Key questions in rock mechanics

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ABSTRACT: This brief address repeats and reinforces what others have said in the past. Rock Mechanics is a mature and well developed branch of learning and engineering practice. It rests upon secure theoretical foundations, but presents unusual problems in continuum mechanics because of scale effects, particularly those relevant to yield and failure. The special characteristics making applications of theory difficult are the extreme problems of inhomogeneity and uncertainty in material properties and pre-existing stress states, which exhibit themselves at all scales. Interpretation of both experiments and field observations is thus a major problem. These special features lend a special significance to finite element methods. It is suggested that sedimentary rocks (and especially shales), with properties spanning those of soils and hard rocks, are worthy of more intense study, and that the circular cylindrical borehole is a geometry worthy of special attention.

1 INTRODUCTION

I have almost no qualifications for giving this Keynote Address, and so approached the task of preparing it with some trepidation. I have not worked formally in the subject, and only became involved in it a few years ago, when I undertook to run a mathematical modelling department covering the full range of drilling and pumping problems in oilfield services. I found those problems involving rock mechanics to be both the most important and the most fascinating, and so I was (rather too) easily persuaded to make these preliminary comments.

My first hope was that that I would be able, as an outsider who had worked in other branches of mechanics, to find areas in your field of work to which ideas and results that had proved useful in others could be applied. However, on almost every occasion that I thought I had something novel to contribute, a little research into the literature proved that others had already exploited the possibilities that suggested themselves to me. As a result, I have spent a significant part of the last few months studying the literature already well known to most of you to bring myself to the point at which my ignorance of the whole subject would not appear too glaring. (A text that I found extremely illuminating was the record of the proceedings of the William Prager Symposium held at Northwestern University in 1983; I shall refer to many of its chapters in what
follows under the name of the authors concerned. Where I make reference to other contributors to this symposium, I shall do so by name alone, without date and without an entry under the reference section.)

My next hope was that I would be able to provide a new and challenging set of Key Questions in Rock Mechanics for us to consider this week. Once again I found that many of you had anticipated in earlier well argued papers all that I could find to say myself. My talk will therefore be on Key Questions revisited. At best, I shall be able to give a different slant to some of the issues involved, and to draw attention to experiences in other branches of mechanics and applied mathematics that may usefully influence the way you tackle outstanding problems. My present, modest aim is to reinforce some of the words of wisdom that your greatest practitioners have already given you, so that you will have them before you at the start of this Symposium.

In what follows, I find it convenient to divide my comments between

- theory - essentially constitutive relations for rock behaviour (as a function of the history of total stress, pore pressure and chemical assault)
- applications - in geomechanics, i.e. analysis and prediction of deformation and failure in natural and man-made rock structures, leading to design methods to avoid unacceptable or catastrophic behaviour during and after engineering operations
- experiment and observation - under laboratory and field conditions, aimed at parametrization and characterisation of particular rocks and rock systems
- finite element modelling - being more than a form of numerical analysis

This does not quite parallel the symposium structure, though interconnections and iterative procedures involving all aspects make any grouping of topics rather arbitrary. What is abundantly clear is that the basic sequence of theory, experiment and application (itself divided into design, execution and evaluation) is common to all engineering disciplines.

What makes rocks different from other engineering or natural materials is that they are simultaneously and importantly
- inhomogeneous over a very wide range of length scales - like some other natural products but unlike the seas or the atmosphere
- non-isotropic - and therefore should always be represented in a fully 3-dimensional (tensor) manner, like wood but not air or water
- porous and permeable - usually saturated with a mobile fluid
- multiphase - in terms of both solid and liquid components

In practice they are subject to strong thermal, chemical and non-linear mechanical effects. In most situations of difficulty, they suffer irreversible changes difficult to observe in detail and almost impossible to characterise with any generality.

Widely accepted theories relate primarily to strong (hard and often largely monolithic) rocks and to weak soils (with two essentially incompressible phases); sedimentary rocks like shales which exhibit all of the complexity described above have been poorly investigated by comparison, and so those involved with drilling oil wells find little in the literature to help them. This is an important issue because many of the difficulties associated with drilling, tunnelling or mining arise in such sedimentary rocks, some 50% of which are classified as shales.
Formal continuum mechanical descriptions (see e.g. Chen 1984) have been developed for a wide range of rock and soil-like behaviour. These are based on the now widely accepted averaging (or homogenizing) techniques inseparable from the development of theories of deformation and flow of continuous media. Though the preferred models vary as different materials are studied (metals, glasses, amorphous polymers, gels) the principles are common and well understood. No fundamental problems remain. A wider knowledge of what is already in the literature is a far greater need today - in all branches of material science - than a further development of formal methods of representation of material behaviour.

