A thermomechanical analysis of a family of soil models


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When Collins and Kelly say that Drucker’s postulate is a ‘classification of a type of material behaviour that the original Cam clay model may satisfy’, they are close to a sentence in Schofield & Wroth (1968) that ‘the index properties are linked with the critical states of fully disordered soil and ... the critical state strengths form a proper basis of the stability of works currently designed by practising engineers.’ Our book linked the critical state plastic compressibility, $\lambda$, with Casagrande’s plasticity index (once regarded as purely empirical). Soil classification helps engineers to select reliable materials and methods for construction. The purpose in developing and teaching the original Cam clay model was to give students reliable soil parameters for plastic analysis. Plasticity, a Greek word for moulding of soft saturated clay, also applies to ductile mild steel. Both soil and steel are in a class of material that can form a structure that is ductile and stable by Drucker’s postulate. Structural and geotechnical engineering students and potential owners of constructed works should all learn that associated plastic flow and ductility are ‘safe’ and brittleness is ‘dangerous’.

In his book Rankine (1862) wrote:

‘Earthwork gives way by the slipping or sliding of its parts on each other; and its stability arises from resistance to the tendency to slip. In solid rock, that resistance arises from the elastic stress of the material, when subjected to a shearing force; but in a mass of earth, as commonly understood, it arises partly from the friction between grains, and partly from their mutual adhesion; which latter force is considerable in some kinds of earth, such as clay, especially when moist. But the adhesion of earth is gradually destroyed by the action of the weather, and especially by alternate frost and thaw; so that its friction is the only force that can be relied upon to produce permanent stability.

The frictional soil that students should select for construction has a significant fraction of clean grains that ‘if left by itself’ in Coulomb’s words) can form a heap with an angle of repose, exhibiting internal friction. If such an aggregate of cohesionless grains is not over-compacted, power dissipation in it is proportional to the plastic shear distortion and the mean effective pressure, $\frac{dW}{d\varepsilon} = Mp$. That function did not arise as ‘the simplest model of dry friction’. It was the (unexpected) outcome of PhD research by an able student, Thurairajah. Cam clay is granular material on the ‘wet’ side of critical states, and is ductile and safe. Over-compacted Teton Dam core material was far on the ‘dry’ side, brittle and dangerous (Muhunthan & Schofield, 2000).

Original Cam clay may violate Collins and Kelly’s expectation of isotropy without violating the laws of thermodynamics. At $q = 0$, the vertex of the original Cam clay model, plastic flow vectors associate plastic volume changes with random shear distortions, and dissipate energy in spherical compression of aggregates of soil grains by causing inhomogeneous internal strain. Collins and Kelly have joined the ranks of many later researchers who, unsatisfied but undaunted by this aspect of original Cam clay, then go on to produce their own modifications. Our integration of the equations of dissipation and associated flow gave an original Cam clay model with a vertex on the pressure axis that was a surprise. I conceived the idea of a vertex in 1962 and, when we published in 1963, Roscoe, Thurairajah and I supported the idea by experimental evidence. But by the time that I returned to Cambridge from sabbatical leave at Caltech in 1964 Roscoe wanted to round off the vertex. When adhesion among grains was so casually introduced to round off the vertex, another valuable ‘point’ of original Cam clay was lost. Original Cam clay explained the behaviour to be expected in an aggregate of soil grains that is stable by Drucker’s postulate. It included the possibility that plastic compression of soft soil, such as Casagrande’s Boston blue clay, is yielding of a cohesionless aggregate in states wetter-than-critical. When Casagrande (1936) began his paper on liquefaction by stating ‘The Meaning of the Term Stability’ as if contractive soil was necessarily unstable from energy considerations, he failed to add together the power inputs from contraction and from shearing to give the power that is dissipated in stable plastic distortion, not in its unstable liquefaction.

A student of soil mechanics needs to learn two very different models for the behaviour of soil. The first is Rankine’s model of a material with many planes of limiting stress. The second is a model of a plastic continuum that dissipates work during plastic strain. Drucker et al. (1957) published a model with a yield surface, closed with a hemisphere on the axis. After the paper by Roscoe et al. (1963) was published, I went to Brown University to ask Professor Drucker about the vertex. He said he liked the model, and advised me to leave the vertex as it was, saying: ‘A corner on a yield locus is useful in getting solutions to problems’ (when Calladine read a first draft of this letter he commented that Tresca gives an example of a yield locus with useful corners). My confidence in geotechnical centrifuge model tests, both in Manchester and in Cambridge, came from this understanding of mechanics of granular material, and I was able to study liquefaction in centrifuge model testing for industry. When I have discussed my ideas in critical state soil mechanics teaching and in my Rankine and other lectures I have found it satisfactory to think of saturated reconstituted soil in terms of the associated flow rule of plasticity theory, the critical state concept and the ‘pointed’ original Cam clay model. The rounded-off modified models that have been proposed were no help to me, but I am glad that publication in Géotechnique and discussion on the point continues.

