INTERNATIONAL JOURNAL FOR NUMERICAL AND ANALYTICAL METHODS IN GEOMECHANICS, VOL. 7, 385–393 (1983)

LETTERS TO THE EDITOR

ELASTO-PLASTIC ANALYSES OF DEEP FOUNDATIONS IN COHESIVE SOIL

by D. V. Griffiths, *Int. j. numer. anal. methods geomech.*, **6**, 211–218 (1982)

Griffiths[1] has used the finite element method to investigate the behaviour of deep foundations in a cohesive soil. Particular attention has been focused on the problem of predicting collapse loads accurately using the displacement type of formulation. This question is obviously of practical significance, and has received attention on several occasions.[2-5] If, for a given soil model, finite elements can be used to predict deformations accurately right up to incipient collapse, then it is no longer necessary to make the traditional distinction between settlement and stability analysis.

In his paper, Griffiths[1] has used the 8-noded quadrilateral element with 2×2 (reduced) integration for undrained plasticity analysis. As discussed by Sloan and Randolph,[4] reduced integration has the beneficial effect of decreasing the total number of incompressibility constraints on the nodal degrees of freedom, thus avoiding the well-known phenomenon of 'locking'. Moreover, since the number of integration points for each element is reduced, the cost of solution is also reduced. There are, however, certain aspects of this approach which warrant further attention. Recently, Nagtegaal and De Jong[6] have noted that, for large strain analysis under conditions of axial symmetry, the use of reduced integration with the 8-noded element may lead to the development of curious deformation patterns. In this note, it will be shown that similar problems may arise in small strain applications where the material is modelled as elastic perfectly plastic.

Figure 1 illustrates the initial and deformed meshes for undrained analysis of a smooth flexible strip footing on an elastic perfectly-plastic Tresca material. In this analysis, with 25 elements and 192 degrees of freedom, 8-noded quadrilateral elements were employed with reduced integration. The total number of integration points is equal to 100. The Euler integration procedure, with an equilibrium correction at each of the 50 load steps, was used to solve the governing non-linear equations, and all computations were conducted in double precision on an IBM 370/165. An equilibrium check, based on the ratio of the norm of unbalanced forces to the norm of applied forces, indicated that equilibrium was satisfied to within 1 per cent for all load steps until collapse occurred at a pressure of approximately $5{\cdot}22\ C_u$. As collapse is approached, the elements in the vicinity of the footing deform in a peculiar pattern which is similar to that observed by Nagtegaal and De Jong.[6] This behaviour is due to the dominance of zero energy modes as a large region of the continuum becomes plastic. Figure 2 illustrates the initial and deformed meshes for the same problem, but analysed using the cubic strain triangle as discussed by Sloan and Randolph[4] (8 elements, 162 degrees of freedom, and a total of 96 integration points). This element requires 12 integration points to evaluate the element stiffness matrices exactly under plane strain conditions. Because the stiffnesses are exact, the deformed mesh does not display the 'barrelling' phenomenon associated with the reduced integration results. The collapse pressure obtained from this analysis, using the same solution algorithm and load steps described previously, was again in the vicinity of $5{\cdot}22\ C_u$. By way of interest, the CPU times required for the two meshes were identical, taking a total of 37 seconds.

To illustrate that difficulties may also arise when reduced integration is used for other classes of problems, Figure 3 shows the initial and deformed meshes for an embankment analysis with a non-linear elastic soil model. This plot has been taken from Reference 7. The element with the heavy outline appears to be deforming in a pattern which is very close to a zero strain energy mode.

There are a number of potential advantages in using the cubic strain triangle with exact integration for plastic collapse calculations. These are:

(1) No problems are encountered with barrelling or zero strain energy modes, since the element stiffness matrices are evaluated exactly. As emphasized by Bathe,[8] reliability is an extremely important quality in finite element analysis, particularly in large scale computations.