These macroscopic theories usually refer to behaviour under affine (uniform) deformation and stress; irreversible or hysteretic behaviour is covered. Materials are treated in almost all models as rheologically simple, i.e. they respond only to the point values (or histories) of strain, strain rate and vorticity. Theories can also include dependence on space derivatives of these quantities, but there are few examples where this idea has been convincingly used in real applications. Justification for the assumption of rheological simplicity rests upon the supposition that the length scale of fundamental material structure is orders of magnitude smaller than that associated with the gradients of continuum stress (or strain) or of gross material inhomogeneities. This allows the averaging process to be carried out on a length scale intermediate between the two. Some of the difficulties that are experienced in rock mechanics may be traced to the inadequacy of this supposition.

Observations on yield and failure provide an example. The occurrence of large cracks or failure planes introduces a coincidence between the length scales of structure and stress; not surprisingly, because they are intimately related to one another. The (unnecessarily schismatic) subject of fracture mechanics attempts to avoid the difficulty by distinguishing firmly between the averaging scale (which is effectively zero) and the crack scale (based on its length). Even so, difficulties ensue at the crack tip singularity, as Barenblatt was the first to emphasize for polymeric materials (which have disconcertingly large structural scales). Some treatments of the 'process zone' are less than soundly based as a result. There is nothing new in this observation (Rice 1985; Ingraffea 1985). The problem intensifies when yield and failure are attributed to the extension of pre-existing microcracks. The question then arises: is this irreversible change in state of the material to be wholly subsumed in a changing constitutive relation (which is for example the basis of critical state soil mechanics, in the sense that the unloading 'line' changes with the current point reached on the consolidation 'line') or is it to be modelled in detail?

In practice, the decision is determined by the scale and nature of the experiment performed as much as by the material. In crude terms, it may be argued that brittle behaviour is associated with a rapid increase in the length scale of yield, implying catastrophic local failure under constant load, while ductile behaviour is associated with a limitation or reduction of this length scale and an absence of strong localisation of yield. (The notions of brittleness and ductility are now so firmly established in material science that it is pointless to object to their crudeness.)
A formal theory to cover scale effects in yield and failure, if possible within a continuum formulation, is now an outstanding need. The theory of plasticity is the simplest continuum formulation: it should be remembered that it may cover small scale effects like shear-banding that at a different continuum scale (e.g. optical microscopy) would be explicitly displayed. At the other extreme, theories of jointed or fractured rocks preserve the discrete nature of many yield and failure mechanisms (Goodman 1976, 1987). A fully satisfactory and comprehensive approach may well have to use the notion of a 2-dimensional subspace - itself characterised by continuum properties (Kulatilake & Wu 1984; Ruina 1985; Pariseau) and representing internal discontinuities - embedded in a 3-dimensional body continuum. This issue is closely connected with the interrelation between boundary or initial conditions and constitutive relations. It will be discussed further under finite element modelling.

When we come to study shales, we find that they exhibit a very rich microscopic structure, particularly in terms of the nature of the fluid-filled porosity. Microscopic examination of highly deformed samples suggests that an explicit attempt to model these very small-scale effects would help in the choice of suitable continuum models. This "synthetic" or "microstructural" approach has been found valuable in polymer science and multiphase fluid mechanics (for suspensions, emulsions and foams). Experience shows that fully 3-dimensional models are needed; simple extension of 2-dimensional models or experiments often fails to illustrate the full range of material behaviour, or even correctly represent standard behaviour.

The traditional method for dealing with instabilities in mechanics uses linearised spectral methods within a wholly continuous formulation. Examples abound in fluid mechanics and in the theory of structures. Extension (via bifurcation theory) to the fully non-linear regime is now common and indeed has been applied to failure testing of rock samples. Although such complete solution schemes are difficult, they provide unique insights into the respective roles of boundary conditions, initial conditions and constitutive relations, via a series of 'benchmark' problems. They also point to difficulties ahead, in the sense that real phenomena often go beyond simple bifurcation behaviour, and are indicative of chaos and turbulence. Despite the best efforts of excellent workers, the problems of even simple inertial (Newtonian fluid) turbulence are unresolved: engineering calculations are still based on simple empirical mathematical relations for mass, momentum and energy transfer. In simple terms, a turbulent flow of any fluid is treated as the flow of a different, non-Newtonian fluid whose properties are governed by the entire flow field. There is no accepted methodology for dealing with such situations; practice is at least as unsatisfying as in engineering rock mechanics.

