour of histograms and bar-graphs to ensure the readability also in B&W printing.

Interactive comment on SOIL Discuss., doi:10.5194/soil-2016-18, 2016.