(2) The element is efficient. Timing runs reported by Sloan[5] indicate that the cubic strain triangle solutions cost no more than

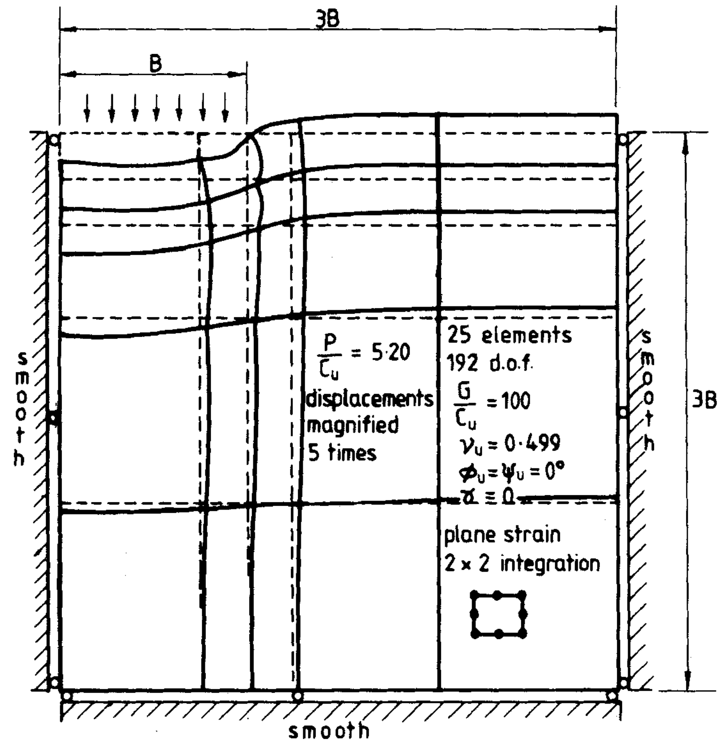

Figure 1. Deformation response for smooth flexible strip footing on a purely cohesive layer; 8-noded quadrilateral element results with reduced (2 × 2) integration

equivalent solutions obtained using the 8 noded quadrilateral with reduced integration. The integration rules that are available for triangles are very efficient (only 12 points are required to evaluate the stiffness matrix exactly for an element with a quartic displacement expansion).

(3) No substantial modifications to a traditional stiffness program are necessary, it is simply a matter of incorporating another shape function subroutine. The information necessary for implementing the cubic strain triangle may be found in Reference 4. Contrary to the claim of Griffiths[1] that the cubic strain triangle is 'extremely complicated', the additional coding required to implement this element is minimal (an additional 30 lines or so compared with the 8-noded element). Moreover, for manual data preparation, there is a positive advantage in using high order formulations as fewer elements need to be specified (compare the 8 elements of Figure 2 with the 25 elements of Figure 1). In cases where all element boundaries are straight, the co-ordinates for all non-vertex nodes may be generated automatically by linear interpolation.

(4) Recent mathematical studies by Babuska and Szabo[9] on covergence of hierarchic displacement elements have shown that, for incompressible elasticity, high order formulations are more accurate than low order formulations (for a given number of degrees of freedom). Indeed, based on their investigations, these authors recommend that elements with at least a cubic displacement expansion should be employed for plane strain analysis of incompressible materials.

(5) The cubic strain triangle is also suitable for analysis of dilatant-frictional behaviour (e.g. soil masses which are modelled using a Mohr–Coulomb yield function). Numerical experiments by Sloan[5] indicate that accurate estimates of collapse may be achieved for both plane strain and axisymmetric conditions.

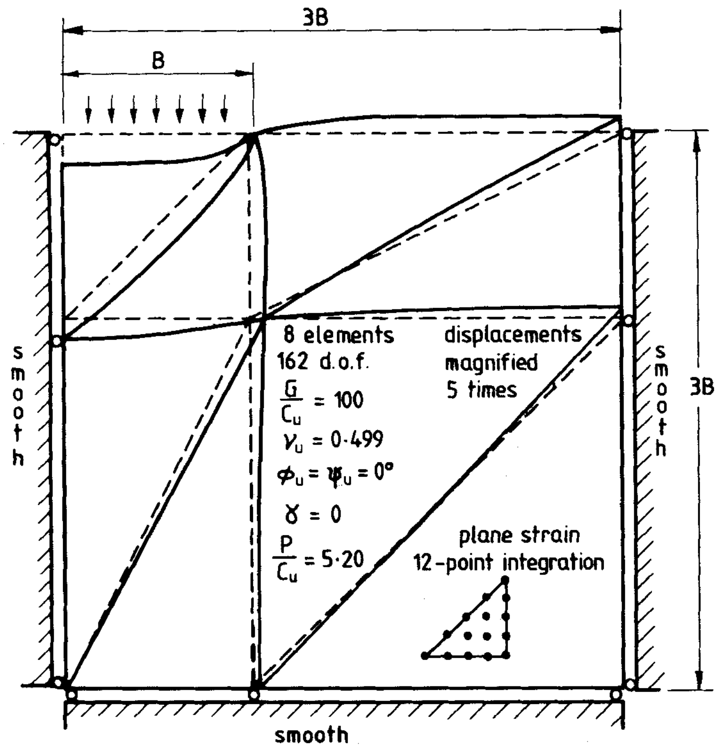

Figure 2. Deformation response for smooth flexible strip footing on purely cohesive layer; cubic strain triangle results with exact 12-point integration

