COMMENTS AND LETTERS
TO THE EDITOR

Estimating Soil Water Diffusivity—Scaling Technique

We read with interest the note published by Miller and E. Bresler (1977) as it is a discussion of one of our early works (Reichardt et al., 1972) which we continued to develop. While agreeing completely with the material presented in the note by Miller and Bresler we want to add that in a later paper (Reichardt and Libardi, 1974) we present the same method using as support the same original data, including information of five tropical soils. We also reconsidered the case of scaling hydraulic conductivity which in Reichardt et al. (1972) was not considered successful, and published the same methodology for estimating soil hydraulic conductivity from infiltration tests into air dry soil (Reichardt et al., 1975).

The power of this empirical scaling technique has been recently recognized by several authors (e.g., Parlange, 1973; Peck et al., 1976; and Warrick et al., 1977) and might be the solution for the problem of spatial variability of soil parameters, which is one of the main issues in soil science today.

Received 14 March 1978.

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Literature Cited


Estimating Soil Water Diffusivity—A Reply

I regret my failure to observe and report that an equation equivalent to that given by E. Bresler and myself (1977) had been previously published by Reichardt and Libardi (1974). We proposed a dimensionless constant $\alpha = 1 \times 10^{-3}$ whereas in their version the corresponding constant was $1.462 \times 10^{-3} \text{ min sec}^{-1}$ owing to their decision to express $m^2$ in cm$^2$ min$^{-1}$ on the one hand and $D(\theta)$ in cm$^2$ sec$^{-1}$ on the other. In our version, $D(\theta)$ and $m^2$ always have the same units, whatever choice may be made.

It is hoped that in "...agreement with the material presented in [our] note..." Reichardt and Libardi mean to say that they now accept our objections to the "...proposed technique to determine the microscopic characteristic length [$\lambda^{*}$] of a soil." This objection was first raised in 1971 in discussion following oral presentation of their original work and again, more explicitly, prior to publication of the resultant manuscript (Reichardt et al., 1972). Specifically, it was pointed out that it was not their technique of assigning values to $\lambda^{*}$ that brought concordance to their experimental results for dissimilar soils but rather data for wetting front advance, expressed by the parameter $m^2 = L^2/\lambda^{*}$. To assign values of $\lambda^{*}$ as they proposed is to invite unjustified expectations that functions other than $D(\theta)$ might be "scaled" using this same $\lambda^{*}$. This expectation is negated by such basic functions as data representing soil water characteristic curves for soils they used.

Recent scaling efforts of Warrick et al. (1977), referred to by Reichardt and Libardi are not subject to the same criticism. Warrick et al. scale within sets of closely related samples which are presumably more or less "similar," each set having been drawn from a single soil type. Instead of using $\lambda^{*}$, however, they could have just as well defined "hydraulic radii" as determined from permeability measurements and "pore sizes" as determined from drying data before undertaking the normalizing procedures that they use to assess variability within each set. Much the same could be said for the efforts of Peck et al. (1977).

Confusion attending efforts to adapt a theory devised for similar media to manifestly dissimilar media has, for too long, obscured the extraordinary implications of the original results of Reichardt et al. (1972) as extended by Reichardt and Libardi (1974) to embrace a total of 13 dissimilar soils. If the dimensionless constants referred to as $\alpha$ and $\beta$ in our note are truly as "universal" as their experimental results suggest, this as yet empirical fact should rank alongside Darcy's law as one of the fundamental useful discoveries about flow in porous media. If we do not encounter too many exceptions, their results demand an effort to explain this universality. I have recently seen a manuscript which takes a long step in this direction [W. Brutsaert, Dept. of Civil and Environmental Engineering, Cornell University]. Brutsaert demonstrates that $\alpha$ and $\beta$ are not independent constants and that if $\beta$ has the suggested value of 8.0 then $\alpha$ can only have the value $1.07 \times 10^{-3}$, which is in superb agreement with the experimental value inherent in the data of Reichardt et al.

Now the question is, Is $\beta$ always close to 8.0? If so, Why? Received 3 July 1978.

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Soil Compaction and Freezing and Thawing

Amelioration of machinery-induced soil compaction below the tillage depth by annual freezing and thawing has been the subject of discussion recently. Blake et al. (1976) probably were the first to question the validity of the widely held belief that machinery-induced compaction in agricultural soils is ameliorated in climates where ground frost penetration is more than 25 cm deep (Gill 1971). Heinonen (1977) asserted that amelioration of compaction with freezing and thawing will occur only if the soil bulk density will exceed a certain limit, which he termed "normal" bulk density. Recent publication (Voorhees et al., 1978), however, supports the Blake et al. contention that colder climates do not necessarily alleviate subsoil compaction. The reason given by Voorhees et al. that the ever increasing size of agricultural