DISCUSSION / DISCUSSION

Discussion of "Bearing capacity prediction of spatially random $c - \phi$ soils"[1]

Radu Popescu

The writer would like to commend the authors for a particularly interesting analytical and numerical study of the effect of spatial variability of soil properties on the bearing capacity. It is generally acknowledged that the problem of accounting for the stochastic spatial variation of soil properties is a particularly complex one, both from the data-collection point of view and from the methodological point of view. Generally accepted techniques have not yet been established to accomplish these tasks. To initiate a constructive discussion on this subject, some comments are provided here on the selected range of coefficients of variation (COVs) of soil properties and on the methodology suggested by the authors to simulate the stochastic soil properties.

Range of coefficients of variation

Confidence in design soil properties, and therefore in predicted structural response, is affected by a series of uncertainties arising from several sources, such as intrinsic spatial variability (i.e., actual variation of soil properties from one location to another), measurement errors, and insufficient field information. Those sources of uncertainty are often considered together as one source of variability in the response. It was observed by the authors, and by other researchers, that the intrinsic spatial variability of soil properties affects not only the variability of the predicted structural response, but also the mechanical behaviour itself. As explained, for example, by Focht and Focht (2001), because of the presence of weaker zones in a soil deposit, "the actual failure surface can deviate from its theoretical position to pass through weaker material so that the average mobilized strength is less than the apparent average strength." It appears that different types of uncertainties (insufficient information, measurement errors, etc., on the one hand, and actual spatial variability, on the other hand) affect the structural response in different manners, and they may have to be analyzed separately. It is the understanding of the writer that this was in fact the intention of the authors, namely to analyze the effects of intrinsic spatial variability alone.

A number of researchers have provided information on ranges of COVs for spatial variability of different soil properties, as obtained from in situ soil investigations. In most cases, those ranges also include variability due to measurement errors. There is, however, a very comprehensive study in this respect (Phoon and Kulhawy 1999), mentioned also by the authors, in which approximate guidelines are provided for the COV of intrinsic spatial variability, separated from other sources of uncertainty. It is the writer's opinion that, until more results become available, the guidelines provided by Phoon and Kulhawy (1999) are a good indication of the order of magnitude for the COV of soil variability.

COVs as high as 5 (i.e., one order of magnitude larger than those usually reported in the literature) are assumed in the paper for the soil parameters. In justifying this range, the authors present a very interesting set of considerations related to soil variability at various scales, starting from the soil particle scale (of the order of micrometres) and going to the regional scale (of the order of kilometres). It is demonstrated that, within such limits, the range of COVs assumed in the paper is reasonable. The writer believes, however, that with respect to intrinsic soil spatial variability, the scales applicable here are bounded by the volume of soil involved in the bearing capacity failure and by the finite element mesh size.

With respect to the upper bound, using soil data obtained at the regional scale that may lead to very large COVs has to do with insufficient information and is not related to the spatial variability of soil strength from one point to another within the soil volume of interest for bearing capacity calculations. With respect to the lower bound, the statement that bearing capacity failure may operate at the microscale is likely to be true; however, the results presented in the paper are obtained from finite element analyses that cannot perceive scales smaller than the size of the finite elements. In view of the aforementioned observations, the interpretation of the results provided in the paper for large COVs should be done with great caution. Such results certainly provide for an interesting sensitivity analysis, but it is not clear whether they can be used for design applications. As the Monte Carlo simulation results presented in the paper are obtained using finite elements with sizes of the order of tens of centimetres and the overall dimensions of the analysis are of the order of metres or tens of metres, the results obtained

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R. Popescu. Faculty of Engineering and Applied Science, Memorial University, St. John's, NF A1B 3X5, Canada (e-mail: radu@engr.mun.ca).

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for very large COVs may lead to overly conservative designs.

