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DISCUSSION

Investigating six-degree-of-freedom loading of shallow foundations on sand

B. BIENEN, B. W. BYRNE, G. T. HOULSBY and M. J. CASSIDY (2005). *Géotechnique* **56**, No. 6, 367–379

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The authors have presented an impressive set of laboratory experiments investigating the elasto-plastic behaviour of circular shallow foundations. This is particularly valuable as load conditions start to approach failure. However, most such foundations will (hopefully) spend most of their operating lives at lower stress levels, in which region elasticity would normally be assumed to be the dominating consideration in evaluating response—even though it is possibly the shortest section in the paper.

In the elastic stiffness matrix for circular footings, though, I would query the need to use stiffness coefficients from finite element analysis, when most engineers are unlikely to do this as a matter of course, particularly when perfectly good analytic results are available, which will generally speaking be a designer's first reference point. If, in the vertical direction, for instance, the conventional equation $dV = 4GRdw/(1 - \nu)$ is used, then an identical result to Doherty & Deeks (2003) is produced, if a Poisson's ratio $\nu = 0{\cdot}311$ is assumed. Clearly, in real life, there would be some latitude in the value of Poisson's ratio that might be assumed, but in reality the likely range is very small and in most dry sands would be assumed to be between 0·3 and 0·35 (the controlling factor in real life being the degree of saturation of any sand). Ideally this should be measured independently in some way (as indeed should G, although one then has the problem of deciding where this should be, and what the appropriate stress level might be). The paper doesn't give any guidance on this important matter, the authors having been satisfied on this occasion with back-calculating an equivalent value of G from Doherty & Deeks' vertical stiffness coefficient after the load test. However, this is not usually a luxury available to a practising designer, and still basically produces a single value of modulus, without any differentiation as to stress level or position under the foundation. Some independent prior determination of the soil modulus would be desirable, so that a comparison could be made with the results of a prior reasonable prediction. The Leighton Buzzard sand used was indeed a fairly well-established soil, although the soil characteristics listed in Table 1 do not extend to any independent value of modulus.

It is not intended to trivialise the determination of modulus (which might have been done perhaps in situ with a bender element, although in real life laboratory tests might also be conducted). However, in the absence of any independent experimental data, and if one is simply going to assign a single value of modulus to an assumed homogeneous subsoil, then use of the equivalent elastic stiffness coefficients would seem to be about as good an assumption as any other, and has the advantage of being well defined. (The great advantage of using numerical solutions like Doherty & Deeks is in cases of significant embedment, or when the soil stiffness is known to vary with depth in a well-defined power law.)

The discusser would suggest that in general design there is not yet the need to deviate from using established analytic results, most easily by writing $k_v = 2/(1 - \nu)$, $k_h = 4/(3(1 - \upsilon))$, $k_q = \frac{2}{3}$, $k_m = 1/(3 - 3\nu)$ and $k_c = 4\pi/(3 - 3\nu)$, although discussion on the details is welcomed. While there may still be some debate about the optimum nature of the latter of these terms, since in reality the limiting factor on the accuracy of any calculation will be the determination of G, other considerations are likely to be secondary in comparison. If a significantly more complicated problem is encountered, then it is probably easiest to perform a numerical computation directly. For reasonably uniform soil, the elastic solutions are in fact quite good—the major exception being in the nature of the off-diagonal term k_c in the stiffness matrix, which (if results like Doherty & Deeks are anything to go by) is substantially overestimated by elastic theory. This discrepancy is probably due to the fact that the relevant deflections are very sensitive to assumed behaviour at the edge of the foundation, where non-linear effects will be most prominent, and where in practice localised yielding will occur even at low stress levels. If the authors are aware of a good study of this problem, the discusser would be interested to hear about it.

Incidentally, the relative density of the soil tested in this study was unusually loose, at only 5%, with no other in-place density tested. Without wishing to trivialise the difficulties of conducting such an impressive experimental programme, readers would no doubt be curious to know whether there was a particular explanation of this, as it would presumably have been easier to test the sand in a somewhat denser state, which would also correspond more closely to normal field conditions.

Authors' reply

The authors thank Dr Morris for his interest in their paper.

Dr Morris states that, as most foundations would be designed not to be near failure, then their response would be dominated by elasticity, while the paper concentrated on foundations undergoing larger plastic deformations. This viewpoint is perhaps conditioned from onshore practice. In offshore foundation design, which provided much of the motivation for the research, there are a number of foundation types (notably the spudcans of jack-up units) where the foundation inevitably undergoes large plastic deformation during installation and possibly even during service. Furthermore, we consider the elasticity problem (even for the full six-degree-of-freedom case) to be relatively well understood, and so have concentrated our research efforts on inelastic behaviour.

Dr Morris queries our use of the Doherty & Deeks (2003) solutions for elastic response, implying that practising engineers would not have access to such techniques, and that a similar result could have been achieved by adopting a conventional solution and a Poisson's ratio $\nu = 0{\cdot}311$. However, no dry sand (as was tested) has a Poisson's ratio this high, with measured values being typically in the range 0·15 to

0·2. The common practice of adopting a Poisson's ratio in the region of 0·3 appears to be borrowed from experience of metal elasticity, and is inappropriate in geotechnical applications. While not suggesting that finite element methods need to be used to determine elasticity response routinely, in our view published solutions such as those of Doherty & Deeks (2003) are the best available means for modelling elastic behaviour (given of course the restrictions this implies), and practising engineers should be encouraged to use these best available methods. If they do not, then what purpose does a journal such as *Géotechnique* serve?

We agree that it would have been desirable to have had an independent measurement of shear modulus, and its variation with depth, by using bender elements, but such measurements were not part of our experimental programme, which was focusing on inelastic response.

Dr Morris quotes a series of 'established' analytical results, but does not quote their source. We have the following comments on the values he uses.

(*a*) We have not previously seen the expression he gives for k_h. The usual expressions, both of which involve approximations, are $k_h = 4/(2 - \nu)$ or $k_h = 16(1 - \nu)/(7 - 8\nu)$. We note, however, that these reduce to the same value as Morris's at $\nu = 0{\cdot}5$.

(*b*) We are not familiar with his expression for k_c. This expression is, however, inconsistent with the known solution at $\nu = 0{\cdot}5$, which is $k_c = 0$.

Useful discussions of the above issues can be found in Bell (1991) and Ngo Tran (1996).

The final point that Dr Morris raises is in relation to the choice of a very low relative density for the testing programme. As is well known, one problem with laboratory-floor model testing in sand is that, if medium to high relative densities are used, then the dilation of the sand can be excessive, and the angle of friction correspondingly too high. One solution is to test at a lower relative density in the laboratory, so that dilation rates in laboratory and field are comparable. For this reason we believe that our data are relevant to significantly higher relative densities in the field, although the exact value would depend on the field scale. Our experience is that, once the results are normalised by the value of the maximum applied vertical load, the influence of relative density on the results is quite weak, so that in fact our results would have quite a wide applicability.

REFERENCES

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