As a final point, it is worth recalling that the displacement finite element method, with exact integration, is usually viewed as giving displacements which are smaller than the exact displacements. This bounding characteristic is useful in practice, particularly for difficult problems where the collapse load can only be inferred from successive analyses with finer mesh configurations.

It is comforting to note that Figure 4 of Reference 1 indicates that the collapse loads obtained from finite element analysis fall between the upper and lower bounds derived by Gunn.[10] In a previous reference, however, Griffiths (Reference 11, Figure 21) presented similar results of N_c versus H/B for deep foundations where the finite element solutions yielded collapse loads which were

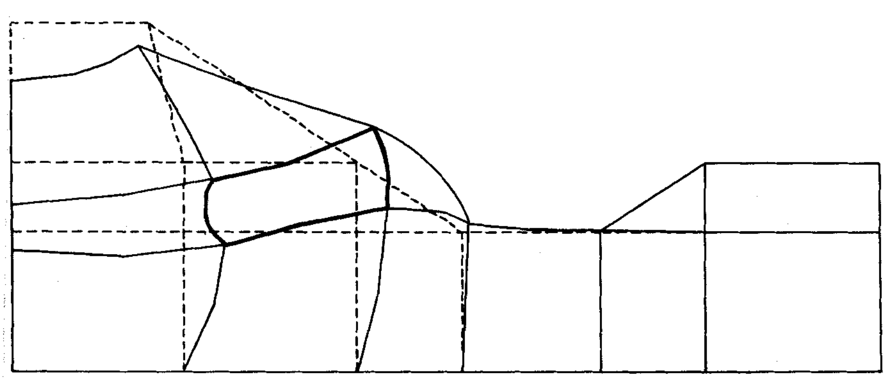

Figure 3. Deformation response for embankment analysis

below the lower bounds given by Gunn.[10] This may imply that the use of reduced integration can lead to collapse loads which are below the lower bounds furnished from plasticity theory. In using high order elements with exact integration of the stiffness matrices, the writer has never observed this type of behaviour.

S. W. SLOAN
Department of Engineering
Trumpington Street
Cambridge CB2 1PZ, U.K.

REFERENCES

1. D. V. Griffiths, 'Elasto-plastic analyses of deep foundations in cohesive soil', *Int. j. numer. anal. methods geomech.*, **6**, 211–218 (1982).
2. J. C. Nagtegaal, D. M. Parks, and J. R. Rice, 'On numerically accurate finite element solutions in the fully plastic range', *Computer Methods in Applied Mechanics and Engineering*, **4**, 153–177 (1975).
3. C. T. Toh and S. W. Sloan, 'Finite element analyses of isotropic and anisotropic cohesive soils with a view to predicting impending collapse', *Int. j. numer. anal. methods geomech.*, **4**, 1–23 (1980).
4. S. W. Sloan and M. F. Randolph, 'Numerical prediction of collapse loads using the finite element method', *Int. j. numer. anal. methods geomech.*, **6**, 47–76 (1981).
5. S. W. Sloan, 'Numerical analysis of incompressible and plastic solids using finite elements, *Ph.D. Thesis*, Department of Engineering, Cambridge University, 1981.
6. J. C. Nagtegaal and J. E. De Jong, 'Some computational aspects of elastic-plastic large strain analysis', *Int. J. Num. Meth. Eng.*, **17**, 15–41 (1981).
7. *Finite Elements in Geotechnical Engineering*, Workshop Manual, Swansea, 4–7 January 1977.
8. K. J. Bathe, *Finite Element Procedures in Engineering Analysis*, Prentice-Hall Inc., Englewood Cliffs, New Jersey, 1982.
9. I. Babuska and B. Szabo, 'On the rates of convergence of the finite element method', *Int. J. Num. Meth. Eng.*, **18**, 323–341 (1982)
10. M. J. Gunn, 'Limit analysis of undrained stability problems using a very small computer', *Symposium on Computer Applications to Geotechnical Problems in Highway Engineering*, Cambridge, 5–30 (1980).
11. D. V. Griffiths, 'Finite element analysis of walls, footing and slopes', *Symposium on Computer Applications to Geotechnical Problems in Highway Enginnering*, Cambridge, 122–146 (1980).

