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DISCUSSION

P. W. Rowe[1]—Both Professors Mitchell and Schmertmann appreciate the importance of structure, and by this they presumably mean the particle geometrical arrangement. Perhaps if I were to comment on Schmertmann's curve-hopping system, using for simplicity pore pressure and volume changes which simulate the consolidated-drained test, I would emphasize the statement made by Professor Schmertmann that he does not take volume change rates into account. Therefore it follows that he must compare specimens at identical structure simply in order that the strength due to structure shall remain constant. He is correct in making his pressure changes small, as otherwise the dilatancy rates would differ, and the structure would change slightly. If one separates structural and interparticle strength, the limitation of comparison of specimens at the same structure is removed. But his work and mine lead to the conclusion that the Hvorslev parameters have been linked across specimens of different structure, without proper allowance for that difference. For those who accept the geometrical arrangement of soil packing as an essential physical property, the Hvorslev parameters cannot form a fundamental general basis for shear strength.

This brings me to an observation regarding the significance of "unique relations" between void ratio or water content and strength. Hvorslev made the assumption that the void ratio defined the soil state. It is now clear that this is true only if the soil structure is defined, which it may well be at the critical voids ratio state line. Soils which have achieved a random packing as a result of large strains, in which all previous stress history in the packing has been removed, may very well exhibit unique water-content strength relations. But I cannot see how this can be true for the range of strains to be considered in practice.

Since the soil structural arrangement is not fixed but is dependent directly on the applied stresses, I support those authors who maintain that in practice the type of shear test should simulate as far as possible the stress changes under consideration in the field.

H. Leussink[2]—Everybody agrees that the volumetric shear behavior of an assembly of individual grains is in reality a rather complicated mechanism. To deal with this there are two principal methods:

1. Simplify the triaxial tests as much as possible and make the grains into well-defined spheres of such a magnitude that they may be observed individually. Our laboratory has been using this method since about 1957.

2. The state of stress and strain should be made less complicated than in the triaxial cell. This method is employed by, among others, Professor Scott of the California Institute of Tech-

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nology. I hope that he will tell us something about his results.

The paper submitted by Dr. Wittke and me tries to give an analytical law of the shear behavior of various arrangements of spheres for different statuses of stress and strain. This was done already for special cases by Idel and Dantu, and at practically the same time with us, by Rowe. It proves that the equations of Rowe are identical with ours.

Basically, all the authors mentioned here assume that failure of the packings is reached by overcoming the frictional forces in all points of contact of the balls and that the relative movement of the balls is a true translation.

Dr. Wittke and I have had some doubts as to whether the phenomena observed could be explained merely by means of the theory briefly outlined in the paper. Three principal observations lead to the conclusion that the mechanism is, in reality, different and more complicated. These observations are:

1. When we fixed the deformations developed into different fractions of the shear strength by means of freezing the deformed specimens, we found that the originally plane, horizontal cross-sections were no longer plane. Two cone-shaped bodies of spheres had protruded from the end-plates into the middle region of the specimen.

2. The measured values of the bearing capacity at failure and at any rate of strain are smaller than the theoretical ones. These differences are higher for plane-strain tests than for triaxial tests. With steel balls—investigated in triaxial tests—there is only a slight difference between test results and theory.

3. The investigated packings of steel balls show almost the same bearing capacity at failure as the glass spheres in the triaxial tests, though according to the theory the surface friction of the material of the spheres should have a much greater influence. The failure strain, however, is smaller for the steel ball packings.

The first observation shows that the assumption about the deformation of the packings does not quite coincide with the behavior of the specimens. The rigid end-plates on the specimen may reinforce the upper and lower parts of the specimen by horizontal frictional forces so that these cone-shaped parts actually become bodies of different mechanical behavior. Although this phenomenon is very well-known and one might expect that concrete testers would have solved the problems which exist in such contact faces between materials of different deformation behavior, little work has actually been done in this field. Therefore, it seems to me that one of the most important difficulties in analyzing triaxial test results is the inhomogeneity of stresses and strains in the interior of the specimen, which is primarily caused by contact friction between the end-plates and the specimen.

Prof. Roscoe has also pointed out very clearly that the stresses and strains in triaxial tests are nonuniform.

For certain packings it seems that we have to be concerned with stability as well as friction problems. I mean stability in the respective arrangement of the spheres.

The fact that the glass spheres are less round than the steel balls may also have a certain influence on the failure behavior, which is not quite covered by the theory.

DR. ROWE—The limitations of Mohr-Coulomb's theory have been discussed in an earlier paper on stress-dilatancy, when it was shown that the theory had a fundamental meaning provided the assumption was made that no volume change occurred. With volume change included in the c, ϕ parameters, the theory is essentially an exceedingly useful engineering tool, but nothing more. Since the volume changes depend on the principal stress system, and since this differs between types of test, one cannot expect to find universal c, ϕ values for a soil independent of the method of test.

However this same criticism arises with any of the classical suggestions for the yield of a plastic material. Such an ideal material is considered to exhibit constant properties, to be determined by the test, and which are not themselves altered by the test system. However, if we consider the simple case of a drained sand, its structure varies widely throughout the loading and its strength depends not only on the interparticle friction which may be mobilized throughout but also on the maximum structural strength which can be developed.

Structure depends on whether the soil is cohesionless or cohesive, nonsensitive or sensitive, on the stress system and therefore on the design of the test apparatus; on stress history and therefore on the method of shear test. It follows that there are a very large number of variables to be examined by trial and error. If all the testing were perfect, the chance that any one of the classical criteria would satisfy every form of test for every soil must be considered remote indeed.

On the other hand, if a particular soil is deformed to such an extent that it is remolded to a singular random structure which is independent of stress history and system as, for example, at the critical voids ratio, then in this condition, which is generally outside the range of practical interest, classical treatment of soil as a plastic material may lead to success.

The basic reason, therefore, for the limitation of the Mohr-Coulomb theory and all classical plasticity theories is failure to measure the influence of structure as a separate parameter. This limitation does not arise with a treatment of soil as a particulate assembly. An initial simple treatment leads to the expression of the effective stress ratio of a soil in terms of the interparticle cohesion and friction components and an angle of interlocking, α, which is a measure of structure. Such treatment also allows an application of the test results to practice in a manner rather similar to that at present associated with the Mohr-Coulomb theory.

I am completely convinced that it is just as necessary to study the internal components of the effective strength of a soil matrix as it was necessary to recognize the components of pressure within an undrained soil specimen.

In this connection I welcome the paper by H. Leussink and W. Wittke who obtained in their Eq (3) an expression identical to that which I derived earlier for any ideal packing, namely,

$$\frac{\sigma_1'}{\sigma_3'} = \tan \alpha \tan (\phi_\mu + \beta)$$

The form of this is important because it can be shown to describe the behavior of any granular packing during deformation and failure. In contrast, R. Scott gives a higher and less critical value in the recent textbook mentioned by Professor Leussink.

The experimental observations illustrate that the peak is reached after larger strains than for packings of steel ball bearings and this may well be associated with lack of perfection of glass spheres. This may account in part for the lower test observations in the triaxial cell compared with the plane-strain test.

W. D. Liam Finn and H. K. Mittal also consider the plane-strain case but assume an E value independent of principal stress direction and adopt a constant value of μ. Both these assumptions are invalid and, to a first order of approximation, using stress-dilatancy theory, the instantaneous Poisson's ratio is directly proportional to the effective stress ratios, shown in the authors' Eqs (12) and (13).