Replies to “A Critique of Some Recent Attempts to Characterize Spatial Variability”

Following are our brief comments to the critique by Horowitz and Hillel in which they questioned some concepts and methodological premises of our analysis of field heterogeneity (Russo and Bresler, 1981).

1) Stationarity—We did not intend to prove the existence of stationarity in our field. The sentence quoted by Horowitz and Hillel is not a conclusion based on our fitting of autocorrelation to a specific mathematical expression. In this sentence (bottom of p. 684 of Russo and Bresler (1981)) we merely wanted the reader to note that the shape of the data points of the autocorrelation function (and not the mathematical expression to which the data points were fitted) may imply that the hydraulic parameters “can be viewed” as a stationary process. By “the existence of an integral scale” we just meant to say that the autocorrelation functions scattered around a zero value at a finite lag the value of which is smaller than the equivalent length of the field. Again, no attempts have been made to test stationarity because, obviously, we did not have more than just one realization.

2) Tests on Probability Density Function (PDF)—We agree that the conclusions regarding the goodness of fit test to the PDF are not well founded, because the tests are based on a small number of samples and a few degrees of freedom. Also, since the variances are relatively small normal and lognormal PDF are practically indistinguishable. The assumption that the PDF is independent of spatial position is equivalent to the stationarity assumption. The justification for the probability plot suggested by Horowitz and Hillel in their Letter to the Editor is also based on the same stationarity assumption and ergodic hypothesis.

3) Integral Scales of $K(h)$ and $\theta(h)$—In principle we agree with Horowitz and Hillel. Without extending the theorem to the general situation our results showed that the integral scales of $K(h)$ and $\theta(h)$ were smaller than the integral scales of $\theta$, $h_w$, and $K_r$.

4) Scale of Measurements vs. Field Scale—The length equivalent scales of the measurements of the hydraulic parameters in the laboratory and in the field were about 5 and 30 cm, respectively (Russo and Bresler, 1980). These values should be compared with the length equivalent of the field scale for the same hydraulic parameters, as may be inferred from the values of the integral scales which are in the order of 10 to 40 m (Russo and Bresler, 1981). These scales have been specified and reported in the above mentioned publications. The relationships between the mathematical expressions for $K(\theta)$ and $h(\theta)$, and field and laboratory measurements are also discussed and reported in our earlier paper (Russo and Bresler, 1970).

5) Conclusion—We certainly agree with Horowitz and Hillel’s conclusion that problems connected with field heterogeneity will be solved better and faster if soil scientists and statisticians will work more closely.

Received 18 Nov. 1982

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Three issues were raised in the critique by Horowitz and Hillel of our infiltration study (Soil Sci. Soc. Am. J. 45:687–691). The first concerns their observation of a slight upward trend in the semivariogram shown for the “before” data (Fig. 2, p. 689) suggesting a range of spatial dependence of 12 m. It is important to appreciate that the semivariance at the closest spacing of 2 m (10.7) exceeds the sample variance for the 48 before-treatment measurements as a whole (9.3). This negates the possibility of spatial structure at the site on the scale of our measurements. Similar results apply to the semivariograms obtained for the after-chemical-treatment data (Fig. 2). These semivariograms were presented since there were no significant effects of the chemicals on infiltration rate. The statistical methods used to establish this result are the second point of the Horowitz and Hillel critique. We conducted the statistical analysis on both the arithmetic and log-transformed data and found no significant effects of chemical treatment on infiltration rate in either case. We reported that the difference in the before- and after-infiltration rates showed no significant effects of chemical treatment. This result is relevant for arithmetic data since differences between two populations tend to be normally distributed regardless of the underlying distribution of the contributing populations.

The tests for normality of the frequency distribution of the log-transformed data were conducted for the two after-chemical groups using all 48 observations in each case. It is agreed that the grouping of data could be invalid if there were significant effects of chemical treatment on infiltration rate but such was not the case. Horowitz and Hillel suspected that insufficient chemical application may have been the reason for the lack of an observed chemical effect. They point out that our calculation of chemical dose was incorrect as stated (the third issue) and we appreciate their recognition of the error. In our original calculation of chemical dose, we had assumed a penetration depth of 0.5 m rather than the 1 m that we reported. Thus, the appropriate soil mass of 97.3 kg was correct as stated in our article.

We also assumed that the soil could react with a maximum of 0.4 meq of chemical per gram; however, we chose (as we state) to apply one-half (0.2 meq/g) of this amount and thus 19.5 equivalents of chemical were required per treatment. Our assumption of a chemical requirement of 0.2 meq/g was appropriate since the average cation exchange capacity for the site was 0.19 meq/g (Table 1, p. 688). The actual depth of chemical penetration in the study was not determined thus the discussion of any assumed penetration depth remains arbitrary. Nevertheless, the possibility that insufficient chemical application was the reason for our failing to observe any effect on infiltration rate is remote. The applied sodium or potassium salts would certainly have saturated the exchange sites of the top layers of these in-situ soil volumes. If there were any possibility for dispersion, swelling, or sodium saturation effects on infiltration rates, these would have been manifested in the top layers. Typical sodium-induced drainage problems become apparent when the sodium adsorption ratio of the saturation extract reaches about 15%, a condition that was likely far exceeded for the top 10 to 20 cm of soil.

Finally, we take this opportunity to report a correction needed in our semivariance equation (p. 689). The denominator should be $n$ rather than $n-1$. This follows since semivariance at a given lag distance can be calculated as one-half the expected (i.e., mean of $n$ values) squared difference between values (Burgess and Webster, 1980). The semivariance data (Fig. 2) were calculated correctly with the computer program obtained from University of California, Davis. We hope these comments clarify the issues raised by Horowitz and Hillel. We affirm that the conclusions of our research are valid as reported.

Received 15 Oct. 1982

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