AUTHOR'S REPLY TO SLOAN'S DISCUSSION*

As far as element integration is concerned, there is no argument over which integration scheme (exact or reduced) is more rigorous in a mathematical sense. It should not be forgotten, however, that finite element techniques are approximate numerical methods for the solution of differential equations, irrespective of the integration scheme used.

The debate here is about whether the use of a high order element with exact integration (15-noded triangle with 12 integrating points) can justify its use in preference to a lower order element with reduced integration (e.g. 8-noded quadrilateral with 4 integrating points) in the prediction of collapse loads for geotechnical problems.

In order to probe this point, it must be demonstrated that the higher order element gives significantly better collapse predictions than the lower order method, especially for the purposes of engineers. Widespread acceptance of finite element techniques in industry has been slow and the use of constant strain triangles is still common. The writer's concern is that the profession might be deterred from finite element collapse predictions if the impression is given that high order elements *must* be used for this type of problem.

Consideration of the evidence of available results indicates that the surface footing problem does not justify the use of high order elements. For example, Griffiths[1] has produced results for surface footings on a wide range of soil types, including cohesionless soils, using 8-noded quadrilaterals with reduced integration which were in close agreement with published closed form and approximate solutions. It may be that 'difficult problems' will come to light in which the higher order elements are an advantage, but the lower order techniques with reduced integration have been used with success[2] in a wide range of geotechnical collapse problems including slope stability and earth pressures.

Referring to Sloan's discussion, the barrelling shown in Figure 1 does not appear particularly serious in view of the crude mesh used. A finer mesh would reduce this effect, but in any case this phenomenon does not appear to have had an influence on the computed collapse load. Indeed, Sloan achieved the same collapse load using both element types (Figures 1 and 2) which fully supports one of the main conclusions of the paper under discussion, namely that provided collapse

* Preceding letter.

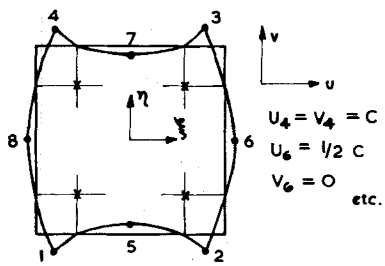

Figure A. Zero energy deformation for 8-noded quadrilateral element with reduced integration

loads are the subject of the analysis, the lower order methods with reduced integration perform adequately.

The barrelling elements in the upper part of the mesh are not, in fact, deforming with zero energy. Zero energy modes only occur when a pattern of nodal displacements produces a strain field that is zero at all quadrature points (Figure A). This definition cannot be applied to the elements considered here because at least two sides of these elements remain approximately straight, and if anything curve away from the zero energy mode (Figure B).

A further point raised by Cooke,[3] is that even if a quadratic element did deform with zero energy, its neighbours could not. This implies that the zero energy mode should not be of major

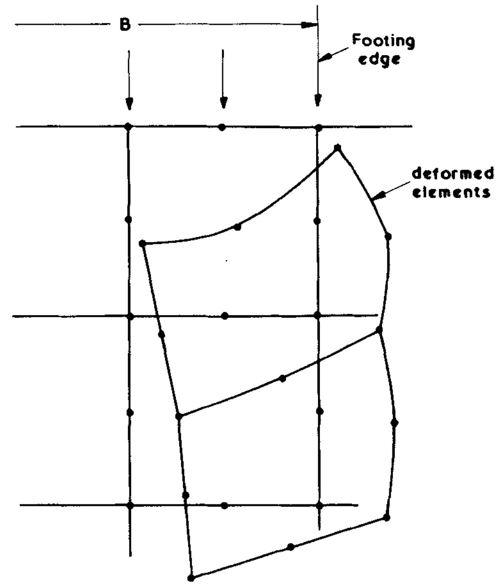

Figure B. Deformation response near footing edge. 8-noded quadrilateral element with reduced integration

consequence in meshes of quadratic elements using reduced integration.

