Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-668-AC2, 2018

Hydrology and Earth System Sciences Discussions
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Interactive comment on "Predicting the soil water characteristic curve from the particle size distribution based on a pore space geometry containing slit-shaped spaces" *by* Chen-chao Chang and Dong-hui Cheng

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Received and published: 3 May 2018

Dear Reviewer,

We appreciate very much for your positive and constructive comments and suggestions on our manuscript entitled "Predicting the soil water characteristic curve from the particle size distribution based on a pore space geometry containing slit-shaped spaces". Now we made a detailed response for your comments as following:

Comment 1: Detailed comments Equation (4) should be rewritten in a more general

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Discussion paper

way, regardless of the units adopted for the water potential. In this regard, it seems that this equation is used to link pore dimension to water potential, even in the silt-shaped space between pores. This aspect should be better clarified, as the dimension of the silts are proportional to the pore diameter, so it is not clear what is the diameter introduced in equation (4) to obtain the corresponding potential.

Reply to comment 1: Equation (4) in our main manuscript was gained by substituting known parameters into Laplace's equation (Eq. (1))(Haverkamp and Parlange 1986), in which σ=7.275$\times$10-2 kg s-2, w=998.9 kg m-3, g=9.81 m s-2, and ε=0°(Mohammadi and Vanclooster 2011).

Then, transforming ri to di and the units to gain Eq. (2) (Eq. (4) in our main manuscript), which is more clear to express the relation between the pore diameter and suction head.

Therefore, we can add the process of transforming Laplace's equation Eq. (1) to Eq. (2) in revised manuscript.

Considering the shape and size of slit spaces were different from the central pore, their suction were calculated using different equations respectively, the suction heads of central pore were calculated using Eq. (2) , while the chemical potentials of slit spaces were calculated using Eq. (3) suggested by (Derjaguin and Churaev 1992), then transforming the units to gain the suction heads.

This aspect have been described in Section 3.1 "Estimating the pore volume fraction" in the main manuscript.

Comment 2: There is also another point, regarding silt-shaped spaces, that in my opinion deserves to be discussed in the paper. To my best understanding, silt-shaped spaces are introduced to consider the water which is bonded to the particles in such a way that the model of the bundle of cylinders fails in describing it. In fact, with such silts dimensions as small as 1 Å are reached. In such a range of dimensions, capillarity is not anymore the mechanism which bonds water to the soil particles, and other kinds of interactions contribute to the potential energy of water (actually, already for quite larger pore dimensions). So, if equation (4) is still used, this turns out to be an effective, but not physically based, way to obtain water potential.

Reply to comment 2: Recent research have identified the lack of consideration of adsorptive surface forces and liquid films in present theories for flow and transport in unsaturated porous media, which would lead error in corresponding calculation, particularly at low saturation. Nitao and Bear (1996) pointed out that the part of the problem lies in the vague definition of the soil matric potential where capillary and adsorptive forces are lumped together. (Tuller et al. 1999) considered the individual contributions of adsorptive and capillary forces to the matric potential, the liquid-vapor radius of curvature (capillary contribution) and the film thickness were calculation using the same given potential. This simplified method is termed the shifted Young-Laplace (SYL) equation(Tuller and Or 2001). In essence, they made a simplification that the chemical potential, the capillary pressure and the adsorptive pressure were equal.

In our study, a simplification was made that we only take the water in central pore and slit spaces into account, without considering the liquid films coat pore and slit walls, therefore the capillary pressure, as the dominant acting forces, was only considered. Besides, the predicted suction head in our study is lower than 5000 cmH2O, therefore the error resulted from the lack of consideration of adsorptive surface forces were relative small. We will add the discussions corresponding to the silt-shaped spaces in the revised manuscript.

Comment 3: Pag. 5, line 13. The water potential values should be negative.

Reply to comment 3: Indeed, it's true that the critical potential values of the biggest slit spaces should be negative on Page 5, line 13. In order to compare in unified standard, this potential values were transformed into the suction head with unit of cmH2O. It was our oversights that it not be described clearly; hence we will change "critical potential" as “critical suction head” on Page 5, line 13 in revised manuscript.

Comment 4: Pag. 9, lines 17-19. This statement sounds surprising, if I understand it correctly. The smaller the particles, the larger I expect soil (specific) surface area, as for instance for clay particles. In this respect, the authors should try (where possible), or at least mention the possibility of using measured surface areas rather than estimating it by means of an empirical formula, and discuss how their results could be (positively or negatively) affected.

Reply to comment 4: The surface area (m2) on Page 9, lines 17-19 refer to the total surface area of particle which is positively related to the equivalent particle radius and is different from the specific surface area (m2 g-1).

The direct measurements of the specific surface area were time- and money- consuming and the measuring error would also exist. Furthermore, the measured surface areas for so many samples were difficult to gain for us at present. Therefore calculating the specific surface area using an empirical formula may be the best choice at present. The empirical method used to estimate the specific surface area in Section 3.2 was presented by (Sepaskhah et al. 2010) with an r2 value of 0.88, it proved that this empirical equation have reliable capabilities to use. Although the errors generated inevitably when calculating the specific surface area from the empirical equation, it would enhance analysis uniformity and avoid some error resulted from abnormal measurement.

Comment 5: Pag. 10, line 21. The reference should read “van Genuchten, M. T.” instead of “Genuchten, M. T. V.”, and the same holds for where such a reference is recalled in the text.

Reply to comment 5: Thank reviewer for pointing out my mistake. “Genuchten, M. T. V.” on Page 10, line 21 will change into “van Genuchten, M. T.”, and other places where such a reference is used.

References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-668, 2017.

$$\psi_i = \frac{2\sigma \cos \varepsilon}{r_i g \rho_w} \qquad (1)$$

$$\psi_i = \frac{3000}{d_i} \qquad (2)$$

$$\mu = -2\sigma / (\rho \alpha d) \qquad (3)$$

Fig. 1.