My Paper was written in a different spirit. I showed that a particular well-defined model led, within the framework of continuum plasticity, to mechanical properties closely resembling those of the 'Cam clay' model in general, and some critical experimental evidence in particular. There remains, as I noted on p. 413 of my Paper, the question of how the organization of clay materials on a microscopic scale corresponds to the features of my model. Perhaps it is worthwhile to comment that the correspondence in question cannot be established by mere microscopy. It is clear that microscopic descriptions of a given clay at high and low voids ratio may be strikingly different, yet the mechanical properties can be similar in the sense that they fit the Cam clay scheme. Again, there is no clear connexion between a visual anisotropy seen in the microscope and mechanical anisotropy discovered from mechanical testing.

The entire problem of explaining the mechanical behaviour of soil in terms of interactions between particles is similar to that of explaining the mechanical properties of metals in terms of the interaction of atoms. Here the present state of knowledge springs from the discovery that metallic atoms pack in regular arrays (crystals) and that irreversible deformation occurs by the passage of line defects (dislocations). The behaviour of dislocations is not thought of in terms of interactions between pairs of atoms, but within the framework of continuum mechanics. In particular, the anisotropy so strongly suggested by the idea of a crystal lattice is hardly relevant to plastic deformation of polycrystals.

Smart also argues that my analysis will somehow be invalidated if clay particles break. As I pointed out on p. 392 of my Paper, my model is conceived on such a scale that inter-particle events enter into consideration only through an averaging process. I would take the point seriously if any evidence could be adduced to show that there are significantly different patterns of behaviour between those clays in which there is widespread breakage of particles, and those in which there is negligible breakage of particles.

Rate of settlement under two- and three-dimensional conditions

DAVIS, E. H. and POULOS, H. G. (1972). *Géotechnique* **22**, No. 1, 95–114.

C. S. Dunn and S. S. Razouki, University of Birmingham

Davis and Poulos concluded that the value of the Henkel (1960) pore water pressure parameter a 'had a completely insignificant effect on the rate of consolidation' of a compressible layer loaded by a circular footing. We disagree with this conclusion. We have recently produced accurate numerical solutions using the A.D.I. method (Peaceman & Rachford, 1955) for a compressible layer loaded by an infinitely long embankment, and found that the rate of consolidation on the centre line was significantly affected by the value of a.

In searching for a reason for this contradiction with the Authors' conclusions, we feel that the Authors' equation (14) should read

$$u_0 = \frac{\theta}{3} + 3a\tau_{oct}$$

if τ_{oct} is defined in the usual form (Scott, 1963) as

$$\tau_{oct} = \tfrac{1}{3}\sqrt{[(\sigma_1-\sigma_2)^2+(\sigma_2-\sigma_3)^2+(\sigma_3-\sigma_1)^2]}$$

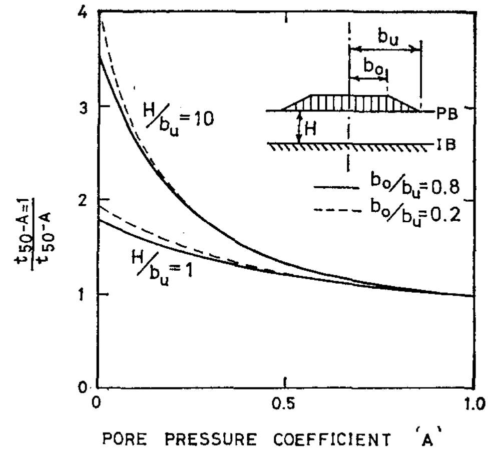

Fig. 1. Effect of pore pressure parameter A on the time to 50% consolidation of a compressible layer loaded instantaneously by an embankment

If the Authors actually used equation (14) as it is printed, then instead of obtaining solutions for values of pore pressure parameter A of 1, $\frac{1}{3}$ and 0, they have actually carried out their calculations for values of 0·57, 0·33 and 0·22. This means that the range of A values studied is relatively small and would largely account for their finding.