Sloan also refers to the prediction of deformation up to incipient collapse. In view of this it is interesting to observe from Figures 1 and 2 that,

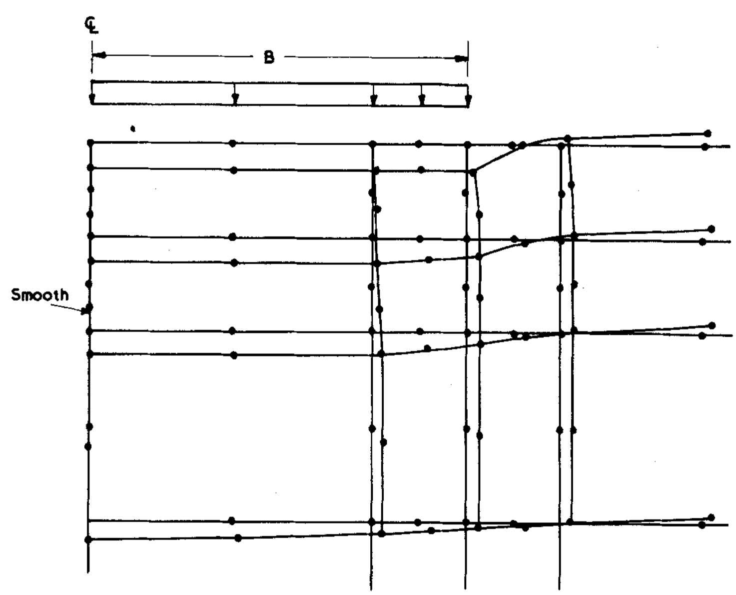

Figure C. Deformation response of a cohesive layer under a smooth rigid footing. 8-noded quadrilateral elements with reduced integration in spite of the barrelling and the crude meshes, the predicted surface displacements beneath and adjacent to the footing are in close agreement.

Finally, using the same mesh as in Figures 1 and 2, a displacement control solution was attempted. This involves prescribing equal vertical displacements at the footing nodes and back-calculating the stresses by either integrating the stresses over the element to give equivalent nodal forces, or simply averaging the vertical stress component in the first row of integrating points beneath the displaced nodes. Physically this method models a rigid footing, and is favoured by the present writer for the solution of bearing capacity problems. It is interesting to note in Figure C that by forcing the displaced nodes to remain in a horizontal line, the barrelling phenomenon occurring in the upper elements is less than when the footing is assumed to be flexible under load control.

D. V. GRIFFITHS
University of Manchester

REFERENCES

1. D. V. Griffiths, 'Computation of bearing capacity factors using finite elements', *Geotechnique*, **32**, (3) (1982).
2. D. V. Griffiths, 'Finite element analyses of walls, footings and slopes', *Ph.D. Thesis*, Simon Engineering Laboratories, University of Manchester (1980).
3. R. D. Cooke, *Concepts and Applications of Finite Element Analysis*, Wiley, (2nd edn), pp. 134–137.

INEXPENSIVE BUT TECHNICALLY SOUND MINE PILLAR DESIGN ANALYSIS

by W. G. Pariseau, *Int. j. numer. anal. methods geomech.*, **5**, 429–447 (1981)

I should like to discuss a few points which cropped up in my mind as regards this interesting paper.

It is indeed surprising that a three-dimensional situation can thus be reduced to a two-dimensional one by merely changing the pillar loading to an 'equivalent' value. The author has modelled relatively flat pillars with a fixed working height of 8 ft (2·43 m). It would perhaps be interesting to study this equivalence between 2-D and 3-D solutions on slender pillars, say $5 \times 5 \times 5$ m high or $3 \times 3 \times 2$ m high, which do occur in practice. The behaviour of the corners of such pillars becomes critical. I have a feeling that the stresses at the corners will differ by a magnitude which will depend on the slenderness of pillars.

The 3-D and 2-D finite element pillar models of Figures 3 and 4 could be reduced significantly in size if the middle horizontal plane of the pillar is assumed to remain plane after excavation—an assumption which is quite logical and common in pillar design, unless the roof and floor have widely different properties. Since the purpose of the paper appears to be to compare two methods, considerable effort would be saved if a symmetric horizontal midplane (zero vertical displacements) were introduced.

One thing that is not very clear from the paper is how virgin or pre-excavation stresses were accounted for and what was the stress field assumed. This, I think, is important since the total stresses in the pillar will depend greatly on the pre-excavation stresses. Will it not be necessary to apply some sort of an equivalence to the virgin stress field as well?