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The authors have presented a very elegant method of generating a host of elasto-plastic models for soils from a thermomechanical approach. The central point of the approach is that the free energy function can be taken to be a function of plastic strains in addition to the usual elastic strains, and that the plastic strains are work conjugates of dissipative stresses and not the true stresses. Two special modelling
features have been introduced, the $\alpha$ and $\beta$ models, which are very appealing. The $\alpha$ model, with its frictional dissipative function, modifies the Cam clay elliptic yield curve and allows a non-associative flow rule. This makes it possible to model important features of soil behaviour such as static liquefaction and instability. The $\beta$ model, with a hardening associated with shear in addition to volumetric hardening, is capable of modelling phase transformation and undrained stress path reversal.

The discusser wishes to seek clarifications on some issues that might arise in applying these models, though the objective of the present paper was only to present the methodology of constructing different models.

As stated by the authors, the usefulness of these models finally lies in their ability to model experimental results. The authors also mention the difficulties in applying these models, as of now, to sandy soils owing to the problem of not having a defined isotropic compression path. Thus at present the models can be tried only for clayey soils. But both these features—static liquefaction and instability behaviour requiring $\alpha$-type yield curves, and stress path reversal requiring $\beta$-type hardening—are essentially those of sand behaviour, and are never observed with clayey soils. It is true that the Cam clay elliptical yield curve is not totally satisfactory for clays, and a slight $\alpha$ modification ($\alpha$ not less than 0-5) may improve the predictions over some stress ranges. For example, an elliptical yield curve is known to over-predict the $K_0$ value for a soil and also the extent of stress ratios that can be reached on the dry side of critical. Rough calculations show that an $\alpha = 0.5$ yield curve may predict a lower value of $K_0$, or it may even slightly under-predict. Instead, if this $\alpha = 0.5$ yield curve is associated with an associative flow rule (rather than the non-associative rule) the predictions might be better. On the whole, what is needed is to get improved predictions over the Cam clay model may be a slightly different shape of the yield curve obtained by some modification of the dissipative function: that is, the dissipative function still being density dependent and not frictional involving explicitly the true normal stress.

The $\alpha$, $\beta$ model features are, however, very well suited for sands. Hopefully the difficulties mentioned can be sorted out soon and the models can be put to real use. There are some doubts as to how these $\alpha$ and $\beta$ features are proposed to be used for a given soil. This being a single-surface model, is the yield curve something like a bounding surface corresponding to an equivalent virgin compression state, or does it correspond to the actual maximum past pressure? Is the intention to vary the $\alpha$ and $\beta$ parameters for a given soil, even for a given initial density, depending on the confining pressure? For example, a given soil would show a static liquefaction type of behaviour in a very loose state and/or at very high confining pressures. For such a condition, a value of $\alpha = 0$ (or at least a very low value) and $\beta = 0$ would be required, whereas for a denser state and/or at lower confining pressure a stress path reversal type of undrained behaviour may be seen. Also, higher failure stress ratios may be reached in drained shearing. This would require a higher value of $\alpha$, and also of $\beta$.

There is not much play available with the $\beta$ parameter: the maximum could be 0.25, as higher values than this would move the failure line unrealistically high. Secondly, for whatever value of $\beta$, the extent of the dilating part of the stress path beyond phase transition appears to be limited and quite small, whereas actual experimental results would show a much higher range of this part (see Verdugo & Ishihara, 1996, for typical data). This would suggest that a major part of the stress path reversal would have to be accounted for not by the $\beta$ hardening but rather by 'overconsolidated states' or soil states within the yield curve possibly exhibiting a plastic contractancy initially and then following the undrained path of a denser state to reach the critical state. The possible plastic behaviour of soil states within the yield curve is an aspect that is not discussed in the present paper, this being a classical single-surface model. But this will be an important issue of consideration for behaviour under cyclic loading. It appears that the stress path reversal can alternatively be explained with an initial contractancy (which may be considered to be shear softening) within the yield curve and then the usual critical state-bound progress of the stress path beyond yield, in which case the $\beta$ hardening may not be required. This might be a better explanation than $\beta$ hardening, because this would simultaneously explain the phenomenon of liquefaction under cyclic loading due to cumulative contractancy over each cycle. Micro-mechanically, there is an indication or justification for such initial softening, as observed by discrete element studies of granular assemblies that there is a loss in the overall number of contacts, at constant density, whenever there is a change in stress ratio (Vatsala, 2001).