Figure 1 illustrates the effect of parameter A on the rate of consolidation in some of the cases we have studied. The ratio of the time to 50% consolidation for $A=1$ to the corresponding time for any value of A is plotted against A. As illustrated by the diagram on the top right hand corner of Fig. 1, solutions were obtained for a compressible layer pervious on its top surface, impervious at its base, loaded by an embankment. Solutions are shown for two ratios of crest to base width ($b_0/b_u=0{\cdot}2$, 0·8) and two ratios of layer thickness to half base width ($H/b_u=1$, 10).

The rate of consolidation is particularly sensitive to changes of A at the lower values typical of overconsolidated clays. For normally consolidated or lightly consolidated deposits, the value of A does not indeed affect the rate of consolidation very much and for practical problems, it would be safe and not unduly conservative to assume that $A=1$ in such soils. Consolidation graphs for such problems are now available and will be published shortly.

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Authors' reply to C. S. Dunn and S. S. Razouki

The apparently negligible effects of the pore pressure parameter a on the rate of settlement, reported in the original paper, do not arise from a mistake in equation (14) as suggested. The apparent error in fact arises because of our unconventional definition of τ_{oct}

$$\tau_{oct} = \sqrt{(\sigma_1-\sigma_2)^2+(\sigma_2-\sigma_3)^2+(\sigma_3-\sigma_1)^2}$$

The conclusions reached in the original paper are unfortunately based on solutions to only one geometrical problem (a circular footing with $h/a=1$, for the boundary conditions IFPB).

For such a shallow layer, the solutions of Dunn and Razouki also indicate that the effects of a are not serious, but that these effects become more significant as the relative layer depth increases. There are a number of differences between our solution and those presented by Dunn and Razouki.

They consider a plane-strain problem rather than an axially-symmetric problem. The boundary conditions are different (PTIB for the strip solutions, IFPB for the circle solutions). The method of computing the stress distribution may have been different. We have used that for a rough rigid base; Dunn and Razouki do not specify which they used. In addition, their method of analysis is not specified, nor is their value of Poisson's ratio ν' (although this would be irrelevant for a simple diffusion solution except in the computation of the stress distribution).

We concede that our statement regarding the insignificance of pore pressure parameter a may have been based on too little evidence. However, the statement of Dunn and Razouki of the effect of a in terms of the time required for 50% consolidation is perhaps too severe and a more useful practical expression would be in terms of the difference in the degree of consolidation at a particular time. The effect is then much less significant than is suggested by Fig. 1 of Dunn and Razouki. Finally, it would appear to be more logical to express the results in terms of the departures from the ideal elastic case ($A=\frac{1}{3}$) rather than the case $A=1$.

The consolidation of soils exhibiting creep under constant effective stress

GARLANGER, J. E. (1972). *Géotechnique* **22**, No. 1, 71–78.
Discussion by Šuklje (1972). *Géotechnique* **22**, No. 4, 670–673.
Author's reply to discussion by Šuklje (1972). *Géotechnique* **22**, No. 4, 673–674.

L. Šuklje, University of Ljubljana, Yugoslavia

The Author's comments to my discussion require the following points to be made. The analytical expression for the set of time-lines according to the suggestions by Taylor (1942) and Bent Hansen's (1969) specification, is my equation (10). The corresponding rate of void ratio change $\dot{e}$ has been given by my equation (12). The main question I raised was whether or not in equation (12) the second member, which expresses the dependence of $\dot{e}$ on the rate of effective stress increase, should be taken into account. If not, equation (12) reduces to the equation of isotaches (16).

Whether the speed of effective stress change $\dot{\sigma}'$ influences the rheological relationships of a viscous body depends on the viscous properties of the body. The deformability of viscous bodies whose rheological models are composed of elements connected in parallel is not governed by the speed of effective stress increase which, however, does influence the stress–strain–time behaviour of bodies whose rheological elements are connected in series. When utilizing, for consolidation analysis of thick layers, isotache sets deduced from the secondary branches of settlement–time curves of saturated samples, the eventual influence of the speed $\dot{\sigma}'$ on the consolidation speed $\dot{e}$ is taken into account where $\dot{\sigma}'$ values do not surpass those which appear during the secondary consolidation of saturated samples. As the time-lines presented in Garlanger's Fig. 1 have also been obtained from observations during the secondary consolidation at small $\dot{\sigma}'$ values, they do not give any experimental support either for considering or for neglecting the second term in my equation (12) when the speeds $\dot{\sigma}'$ have greater values.