I would also like to know the reason for using the Drucker–Prager failure criterion rather than the Mohr or Coulomb–Navier criterion. Can the spot safety factor be used for predicting the strength of a pillar as a whole? Unless this is possible, we shall be required again to fall back upon empirical pillar strength formulae in spite of exhaustive finite element stress analyses.

P. R. SHEOREY
Central Mining Research Station
Dhanbad, India.

RESPONSE TO SHEOREY'S LETTER*

Dr. Sheorey raises four points in his letter. The first concerns the 2D–3D equivalence for pillars of different dimensions used in the numerical work reported in the paper. The equivalence is independent of the actual dimensions and so should produce similar results. The stress distribution will change, of course, but they change in the 2D, 3D and 3D equivalent as well. I do not think pillars that are as wide or wider than they are high qualify as 'slender'.

The second point concerns the use of horizontal symmetry and the assumption that the midplane of the pillar remains plane. If the geology justifies or indicates such symmetry, then indeed it would reduce mesh size by half and thus cost. However, this seems quite unlikely to be the case, especially in coal mines. One of the advantages of the FEM is the ability to include geologic effects such as stratification. The 2D runs cost less than a dollar in many cases; the savings would be insignificant relative to the total job cost. In 3D, the savings would be more of the order of 75 dollars, but the quality of the output would be suspect and perhaps not really worth while obtaining.

The third point concerns the pre-excavation stress field. This is taken to be the unit weight of overburden times depth in the vertical direction. The pre-mining horizonatal stresses are the vertical pre-mining stress times $\nu/(1-\nu)$ where ν is Poisson's ratio at the point of interest in the homogeneous case. In the layered case, the horizontal stresses are calculated on the basis of a compatible initial strain state that is also an equilibrium state. If the 1-direction is vertical and the 2- and 3-directions are horizontal, then the initial strain state is $\sigma_1 = \gamma h/E_{11}$ where $\gamma h = \sigma_1$, γ = unit weight, h = depth, E_{11} = 1-row, 1-column element of the material properties matrix in $\{\sigma\} = [E]\{\varepsilon\}$ (where σ = stress). The horizontal pre-excavation stresses are $\sigma_2 = E_{21}\sigma_1/E_{11}$ and $\sigma_3 = E_{31}\sigma_1/E_{11}$. The ratios E_{21}/E_{11} and E_{31}/E_{11} are the K_0's for the anisotropic layer considered.

The last question contains two parts. The choice of yield criteria in site specific problem would be based on experimental data. The choice in the paper is based on pressure dependent yield required for geologic media and programming efficiency. The latter factor refers to the complication of multiple yield surfaces (planes) needed in the 3D Mohr–Coulomb treatment that even in 2D requires consideration of all three direct stresses.

The local or spot safety can be used to calculate a global pillar safety factor, say by averaging over the pillar midplane. However, if the design is elastic, then safety prevails if the minimum local safety factor is adequate. Plastic or yield pillar design is another story.

The conditions in the paper were such as to result in an entirely elastic pillar. However, the program used is elastic–plastic, so that the redistribution of stress that occurs with inelasticity can be followed if circumstances dictate. However, the total load (force) on the pillar is the same independent of the distribution, the 2D–3D equivalence still prevails. In this regard it is worth while to mention again that the safety factor computed by the extraction ratio approach is comparable to the most severely stressed element which shows the lowest local safety factor in the pillar. The reason for this, I suspect, is that both are essentially uniaxial calculations.

I appreciate Dr Sheorey's interest in the subject and hope that others might try the 2D–3D equivalence on other geometrics and geologics.

W. G. PARISEAU
Department of Mining Engineering
University of Utah, U.S.A.

* Preceding Letter.

