Reply to “Comments on ‘Scaling Water Characteristic and Hydraulic Conductivity Based on Gregson–Hector–McGowan Approach’”

We certainly appreciate the comments of Dr. A.P. Meissner for bringing into focus and clarifying the key features of the scaling approach we presented in Ahuja and Williams (1991). We do disagree with Dr. Meissner’s conclusion drawn in the last paragraph of the comments, and we also wish to clarify some other points. First, the clarifications. Dr. Meissner states: “However, in the field or in ‘real soils’, the values of $\psi_s$ and $\psi_i$ are measured with error”. We do not believe it is primarily the measurement of $\psi_s$ and $\psi_i$ with errors that causes the values of these parameters to differ from location to location within a soil type or a group of soils, but rather it is a result of natural variability in real soils. The real soils do not follow the perfect linear relationship between $a$ and $b$ of Eq. [2] of Meissner.

In Table 1, Dr. Meissner gives effective average values for $\psi_s$ and $\psi_i$ calculated from our $p$ and $q$ values for the different soils. For Lakeland and Norfolk soils, the effective $\psi_s$ values should be 0.276 and 0.374 m$^3$ m$^{-3}$, respectively, since these soils had a residual water content, $\psi_r$, of 0.02 (Ahuja and Williams, 1991, Table 1). We may also point out that the rather low value of effective $\psi_s$ of Lakeland soil was probably a result of poorer fitting of experimental $\ln(-\psi) \text{ vs. } \ln(0.02)$ data by a straight line. The same is probably also true of the 0- to 45-cm depth interval of Bernow soil. Both of these soils are sands, whose $\psi(0)$ curves are known to deviate appreciably from a log–log linear relationship.

In the last paragraph, Dr. Meissner states, “A ramification of the analysis is that $b_i$ is only a scaling factor if you do not take the Campbell model into account. As it can be seen if you substitute the values of $\ln(-\psi_i)$ and $\ln(\theta_i)$ for $p$ and $q$ in Eq. [4] of Ahuja and Williams, you arrive at the equality of $\ln(\theta) = \ln(\theta_i)$. The problem is that Eq. [4] of Ahuja and Williams is written for a fixed location for real soils, and $p$ and $q$ values for a soil type are not equal to $-\ln(-\psi_i)$ and $-\ln(\theta_i)$ at that location in real soils. [The $p$ and $q$ are equal to effective average values of $\ln(\psi_i)$ and $-\ln(\theta_i)$ across locations.] Hence, the substitution called for in the second sentence of the above statement of Dr. Meissner cannot be made. Only for an imaginary set of perfect soils will the $\ln(\psi_i)$ and $-\ln(\theta_i)$ be the same at all locations, equal to $p$ and $q$, respectively. In that case, the above-noted substitution into Eq. [4] of Ahuja and Williams (1991) will indeed lead to the identity $\ln(\theta) = \ln(\psi_i)$, a 1:1 relationship between $\ln(\theta)$ and scaled $\ln(-\psi)$. But this identity simply indicates perfect scaling, the way it should be.

We also take this opportunity to suggest a more appropriate name for the type of scaling presented in Ahuja and Williams (1991). The scaling is based on the property of all $\ln(-\psi)$ vs. $\ln(\theta)$ straight-line functions within a soil type or group, below the air-entry values, to converge within a narrow band around a point $(p, -q)$, $p$ and $-q$ being the effective average values of $\ln(-\psi)$ and $\ln(\theta)$, respectively. We, therefore, suggest that this scaling technique be called convergence scaling.

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References

Communication in Soil Science

The growth of the Soil Science Society of America over recent decades is proving to be a mixed blessing. On the positive side, we now have a much improved sense of professional identity. One of the counterbalancing costs, however, is communication. The annual meeting has always been a primary time for the interchange of ideas. As the meetings become larger, opportunities for formal interchange through symposia are expanding, but opportunities for interdivisional and even intradivisional dialogue are being increasingly restricted. It has now been 15 yr since the name of the Soil Science Society of America Proceedings was changed to the Soil Science Society of America Journal, an overdue action because the function of the journal had already changed. Going back another 20 yr to the mid-1950s, following most papers published in the Proceedings we find the transcript of discussions that followed oral presentation of the paper. These discussion transcripts were dropped because they were not always kind nor appropriate for print, but one cannot deny that their inclusion was an effective communication device. During the 1960s and 1970s, the increase in numbers of scientists presenting papers caused continual erosion of time available for discussion, and intradivisional communication began to suffer. The only programmatic solution short of establishing a policy of title–summary review and possible rejection was to encourage poster presentations; this encouraged discussion, but one-on-one rather than group discussion. Thus, our traditional meeting communication device has swung from encouraging group discussions to promoting individual discussions.

Changes in our meeting structure were, of course, necessary in view of programmatic time restrictions. We are now faced, however, with the challenge of redesigning our communication structure if we are to continue as a unified Society and not gradually break into component subdisciplines. We need a forum for placing new ideas (even weird and off-beat ideas) before the membership as a whole for evaluation and reaction. We need a forum for expressing opinions on decisions and events affecting the Society. We also need a practical forum for rapid publication of transitory observations, such as fires in Yellowstone or a hurricane in Puerto Rico, where attraction of additional research effort can be of great value in understanding the event.