Stability theories raise the interesting question of time scales. Non-linearities in unstable systems are the source, not only of progressively more complex spatial departures from the symmetry of affine deformations or flows, but also of a potentially very wide range of time scales. These may be intrinsic to the material or dependent on loading sequences and sample length scales. For example, seismicity covers a range from microseismic events (with very short time scale) that are averaged out in all but the most specialised experiments to infrequent tectonic disturbances that are usually neglected in most engineering calculations (but important in the case of nuclear waste disposal). A more homely example is
provided by the number of decades in log(time) needed to plot a creep curve.
Less obvious examples are provided by permeability (e.g. Biot models) and
chemical effects (Lajtai & Bielus 1986).

Although this section started by saying that there are no fundamental problems
in a continuum description of materials, there are many tactical decisions to make
when treating applications. Is it better to use elaborate history functionals relating
stress and strain, or is it better to introduce a sufficiency of additional variables
(e.g. for damage - Erlacher 1985) to work wholly with first-order (in time)
evolutionary equations? Should stress or strain/strain rate be regarded as more
fundamental in describing the current state of a rock? The latter issue is by no
means trivial when the state reached is path dependent.

3 APPLICATIONS

At a general level, the problems of engineering rock mechanics are those of other
engineering disciplines. Indeed, its practice is almost too like that of the others.
Academic teaching may well overemphasise deterministic methods of analysis
and design, to the point at which young workers are unprepared for the lack of
data that they will actually be faced with in the field. It is not too much to say
(Bieniawski 1984) that

uncertainties about the pre-existing state of a rock mass are the fundamental
problem in rock engineering.

This includes knowledge of the rock properties, the current state of (earth)
stress and pore pressure and the incidence of tectonic changes. In mathematical
terms, this means that the model boundary and initial value problems to be
solved are rarely well posed. The challenge is therefore

whether a combination of sensitivity analysis and statistical investigation of
(computer) solutions, combined with extensive data banks, can ever replace the
combination of empiricism, judgement and overcautiousness that is currently
relied upon today?

In any particular application, success is seldom achieved unless all stages of
systematic method of approach are followed. Though experienced workers may
think formalisation of working practices into a methodology rather trite, it is worth
restating the essential steps (see also Starfield & Cundall 1987, and chapters 2, 4
and 6 of Polkinghorne 1986).
A What are we trying to predict or control? Why? What answers do we want?
Above all, how shall we use them? This is a largely conceptual activity; it should
provide 'user specifications' for the applications scientist, so that ensuing
experimental and theoretical studies have a clear objective. Cornet's contribution
to this symposium could be taken as a model to be followed. (In my experience,
far too few of the PhD theses presented in engineering departments solve real
engineering problems: many students seem satisfied if they can merely "make a
contribution" leaving others to carry out the real application; unfortunately when
these supposedly highly trained students pursue their careers in industry, they
often continue to "make contributions" rather than solving problems.)
B Given the answers to to the questions in A, how do we describe the system to
be considered? What mathematical models do we choose for the whole and the
parts of the system? Are they complete and mutually consistent? Are they quantifiable? Could they be validated against observations? Could they be analysed (solved) to give the answers needed according to A above? This is also a largely conceptual activity. To the extent that we seek rational design methods and must calculate, this stage is vital: inadequacies or errors at this point can vitiate lengthy and expensive work done subsequently. A typical example is provided by design techniques that assume the response of a system will have the symmetries of the imposed loads and displacements: buckling of beams in compression is neglected, the Tacoma bridge was destroyed by unexpected wind-driven oscillations, early thermonuclear fusion reactors failed to work because of unexpected instabilities in the magnetic field. On the other hand practicality and the need for generally applicable methods requires maximal simplicity and idealisation: this is the stage at which the critical effects must be modelled; marginal complications can be eschewed.

C Given adequate models, how do we calculate the necessary desired answers? What degree of mathematical approximation is acceptable? How do we test the answers for accuracy and consistency? This is the realm of the applied mathematician; I think it is unreasonable to suppose that all engineers should do their own calculating, and overhopeful to suppose that standard computer software packages will usurp the role of the specialist numerical analyst.

D The last stage of the process has now been reached. All those involved so far have to ask themselves: Have we answered the questions we originally posed? Have we validated these answers? If not, what changes in the choices we made in A, B or C above should we now make? This implies that the process becomes iterative, and that an element of team work is necessarily involved. It also implies that a significant amount of field testing should be undertaken after every project is implemented; we should not wait until we suffer a disastrous failure before assessing the reliability of our design methods.