Finally, in all these $\alpha$-yield curves, the ratio of isotropic stress to critical state stress is the same as that of the elliptical yield curve—that is, 2.0. But experimental results for static liquefaction of several sands show much higher ratios of up to 4 or even more (see Verdugo & Ishihara, 1996, for typical data). Hence there is some need to modify the shape of the yield curve. What form of dissipative functions can be tried to get unequal limbs of yield curve on either side of the critical state? Is it necessary to have the yield stresses in dissipative stress space the same both for compression and for tension or expansion?

Authors' reply

Schofield

In his discussion, Professor Schofield makes a number of comments on the ideas that led him and some of his Cambridge colleagues to propose the model, now known as original Cam clay. We are not competent to comment upon these, but would like to attempt to clarify some of the reasons for our criticisms of this model, which relate to Professor Schofield's other remarks.

Our first point is that this model cannot model irreversible, isotropic compression, as the proposed dissipation function in equation (21) involves only plastic shear strain increments, so that it predicts that no energy is dissipated in isotropic compression, which is in violation of the second law. The yield locus for original Cam clay has a vertex on the $p$’ axis, so that the normal flow rule predicts that the direction of the plastic strain increment vector lies somewhere within the wedge of outward normals at this vertex. The exact direction is not uniquely determined by the constitutive law alone, but is determined in any given boundary value problem, once the boundary conditions are specified. If one wants to model the response of an isotropic material to the application of an isotropic stress, then the response must be an isotropic compression, with no distortional plastic strains. In this case, the plastic strain vector in the original Cam clay model will point along the $p$’ axis, the plastic shear strain increment $d\varepsilon$ will be zero, and the second law is violated. If such shear strains are observed in a test, the specimen has a preferred direction and hence must be modelled as an anisotropic material. Original Cam clay is, of course, an isotropic model, as it is expressed in terms of stress invariants. It would need to be modified to include anisotropic responses to model such observations. This point will be returned to below.

It is to be emphasised that it is not the existence of the vertex in original Cam clay that causes our difficulty; it is the lack of any plastic volume strain term in the dissipation func-
tion that causes the problem. We agree with Professor Schofield that yield loci with corners are quite acceptable, and are useful in many areas of application of plasticity theory. As explained in the paper, and in its sequel by Collins & Hilder (2002), the theory of plasticity in vogue at the time when the Cambridge models were being developed has now been superseded. The two major new developments have been the construction of a very general thermomechanical theory, which enables the constitutive behaviour of a great many materials to be formulated systematically starting with the laws of thermodynamics. The second development has been the realisation that the macro-level, continuum behaviour of elastic/plastic materials depends crucially on the energy storage and dissipative mechanisms at the micro level. An important example is the existence of stored or frozen plastic work in the continuum model, which results from some of the elastic energy at the micro level not being recovered upon unloading. The presence of such residual elastic strains is a direct consequence of the fact that the plastic strain is not a true kinematically compatible strain in that it cannot be derived from a continuous displacement field. Hence, upon unloading, the residual elastic strain must be added to the plastic strain to give the total residual strain, which is a proper strain. Thus we agree with Professor Schofield when he states that ‘spherical compression of soil aggregates causes inhomogeneous strains’, and these must be allowed for in any averaging process. However, original Cam clay has zero stored work. The reason for this will become apparent below. An account of plasticity theory based upon these ideas has recently been given by Ulm & Coussy (2003), who discuss modified Cam clay as an example. In this view the development of these ideas requires a complete re-examination of much current thinking on soil mechanics.

The most compelling arguments for adopting the original Cam clay dissipation function (equation (21)) are the results of Thurairajah’s experiments with kaolin and sand. According to Schofield (2000):

Thurairajah (1961) found that in a range of drained and undrained tri-axial tests, with and without volume change, the dissipation of work during increments of distortion was everywhere the same as at the critical state. That is, it was given by equation (21). Accordingly, Collins & Muhunthan (2003) have recently analysed this problem within the thermomechanical framework. As a specimen is compacted, the elastic compressive strains increase, and hence the residual strains, residual stresses and stored plastic work increase too. If the specimen is then unloaded elastically and loaded in the dry (dense) regime, it undergoes plastic dilation and the stored plastic work is reduced. At some point the stored work becomes zero. Using the anisotropic model used by Muhunthan et al. (1996) and Collins & Hilder (2002), it was shown that, when this happens, the dissipation due to isotropic compaction is also zero, so that the dissipation function is indeed given by equation (21). Only the plastic shear strain increment contributes to the dissipation. The accompanying dilational, plastic volume strain increments reduce the stored plastic work, but these changes are exactly balanced by the positive stored plastic work increments due to distortion, so that the total stored plastic work remains zero. The material has ceased to harden and is behaving as a perfectly plastic material. Moreover it was found that, under these conditions, the dilation, mobilised friction and critical state friction angles are related by Taylor’s classical relationship. This analysis hence reinstates the original Cam clay dissipation function, but in the context of anisotropic models, and in situations of high stress ratios, away from the $p’$ axis.