CONSEQUENCES OF DEVIATORIC NORMALITY IN PLASTICITY WITH ISOTROPIC STRAIN HARDENING

by R. Baker and C. S. Desai, *Int. J. numer. anal. methods geomech.*, **6**, 383–390 (1982)

The paper by Baker and Desai discusses an interesting aspect of the description of plastic soil behaviour, namely the simplification obtained by assuming deviatoric normality. It seems to contain a logical error, however, which undermines the conclusions reached in the paper. Equations (6) and (7) give two expressions for the deviatoric part of the plastic strain tensor, one containing a proportionality coefficient α, and the other a factor λ. It is then stated that the two vectors $\partial F_1/\partial\sigma_{ij}$ and $\partial Q_1/\partial\sigma_{ij}$ must be equal in order to have the same direction, which leads to equation (9): $\alpha=\lambda$. This seems to be in error, because two vectors can very well have a different length and still be parallel. Thus, there is no solid ground for the deduction that the two parameters α and λ are equal. As this equality plays a vital and non-trivial role in the remaining part of the paper, the conclusions regarding the validity of the equations are not justified. The class of functions described by equation (22) possesses the property of deviatoric normality, but deviatoric normality does not necessarily imply the validity of that equation.

A. VERRUIJT
Department of Civil Engineering
University of Delft, The Netherlands

AUTHORS' REPLY TO VERRUIJT'S LETTER

The writers thank Verruijt for his discussion. The main point raised in his comment, namely that the class of functions F and Q which satisfy deviatoric normality is wider than that defined in equation (22), is essentially correct. We will show, however, that this wider class of functions is of the same general form as that derived by us.

The limitation of our previous analysis is the result of equation (8). This equation, which requires the equality of the corresponding components of the two gradient vectors $(\partial Q_1/\partial\sigma_{ij})_D$ and $(\partial F_1/\partial\sigma_{ij})_D$, is a sufficient, but not necessary condition for deviatoric normality. Deviatoric normality requires only that these two vectors be parallel, but imposes no restriction on their relative magnitude. A necessary and sufficient condition for deviatoric normality is therefore the equality of the corresponding components of the unit vectors which are parallel to these two gradients. Hence equation (8) can be replaced by

$$\frac{(\partial Q_1/\partial\sigma_{ij})_D}{|(\partial Q_1/\partial\sigma_{ij})_D|}=\frac{(\partial F_1/\partial\sigma_{ij})_D}{|(\partial F_1/\partial\sigma_{ij})_D|} \qquad (27)$$

where $|(\partial Q_1/\partial\sigma_{ij})_D|$ is the absolute value, (magnitude) of the gradient vector $(\partial Q_1/\partial\sigma_{ij})_D$. This magnitude is given by the expression:

$$\left|\left(\frac{\partial Q_1}{\partial\sigma_{ij}}\right)_D\right|=\sqrt{\left[\left(\frac{\partial Q_1}{\partial\sigma_{11}}\right)_D^2+\left(\frac{\partial Q_1}{\partial\sigma_{12}}\right)_D^2+\left(\frac{\partial Q_1}{\partial\sigma_{13}}\right)_D^2+\ldots\right]} \qquad (28)$$

A similar interpretation holds for $|(\partial F_1/\partial\sigma_{ij})_D|$.

Obviously, if $(\partial Q_1/\partial\sigma_{ij})_D=(\partial F_1/\partial\sigma_{ij})_D$ as required by equation (8) then equation (27) is satisfied so that our previous analysis is contained in the present more general formulation. The general solution of equation (27) is, however,

$$\begin{aligned}(\partial Q_1/\partial\sigma_{ij})_D&=f(\sigma_{ij})(\partial F_1/\partial\sigma_{ij})_D\\&=[f(\sigma_{ij})(\partial F_1/\partial\sigma_{ij})]_D\end{aligned} \qquad (29)$$

where any arbitrary function $f(\sigma_{ij})$ will satisfy equation (27).

There exists, however, a consistency relation which limits the possible choice of the functions $f(\sigma_{ij})$. In order to derive this relation, write two of the equations (29) explicitly:

$$(\partial Q_1/\partial\sigma_{11})_D=[f(\sigma_{11},\sigma_{12}\ldots)(\partial F_1/\partial\sigma_{11})]_D \qquad (30a)$$

$$(\partial Q_1/\partial\sigma_{12})_D=[f(\sigma_{11},\sigma_{12}\ldots)(\partial F_1/\partial\sigma_{12})]_D \qquad (30b)$$

Taking the derivative of equation (30a) with respect to σ_{12}, and that of (30b) with respect to σ_{11}, subtracting the resulting equations and using the fact that the operations of differentiation and that of extracting the deviatoric part are commutable, the following result is obtained:

$$[(\partial f/\partial\sigma_{12})(\partial F_1/\partial\sigma_{11})]_D = [(\partial f/\partial\sigma_{11})(\partial F_1/\partial\sigma_{12})]_D \tag{31}$$