The general principles (methodology) described above can be applied at several levels, either independently or as part of a larger whole. A useful classification suggests itself.

(1) Preliminaries - experiments designed, carried out and interpreted to provide fundamental parameter values (in constitutive experiments); these incidentally often provide evidence of new unexpected phenomena, even though we have sought a maximum degree of control in the situation chosen. This, basic material science, attracts its fair share of outstanding talent doing admirable work.

(2) Learning exercises - armed with the results of (1), idealised and highly simplified field problems are tackled theoretically and in the laboratory to justify the use of certain models, calculation methods and experimental arrangements. These are the preferred form of activity (an adolescent phase) for graduate students; there is a danger that many of these exercises will prove sterile, and that endless improvements on adequate analyses become ends in themselves to international coteries.

(3) Natural phenomena - observation and explanation of natural processes is the heart of geoscience. The scientific worker has no control over his subject matter, and has to be prepared to revise his ideas constantly. It is hardly surprising that this exciting (adult) activity attracts many of the great workers in geomechanics.
We can only hope that advances in the subject will allow us to predict the present state of the earth's crust with more confidence than at present.

(4) Engineering - construction, destruction and removal of rock masses. The development of rational design techniques is an outstanding need. The primary aim is to identify potential failure modes and prevent them, subject of course to economic constraints. As many of you will know, this apparently simple need leads to extremely complex problems, often far beyond our ability to solve completely. Real systems require simultaneous consideration of 'post-peak' deformation, time effects, thermal effects, pore-fluid flow effects, all acting on ill-characterised rocks under uncertain stress conditions. Even if adequate mathematical models could be devised to encompass all these effects and uncertainties, it is doubtful whether existing computer facilities and expert manpower would be sufficient to solve them satisfactorily on a routine basis.

Sadly (as with other major engineering problems) these difficulties are usually underestimated by those who call for and dispose of R&D funds; there is little incentive for the very best talent to tackle these awesome problems, even though there is agreement on their importance. A MANHATTAN project can only be sustained under conditions of national emergency. Pure science can avoid the needlessly short-term and chauvinistic commercial imperatives that beset much of applied science, and thus attract many of the best potential engineers.

A modest example of a currently important problem of interest to a very large number of engineers (Guenot & Santorelli, Hueckel & Peano, Haimson) is provided by the (circular cylindrical) well-bore stability problem. This could usefully become a benchmark problem at the interface between levels 2 and 4. It incorporates all the uncertainty surrounding rock properties, earth stresses and pore pressure; it covers many modes of collapse; it stretches our modelling and computational skills to the limit. Furthermore, it has the advantage of wide applicability in the oil and mining industries. It deserves a substantial measure of international collaboration, if only to help avoid the loss of life that is still the price paid in our industries.

4 EXPERIMENTS AND OBSERVATIONS

Experiments are essential and the main source of increased understanding. They are our connection with the physical world about which we theorize. The greatest scientific sin is to force experiments to conform to theories, attributing deviations in behaviour to experimental error; their most important role is to find weaknesses in theories and to bring to light new phenomena. Surprises (Polkinghorne 1986) are the stuff of active science. I say this because much of the complexity displayed by modern continuum mechanics is not the result of wilful self-indulgence on the part of theorists, but a necessary response to the behaviour of real materials.

We can explain deformation in the elastic region, but when we carry out experiments on yield and failure, we are often faced with apparent inconsistencies. In principle, if not always in practice, accurate estimates of Poisson's ratio and Young's modulus can be obtained unambiguously and consistently from a range of tests on coherent rock samples, using different geometries and loadings. Similar tests carried out beyond the elastic range are
often difficult to interpret in terms of traditional yield and failure criteria, chosen by analogy with those of metal physics. Paradoxes abound.

For example, compression tests carried out on cylindrical samples of saturated poorly permeable shales show ductility increasing with strain rate in a range of engineering interest, in contrast to standard arguments. A moderately convincing argument can be given to explain this in terms of localisation, dilatancy, pore pressure variations and effective stress. However, it has not been validated, and cannot yet be relied upon.