Method for generating sample functions of non-Gaussian fields

Two soil properties (c and ϕ) are modeled as a bivariate, non-Gaussian, homogeneous stochastic field in two dimensions. The methodology proposed in the paper to simulate sample functions of such a field for the subsequent Monte Carlo simulations is the following: a Gaussian stochastic field is simulated first according to the prescribed spectral–correlation characteristics of the target non-Gaussian field, and then it is mapped to the desired non-Gaussian field according to the prescribed marginal probability distribution functions (pdfs). The issue of cross-correlation between the two components of the vector field (c and ϕ) is addressed by considering the special limiting cases of $\rho = +1$, 0, and -1 that can be studied as scalar fields.

The authors mention a limitation of their simulation methodology, namely, that the nonlinear mapping from the Gaussian to the non-Gaussian field destroys the spectral–correlation characteristics of the Gaussian field. Consequently, the resulting non-Gaussian field has a spectral density function (autocorrelation function) that is different from the prescribed function. The authors mention that this should not be a problem because the correlation lengths of the transformed (non-Gaussian) and untransformed (Gaussian) fields "will be similar." They provide an example where this difference is less than 15% (when $s = 1.0$).

For the particular case of normal to log-normal mapping (eq. [5]), there are analytical solutions for calculating the resulting correlation function of the non-Gaussian field (e.g., Grigoriu 1995; Rackwitz 2000). Using such solutions for the correlation structure assumed in the paper for c, it can be shown that the ratio $\theta_{\ln c}/\theta_c$ of the correlation distance of the Gaussian field with respect to that of the log-normal field increases significantly as a function of the COV of c. This ratio is as large as 2.5 (indicating a significant difference of 150%) when the COV becomes as large as 5. When the COV is less than unity, this ratio is generally less than 1.2 (difference of less than 20%). Given the uncertainties in estimating correlation distances in the field, this latter value may be acceptable for univariate fields, but, as explained in the next paragraph, the errors are still too large when considering two different soil properties simulated by a bivariate random field. Regarding the friction angle, it is likely that the errors induced by the mapping (eq. [8]) are much smaller than those induced by eq. [5], as the pdf assumed for ϕ is symmetrical and closer to the normal distribution.

Another issue that would benefit from some discussion is the effect of the cross-correlation between the two fields modeling c and ϕ. First, it is not perfectly clear whether in the case of $\rho = +1$ the value of unity for ρ is preserved after the nonlinear mapping of the two components of the bivariate field. Second, because of the nonlinearity of the transformations (eqs. [5] and [8]), the resulting correlation distances for the two soil shear strength parameters, c and ϕ, may be different, as discussed previously. For assumed perfectly correlated c and ϕ, working with different correlation distances fades out the spatial variability of the overall shear strength, even for relatively small differences between the actual correlation distances of c and ϕ. This might explain some of the unusual behaviour in the results presented in the paper, namely the conclusion that the cross-correlation between c and ϕ would have a relatively small effect on the resulting mean bearing capacity and an insignificant effect on its variability. Although it is recognized that estimating the coherence between c and ϕ from in situ data is not a trivial task, the aforementioned conclusion seems to be somehow counterintuitive; also, it is not in agreement with results presented by previous studies (e.g., Cherubini 2000).

To conclude, the writer would like to point out that currently there are several techniques available to simulate non-Gaussian vector fields preserving the prescribed autocorrelation and cross-correlation structures, and the marginal pdfs. These techniques are based on the translation process theory (Grigoriu 1995). Such algorithms have been developed for various applications and have been also used to account for the spatial variability of soil properties. They are more time consuming than the methodology proposed in this paper, but compared with the time necessary to solve the finite element part of the problem, the computational effort to simulate the soil properties is still insignificant.

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DISCUSSION / DISCUSSION

Reply to the discussion by R. Popescu on "Bearing capacity prediction of spatially random $c - \phi$ soils"[1]

Gordon A. Fenton and D.V. Griffiths

The discussor brings up a number of good points regarding the difficulties in characterizing spatial variability of soils. The main points of concern raised by the discussor are the high coefficients of variation (COVs) considered by the authors and the methodology used to simulate the random soil property fields. It is hoped that the following discussion will shed some light on these concerns.

