DISCUSSION

N. D. NATHAN[1]—The authors have shown certain relationships between principal stress ratios and strains at failure for various values of the intermediate principal stress. Limiting themselves to cohesionless materials, they have used the maximum principal stress ratio as the criterion of failure and have drawn attention to its relationship to φ, the maximum angle of internal friction, or the angle of shearing resistance.

For example, assuming a constant value of Poisson's ratio $\mu = \frac{1}{2}$, and a constant value of the secant modulus of elasticity at failure, they show that, for equal values of φ, the strain at failure in a plane-strain test, ϵ_p, would be 75 per cent of that in a triaxial test, ϵ_t.

Since no reason is offered to suggest that φ should be any different in the plane-strain test, this reasoning would, in the first instance, merely lead one to expect $\epsilon_p = 0.75\ \epsilon_t$. However, the observed fact is that φ usually is greater in the case of plane strain. The authors' theory, then, consists of the postulate that the upper limit of strain at failure will be that observed in triaxial tests with $\sigma_2 = \sigma_3$; and they deduce thence an upper limit of φ, which is shown to satisfy some experimental observations made in two other types of test: with $\epsilon_2 = 0$, and with $\sigma_2 = \sigma_1$. The authors then state that an upper bound can be deduced for any stress system.

As was pointed out above, no *a priori* reason is advanced to indicate that ϵ_p should be any greater than 75 per cent of ϵ_t, and it is difficult, therefore, to see why it should never be any greater than 100 per cent of ϵ_t. Thus the authors' upper bound to φ seems to owe its validity to the fact that it has not been transgressed by the cited observations.

Their lower bound to φ is the value observed in the triaxial test; again, no reason is offered beyond the experimental observations as to why φ_p should be at all different from φ_t, so this bound, too, is valid because it has not yet been caught out.

It has been suggested[2] that the reason for the higher values of φ when $\sigma_2 > \sigma_3$ is that in this case the soil is forced to fail in planes parallel to the direction of σ_2, and any preferred failure planes in zones of weakness not so oriented are eliminated. This argument, if accepted, does lend authenticity to the authors' lower bound, but casts some suspicion on their upper bound. The quoted explanation would suggest that, once σ_2 has reached a value high enough to force the failure planes to be parallel to its own direction, σ_2 would have no further effect on φ.

In summary, the writer feels that changing the question "how high is φ at failure?" to the question "how high is the strain at failure?" does not really contribute to its answer, unless some reason can be advanced as to why the strain should not exceed the chosen

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[2] R. F. Scott, *Principles of Soil Mechanics*, Addison Wesley, Reading, Mass., 1963, p. 312.

(triaxial test) value. The authors' predicted upper bound has the doubtful virtue of varying with the relative value of intermediate principal stress; in the light of one tentative explanation of the higher φ, this may be positively misleading when $(\sigma_2 - \sigma_3)$ exceeds a value sufficient to control the direction of the failure planes.

A final point may be worth making in connection with the relationship between φ and strain: in their formulas, the authors have assumed plane strain from the first application of stress; in their own tests, the plane-strain condition was imposed only during the application of deviator stresses; in a natural soil, plane strain may follow one-dimensional consolidation. With a value of $\mu = \frac{1}{2}$, the strains under ambient stresses are zero, and the authors' formulas serve for all cases. With a general μ, and equal strains under deviator stresses, which follow isotropic consolidation, Eq (12), for example, becomes

$$\sin \phi_p = \frac{(\sigma_1/\sigma_3)_t - 1}{(\sigma_1/\sigma_3)_t + (1 - 2\mu^2)}.$$

With $\mu = \frac{1}{2}$, this reduces to the authors' Eq (11), but, with other values of μ, it gives slightly different results than does their Eq (12). In pursuing the relationships between strains in various stress conditions, previous strain history should be borne in mind.

W. WITTKE[3]—In the calculation of the axial strain of a triaxial or a plane-strain specimen (ϵ_t ; ϵ_p , Eqs (6) and (7)), the authors make the assumption that the soil behaves elastically until failure occurs, or in other words, that there is a linear relationship between stresses and strains. The test results (Figs. 3 and 4), however, show that this assumption is not quite valid. The tests described in the paper by Prof. Leussink and me lead to the same conclusion. The measured stress-strain relationship differs from a straight line.

Furthermore, the assumption that the strains at failure in a triaxial and a plane-strain test (ϵ_t and ϵ_p) are equal is not quite correct. This was already mentioned by the authors themselves and by Prof. Meyerhof in his general report. Our test results show, for instance, that the densest hexagonal packing at failure has a strain of $\epsilon_t \approx 2.0$ per cent for the triaxial conditions and a value of $\epsilon_p \approx 1.5$ per cent for plane-strain conditions.

So the two basic assumptions made in this paper do not correspond with the test results, and the reported coincidence of theory and test results could be fortuitous. If the theory, for instance, is applied to the test results obtained with regular packings of spheres it leads to wrong values.

Though possibly a way of calculating plane-strain strength from triaxial test results is shown, it may be necessary to add some corrections regarding the present and future test results.

W. D. LIAM FINN AND H. K. MITTAL (*authors' closure*)—The authors appreciate the discussion presented by Nathan and Wittke.

Nathan objects to the upper and lower bounds determined in the paper because they rest on only presently available experimental data and no *a priori* reasons are given. It is the authors' contention that experimental data is the proper foundation for any empirical theory.

Analysis of regular packings of uniform spheres in states of uniform dilatation demonstrates that the effective principal stress ratio in plane strain will always be higher than in the triaxial test,[4] although

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[4] See p. 77.

in such packings there are no preferred failure planes in zones of weakness as postulated by Nathan.

Nathan's remarks about previous strain history are well taken and for values of μ other than 0.5, if one wishes to consider an incremental theory in which strains are measured from the consolidated equilibrium condition, and his equation is correct. The authors' tests did not start from a condition of plane strain, but the other test data quoted are not open to this objection. It is not possible with presently available data to decide whether a continuous or incremental theory is preferable.

Wittke in his discussion and Meyerhof in his general report give the impression that their interpretation of the term upper bound is not that usually held by workers in applied mechanics. As the authors clearly state, failure in plane strain occurs at a smaller axial strain than in the triaxial test. The authors assumption that the strains are the same imposes a kinematic constraint on the deformation in plane strain. The imposition of such a constraint leads to an upper-bound solution. Thus the results are not fortuitous but expected. The use of a secant modulus to approximate a nonlinear stress-strain curve is commonplace; in fact, until a general stress-strain theory for materials with nonlinear stress-strain curves is developed no other approach is feasible.