We are grateful to Professor Schofield for his interest in our paper, and for giving us the opportunity to attempt to clarify some of the concepts used in our analysis.

Vatsala

Dr Vatsala raises a number of issues related to the interpretation and use of the parameters in the new family of models proposed in our paper. This paper was our first attempt to construct a family of single-surface models, based on recently developed ideas in thermomechanics, which incorporated the classical Cam clay models as special cases, but which included other observed effects, such as non-associated flow rules and ‘pear drop’ shaped yield surfaces. Such generalisations, it was hoped, would make the models more flexible and able to model a wide range of soil types, including sands. It soon became apparent, however, that the proposed class of models needed to be appreciably broadened if we were to achieve this goal. An extended family of such models is described in the sequel paper by Collins & Hilder (2002). The generalisations achieved in these models reflect many of the points made by Dr Vatsala.

The first problem addressed was the last point in Dr Vatsala’s letter. The spacing ratio, $r$, of all the models in the paper under discussion was 2 as in modified Cam clay. (The spacing ratio is the ratio of the consolidation pressure to the critical state pressure.) However, this ratio is closer to 4 or 5 for sands. This generalisation was achieved by introducing a further parameter, $\gamma$, and modifying the pressure term multiplying the volumetric strain increment in the dissipation function in the same way as had been done for the shear strain increment in the first paper. Accordingly the dissipation increment is now written:

$$\dot{\Phi} = \sqrt{[(1 - \gamma)\sigma + \frac{\gamma}{2}p_c^2]\dot{\varepsilon}_v^2 + M^2[(1 - \alpha)\sigma + \frac{\gamma}{2}p_c^2]\dot{\varepsilon}_p^2}$$

(48)

It was shown that $\gamma = 2/r$, and is a measure of the proportion of plastic work that is stored during isotropic compression, and also that the resulting yield curves have unequal limbs on the two sides of the critical state line. The original models are regained when $\gamma = 1$. If these parameters are regarded as constants, the resulting family of yield curves will be geometrically self-similar. There would appear to be some experimental evidence for this, at least at high stress levels (Lade & Kim, 1988). However, the significance and evolution of these parameters can only be determined from a conceptual micro-mechanical model. It can be shown, using a much simplified such model, that the $\alpha$ parameter can be interpreted as the volume fraction of the particles, which are in the load-bearing force chains. This fraction is likely to change and decrease at lower stress levels, where liquefaction occurs, as surmised by Dr Vatsala. A criticism often made of the thermomechanical approach is that it starts by postulating a stored plastic work and a dissipation function, neither of which can be measured in the laboratory. It is hence necessary to deduce the values of the model parameters by using curve-fitting techniques applied to observed yield curves, flow rules and stress paths. Our defence of our approach, however, is that the underlying physics of the problem is incorporated at the outset in these two functions. Further analysis of the conceptual, micro-mechanical models is needed, however, to improve our physical understanding of the various possible forms of these two functions.

The second extension made in the sequel paper stemmed from the realisation that, as soon as shear hardening is included, the plastic part of the free energy will also depend...
on the plastic shear strain, so that the shift stress will also have a shear component. This means that the centre of the yield locus is no longer on the pressure axis, and that the yield curves rotate in the \((p, q)\) plane. In other words anisotropy will be induced. This is in accord with the findings of many researchers, and leads to a much more realistic model for sands and materials with a pronounced granular structure. A first attempt at constructing such models, using the thermomechanical approach, was given in Collins & Hilder (2002), and further development is underway. These models cope with the problems of not having a well-defined isotropic compression path, and giving an extended range of the stress paths beyond the phase transition line—two points that were raised by Dr Vatsala. We hence no longer subscribe to the \(\beta\) model, which was developed only in an isotropic context.

Finally we note Dr Vatsala’s comments on cyclic loading and multiple surface models. Such models would be produced if we could identify two or more independent micro-mechanical dissipation mechanisms. This would be a promising avenue of research.

We are grateful to Dr Vatsala for his interest in our paper, and for making a number of insightful comments.

No mention of equation (48) in text.

REFERENCES