Regarding the range in COV considered by the authors, which was 0.1–5.0, the authors believe that it is still unknown what value(s) of COV should be used in geotechnical characterization. The appropriate COV depends on several things, such as the intensity of site investigation; the level of deterministic site characterization (e.g., higher order trend, layer-wise descriptions); and how the soil variance affects the response quantity, or engineering property, of interest (i.e., is the property itself a measure of some form of local average, or is it highly dependent on microscale "defects"?). The issue of site investigation intensity is intimately connected to the degree of deterministic site characterization, for example, if only a single value global average property is employed in the design of a footing, then the site investigation results could yield a large COV, particularly if the site is large and the investigation points are widely separated. The COV would then be interpreted as one's "uncertainty" about the value of the property at the footing if no test results were available near the footing. If a particular test result were available near the footing, then that result would be preferentially used to design the footing, and the corresponding COV would be reduced. In such a case, the site characterization moves away from a single value global average to a more detailed deterministic description that incorporates observation versus footing locations. The COV used for design depends on how the investigation data are used and on where the investigation points are relative to the footing. For one site with considerable data near the footing, the COV to be used might be quite small, whereas for another site with limited data and (or) data well removed from the footing location, the appropriate COV might be quite large.

In this sense, the comprehensive results reported by authors such as Phoon and Kulhawy (1999) are really just a start at the characterization of soil variability. They are basically reporting COVs estimated from a particular dataset, reflecting the residual variability about the locally estimated mean (or mean trend). These results tell us little about how to handle *uncertainty* about the soil properties at some distance from where the soil was actually sampled. There is much that is unknown about this problem, and considerable research that needs to be done before definitive levels of COV can be stated for any given situation. For this reason, the authors chose to perform their analysis over a wide range in COV values. This is not viewed as a recommendation that designers should be considering COVs as high as 5.0, but rather allows the results to be used in the event that a designer determines such a high COV is appropriate. Alternatively, if a lower COV seems appropriate, these results are also included in the paper.

It is not clear to the authors why the discussor is introducing the idea of the finite element model size into the choice of COV. The important issue is the estimate of the "point" COV (where point is usually some local average over a small volume) from a set of data collected in the field. How the data are collected, how the statistical analysis is carried out, and how the results are to be used will affect the value of the estimated point COV. Once that value has been determined, it is appropriate to use it in whatever numerical model one chooses. The quality of the numerical model in representing reality is another issue, but the authors have strived to produce a model that reflects the material behaviour as well as possible given current computational resources. In particular, the Local Average Subdivision method employed by the authors correctly reflects the transformation from the true point statistics to the element averages that is consistent with the continuum finite element model.

The second issue raised by the discussor has to do with the ability of the authors' simulation technique to adequately represent the prescribed random fields. The discussor is concerned that the nonlinear transformation, when going from the underlying Gaussian random field to the target soil property, affects the final correlation structure. This transforma-

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G.A. Fenton.[2] Engineering Mathematics, Dalhousie University, Halifax, NS B3J 2X4, Canada.
D.V. Griffiths. Colorado School of Mines, Golden, CO 80401, U.S.A.

[1]Appears in Canadian Geotechnical Journal, **41**: 366–367.
[2]Corresponding author (e-mail: Gordon.Fenton@dal.ca).

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tion certainly does affect the correlation structure, as it does the marginal distribution, and this is as intended. In the authors' opinion, this issue is a not a concern, as explained in the following for the particular case of the cohesion field, c.

It is generally accepted that many soil properties are reasonably well modeled by the log-normal distribution. For one, the log-normal distribution is strictly non-negative, a beneficial feature for most soil properties such as cohesion (the normal distribution approximation suffers from the fact that it allows negative soil property values). In addition, soil properties are generally measured as averages over some volume, and these averages are often *low strength dominated*, as may be expected. The authors have found that the geometric average well represents such low strength dominated soil properties. Since the distribution of a geometric average tends to the log-normal distribution by the central limit theorem, the log-normal distribution may very well be a natural distribution for many spatially varying soil properties.