Repeating the procedure for every pair of indices (i, j) and (k, l), the consistency relation may be written as:

$$[(\partial f/\partial\sigma_{ij})(\partial F_1/\partial\sigma_{kl})]_D = [(\partial f/\partial\sigma_{kl})(\partial F_1/\partial\sigma_{ij})]_D \tag{32}$$

Equation (32) represents the restriction which has to be imposed on the arbitrary function $f(\sigma_{ij})$ in order that the terms $(\partial Q_1/\partial\sigma_{ij})_D$ which are calculated from equation (29) represent the components of a gradient, or, in other words, equation (32) is an integrability condition.

The general solution of equation (32) is

$$f = g(F_1) \tag{33}$$

where $g(F_1)$ can be any arbitrary function. Equation (29) becomes, therefore,

$$(\partial Q_1/\partial\sigma_{ij})_D = g(F_1)(\partial F_1/\partial\sigma_{ij})_D \tag{34}$$

The concept of deviatoric normality requires that the function $g(F_1)$ be continuous, for if $g(F_1)$ contains a discontinuity, then $(\partial Q_1/\partial\sigma_{ij})_D$ has more than one direction at a point where the direction of $(\partial F_1/\partial\sigma_{ij})_D$ is uniquely defined, so that no parallel direction can be defined. A continuous function $g(F_1)$ can always be expressed as a derivative of some other function. Hence equation (34) can be written also as

$$(\partial Q_1/\partial\sigma_{ij})_D = \frac{dG(F_1)}{dF_1}\left(\frac{\partial F_1}{\partial\sigma_{ij}}\right)_D \tag{35}$$

where $G(F_1)$ is some other unknown function which is related to $g(F_1)$ by $g(F_1) = (dG/dF_1)$.

Introducing equations (14b) and (15) into equation (35) yields

$$J_{3D}^2 S_{ij}\left(\frac{\partial Q_1}{\partial J_{2D}} - \frac{dG(F_1)}{dF_1}\frac{\partial F_1}{\partial J_{2D}}\right) + J_{2D}(S_{ik}S_{kj} - \tfrac{1}{3}J_{2D}^2\delta_{ij} \times\left(\frac{\partial Q_1}{\partial J_{3D}} - \frac{dG(F_1)}{dF_1}\frac{\partial F_1}{\partial J_{3D}}\right) = 0 \tag{36}$$

Satisfaction of equation (36) requires that:

$$\frac{\partial Q_1}{\partial J_{2D}} = \frac{dG(F_1)}{dF_1}\frac{\partial F_1}{\partial J_{2D}} \tag{37}$$

$$\frac{\partial Q_1}{\partial J_{3D}} = \frac{dG(F_1)}{dF_1}\frac{\partial F_1}{\partial J_{3D}} \tag{37b}$$

Integrating these equations and following the same procedure as in the paper, (see equations (18)–(21)) the following result is obtained:

$$Q_1(J_1, J_{2D}, J_{3D}) = G[F_1(J_1, J_{2D}, J_{3D})] + h(J_1) \tag{38}$$

The previous solution (equation (22)) is contained in equation (38) for the function $G(F_1) = F_1$, but other forms such as $Q_1 = F_1^2 + h(J_1)$ or $Q_1 = \ln F_1 + h(J_1)$, etc. are equally consistent with the requirement of deviatoric normality. In every case however the plastic potential function Q_1 is defined in terms of the yield function F_1 and an additive term which depends on J_1 only. The existing plasticity models which are based on experimental results seem to support the relation $G(F_1) = F_1$, but the general form is that given by equation (38).

Finally, the consistency relation $\alpha = \lambda$ (equation (9)) is valid only for the case $G(F_1) = F_1$. It is easy to verify that in the general case this relation has to be replaced by

$$\alpha = \lambda\frac{dG(F_1)}{dF_1} \tag{39}$$

The authors are doubtful if the additional generality introduced by the function $G(F_1)$ will prove useful for the purpose of modelling soil behaviour, but we are grateful to Verruijt for pointing out the possibility of generalizing our previous results, and re-emphasize that the derivations presented herein do not represent the general case, but a special class of materials and Q.

R. BAKER
Technion, Haifa, Israel

C. S. DESAI
Department of Civil Engineering and Engineering Mechanics, University of Arizona, Tucson, Arizona 85721, U.S.A.