Furthermore, although the tests are chosen to lead to affine deformations (which are achieved in the elastic range) progressively more complex deformation structures arise at higher loads; these are strongly dependent on both isotropic and deviatoric components of mean stress (or of their histories since rate effects are noticeable) and have length scales that can span the full range between the sample dimensions and that of the separate mineralogical components. Thus attempts to avoid localisation merely ensure that it arises in an uncontrolled fashion! By contrast, tests that are deliberately non-affine, such as notch or indentation tests, are much more controllable. This is an example of the difficulties introduced by non-linearity in behaviour and explains why it is difficult to extract 'fundamental failure criteria' from laboratory experiments on rocks. Simple continuum mechanical notions may prove to be inappropriate.

The other major difficulty arises because of inhomogeneities: there seems to be no alternative to observing and cataloguing them, using the sort of statistical techniques and descriptions that have served in theories of mixing and inertial turbulence. This leads on to the issue of classification of rocks (Ghose) which has historically been dominated by the mineralogist, but which for our purposes should be more directly related to application and hence be based on mechanical properties. Clearly, elastic parameters (these can encompass anisotropy and static Biot models) will be insufficient, while we have argued that conventional failure criteria are not unique; there is a strong case to be made (Mróz 1985, Xuecheng 1987) for including a full range of dynamic test data. This has proved very successful in the case of viscoelastic polymer characterisation and has been anticipated by Biot.

5 FINITE ELEMENT MODELLING

To many applied mathematicians, FE (or BE) methods are no more than a way of getting (weak) numerical solutions to sets of integro-differential equations subject to specified boundary and initial conditions. These equations are governed by known physical models with unique mathematical form. The discretised elements - with their associated approximate functional forms for the field variables - lead, via finite matrix equations, to approximations to the assumed exact continuous solution (defined on a specified function space compatible with the original mathematical model of the physics). It is often formally required that an acceptable FE solution asymptote to the true solution as the FE mesh is successively refined. It must also predict bifurcations or instabilities correctly. The modelling associated with constitutive behaviour is thus quite distinct from the mathematical solution. This approach covers much of the FE work done in fluid mechanics and elasticity.
To practitioners in rock mechanics, on the other hand, the FE method is often inseparable from the physical and mathematical modelling. The formal process of averaging, or homogenising, that underlies traditional continuum mechanical models, is in effect rejected. It is the discretisation process itself that determines the scales in space and time over which the averaging takes place. In the applied mathematician's approach, the averaging scales are taken to be very small compared with the FE element scales; in the rock mechanics approach, the scales may be, or indeed must be, equal. Although this interpretation of FE modelling is rarely made explicit, it explains why, bemusingly to the outsider, FE modelling is treated by some of its users as a new branch of engineering science.

The simplest example of this is provided by numerical methods for jointed rocks (e.g. Goodman 1976): the boundaries of the elements are the frictional surfaces of the jointed rocks (Olsson, Bandis et al.); fracturing of the discrete rock elements can lead to new elements. For precisely specified initial conditions, i.e. a known mesh, precise responses to load histories can be calculated. An interesting possibility arises: that of ensemble averaging over an ensemble of initial jointed structures. This could lead to a higher level of constitutive behaviour (Rice 1985), in some ways capable of incorporating inhomogeneities at any given length scale into average properties on a larger length scale. My impression is that this has been done for closely packed granular media (soil-like) already.

It may be that the difficulties of predicting and explaining failure behaviour, insofar as they are connected with multiple scale effects, will only be dealt with in this way. Methods of classical mathematical analysis have been used to deal with multiple time expansions for mechanical systems, and it is possible that these could be extended to multiple spatial scales (some formal averaging methods already introduce multiple scales, but not for evolving systems). It is more likely that heuristic methods based on finite elements will be tried, which seek to embed detailed FE solutions at a small scale as averaged quasi-affine behaviour on a larger FE scale. In a sense this has already been done in the sense that models chosen for rock behaviour do vary over the 13 orders of magnitude in length scale that are needed to go from grain diameter to earth diameter.

6 CONCLUSIONS

Rock mechanics is a mature subject. Constitutive models based on general continuum mechanical principles have been well explored, even though they may not be as well known as they deserve to be.

Inhomogeneities are a crucial factor in applications, and their effect must be included (at all scales) in models and in the description of material properties.

Current yield and failure criteria are inadequate; experiments to determine them must be carefully interpreted.

Scale effects represent the biggest challenge in modelling.

Finite element modelling has a special significance in rock mechanics and forms
an intrinsic part of modelling.

Sedimentary rocks, showing all the complexity of soils and hard rocks, are ripe for intensive study. Fully 3-dimensional microstructural models for predicting (continuum) constitutive relations are needed.

The circular well-bore stability problem is now suitable as a benchmark application.

REFERENCES