If this is so, then it is also natural to estimate the parameters of the soil property log-normal distribution using the logarithm of the data in the estimation process. This applies also to the estimation of the correlation length, $\hat{\theta}_{\ln c}$. That is, if the value of $\hat{\theta}_{\ln c}$ is estimated from the logarithm of the data and is used as the correlation length of the underlying Gaussian random field, then the resulting log-normal field is as close as possible, aside from the errors in the estimation process, to the true field. It does not matter if θ_c differs from $\hat{\theta}_{\ln c}$. The value of θ_c arising after the transformation of the Gaussian field to the log-normal field will be the same as the value of $\hat{\theta}_c$ estimated from the raw data (again ignoring errors arising from using only a finite sample). This approach to the joint estimation–simulation problem *cannot* be improved upon without improving the statistical estimation process. That is, the simulation approach is optimal.

The same argument can be made about the friction angle field, namely that if the correlation length is estimated from the inverse transformed data, then the estimation–simulation method is optimal. The discussor raises a good point, however, when he says that if the correlation lengths are the same in the untransformed space, they will no longer be the same in the transformed space. This is entirely true, although they will often be still quite similar. It has to do with the fact that the two transformations are not the same and is an issue discussed in the paper. There is no simulation method that can get around this problem: if one insists that the transformed correlation lengths be equivalent, then the untransformed lengths will no longer be equivalent, and vice versa. So the concern here is really with the authors' assumption, and ensuing justification, of keeping the correlation lengths the same in the untransformed space (i.e., for the Gaussian random fields), using the claim that common changes in the constitutive nature of the soil over space will lead to different properties having similar correlation lengths. Obviously, such a contention cannot be strictly correct, since different soil properties are generally obtained using different (sometimes nonlinear) transformations from the raw data (e.g., cone penetration test results). Although the authors suspect that the correlation lengths will be similar as a result of common geologic processes, research evidence is not yet available to specify how this assumption should be applied. In any case, for the purposes of the paper, this assumption simplifies the problem without sacrificing the ability to quantify the general probabilistic behaviour of soil bearing capacity, which is the goal of the paper. The detailed consideration of differing scales must be left for future refinements. In particular, since correlation lengths for even a single soil property have yet to be established, it is probably better to determine worst-case design correlation lengths than to worry about such refinements at this time.

The authors believe that the discussor must have misinterpreted the paper when he says "The authors mention a limitation of their simulation methodology, namely, that the nonlinear mapping from the Gaussian to the non-Gaussian field destroys the spectral–correlation characteristics of the Gaussian field." No such mention was made in the paper. Perhaps the discussor meant to say that the nonlinear mapping results in a field that has different spectral–correlation characteristics than the original Gaussian field (since the mapping just produces a second field and does not affect the original Gaussian field from which it came). As discussed previously, a nonlinear transformation will always result in a change in the distribution: this is to be expected, and any simulation technique that does not allow this change to happen is not properly performing the transformation.

Lastly, the discussor raises concerns about the simulation of cross-correlation between c and ϕ. Although not explicitly stated in the paper, the cross-correlation was applied between the underlying Gaussian random fields, so that, for example, when $\rho = +1$, both properties are derived from the same (single) random field. This is believed to be reasonable, given the large uncertainty in ρ. The comment by the discussor that the results shown by the authors are not in agreement with results presented by Cherubini (2000) is not a valid comparison. Cherubini represents c and ϕ using just two random variables, rather than two random fields, and so a much stronger dependence of the results on ρ is to be expected in Cherubini's case. The treatment of soil properties as random fields, as was done by the authors, is much more realistic than a simple two random variable analysis.

Along this line, the discussor also suggests that having "different correlation distances [as a result of the nonlinear transformations] fades out the spatial variability of the overall shear strength" and may be a reason for the small effect that ρ has on bearing capacity behaviour. There is no reason that differing correlation lengths would cause any additional "fading out" (and any fading out after local averaging is just due to the usual statistical laws of averaging). The authors believe that the reason ρ has only a small effect on bearing capacity mean and variance is because it is overwhelmed in magnitude by the weakest path phenomenon, again something that cannot be seen using a single random variable for each soil property.

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