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Towards a Methodology for Rock Mechanics Modelling

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Rock mechanics models fall into the class of "data-limited problems"; one seldom knows enough about a rock mass to model it unambiguously. Modellers are beginning to realize that data-limited problems require a very different modelling approach from that developed in, for example, electrical or aerospace engineering.

It follows that one cannot use models in rock mechanics in a conventional way, and that there is a need to adopt a distinctive and appropriate methodology for rock mechanics modelling. Some guidelines and heuristics, which may be considered as the first steps towards developing such a methodology, are presented. Three case studies are then used to illustrate the application, in practice, of these ideas.

INTRODUCTION

Perspectives on modelling in rock mechanics have changed dramatically during the past quarter of a century. There was a time when the focus of attention in rock mechanics was on laboratory and field measurement, and models (mathematical or computational) were generally thought to be either irrelevant or inadequate. Modellers spent a large part of their efforts trying to persuade sceptics that modelling was a useful engineering exercise.

The focus has shifted from measurement to computation; nowadays everybody builds models. It would be comforting to believe that this popularity has been hard won, that modellers in rock mechanics have converted the erstwhile sceptics. While this is partly true, there are probably three more important reasons for the upsurge in modelling:

—ease of access to versatile and powerful computer packages;
—a dramatic increase in the ability to include geological detail in the construction of a model;
—the manifest success of modelling in *other* branches of mechanics.

Each of these reasons carries with it both a positive and negative connotation and highlights why there is a need to develop an explicit methodology for modelling in rock mechanics. Ease of access to computer packages provides the rock engineer with a kit of computational tools. The problem is in knowing how to use the kit effectively and in having an understanding of both the strengths and weaknesses of the tools. Rock mechanics may be passing through the phase where the tool is perceived as a solution rather than as a means to a solution. We need to develop the intuition of the craftsman who knows the inherent limitations of the materials he works with, and is intrigued but not bedazzled by new and more versatile tools.

The lack of geologic detail was a major stumbling block to early acceptance of modelling in rock mechanics; models appeared to be such gross oversimplifications of the geology that few stopped to ask whether they might nevertheless be useful. The ability to include more detail is welcome, but only up to a point. As a carryover from the past we still seem to have an implicit credo that more detail implies a better model. It is an addictive credo: the modeller becomes hooked on bigger and "better" models and these in turn need more data, leading to more field and laboratory measurements. At best these efforts are a waste of time and resources; at worst they are counter-productive, concealing the wood for the trees. After all, we build models because the real world is too complex for our understanding; it does not help if we build models that are also too complex. The art of modelling lies in determining what aspects of the geology are essential for the model. The challenge is to turn that art into a methodology.

Finally, the success of modelling in other engineering fields has been a spur to modellers in rock mechanics, but we must be wary of emulation because the differences between rock mechanics and, for example, aerospace or even structural mechanics, may be more important than the similarities. Modelling techniques and approaches, as well as expectations of what modelling can achieve, that are appropriate to the one may not be appropriate to the other. The challenge to modellers in rock mechanics is to recognize these differences and to develop a distinctive modelling methodology that

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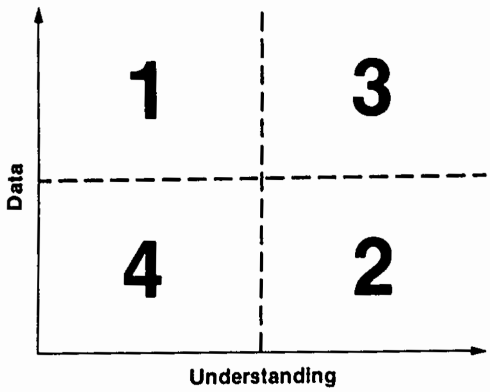

Fig. 1. Holling's [1] classification of modelling problems.

is both purposeful and effective. In this paper we will try to identify some of the distinctive features of rock mechanics and to suggest useful approaches and perspectives.

DATA LIMITED PROBLEMS

Figure 1 introduces a classification, due to Holling [1], that modellers in ecology have found to be useful. Holling introduces two axes, one that is a measure of the quality and/or quantity of available data, while the other measures the understanding of the problem to be solved. He then divides the quadrant between the axes into four regions. In region 1 there are good data but little understanding; this is where statistics is the appropriate modelling tool. In region 3 one has both the data and the understanding; this is where models can be built, validated and used with conviction. Regions 2 and 4 relate to problems that are data-limited in the sense that the relevant data are unavailable or cannot easily be obtained.

We would argue that problems in rock mechanics usually fall in the data-limited category; one seldom knows enough about a rock mass to model it unambiguously. Many of the problems in other branches of engineering mechanics, however, belong in region 3. Some of the old arguments against modelling in rock mechanics were, in essence, based on the recognition that the rationale for modelling in region 3 did not necessarily apply to the more amorphous data-limited problems of regions 2 and 4.

The modern trend is to hope to collect sufficient data to move rock mechanics into region 3, and so use models in a conventional way. Our hypothesis is that neither the old arguments nor modern goals are useful. The old argument is easily disposed of: if modelling in the conventional way is inappropriate to rock mechanics, that only creates the need for more thoughtful, unconventional modelling. The modern trend is more pernicious; it leads, as we pointed out in the Introduction, to more and more complex models and more expensive site evaluations without a concomitant improvement in either understanding or design. There are two salient arguments against it: the first is that it is futile ever to expect to have sufficient data to model rock masses in the conventional way, and the second is that as one includes more and more detail, one loses intellectual control of the model and so it becomes *less* instead of more effective.

What *is* useful is to ask how modelling tools developed for the well-posed problems in region 3 can be applied to rock mechanics problems in regions 2 and 4. We need to accept that rock mechanics does not fit the more conventional mould and develop a philosophy of modelling that fits rock mechanics instead of trying to fit rock mechanics to the prevailing philosophy.

MODELLING GUIDELINES

Starfield and Bleloch [2], in the context of ecological modelling, list some of the characteristic differences between problems in region 3 and those in regions 2 and 4 of Holling's diagram.

—The fundamental problem is the question of resolution; in region 3 one knows what level of detail is necessary to solve a problem and when simplifying assumptions are appropriate, whereas in regions 2 and 4 one is nervous of over-simplying the problem. As a result, modellers tend to concentrate on detail and build complex, unwieldy models.

—Problems in regions 2 and 4 are often ill-posed, leading to difficulties in interpreting the results and the nagging question of whether or not the correct problem has been modelled.

—Validation of models in region 3 is a well understood process; in regions 2 and 4 it may be impossible to validate a model, but instead of recognizing that impossibility and developing pragmatic approaches that address it, modellers pay lip-service to the process of validation.

—Models in region 3, once validated, can be used, almost routinely, for prediction. Models in regions 2 and 4 can never be used routinely; they have to be used far more cautiously and thoughtfully.

Anybody who has built a rock mechanics model will recognize that these points are as pertinent to rock mechanics as they are to ecology. Starfield and Bleloch also suggest ways of thinking about models that address the differences between the well-posed problems of region 3 and the data-limited problems of regions 2 and 4.

—A model is a simplification of reality rather than an imitation of reality. It is an intellectual tool that has to be designed or chosen for a specific task.

—The design of the model should be driven by the questions that the model is supposed to answer rather than the details of the system that is being modelled. This helps to simplify and control the model.

—It might even be appropriate to build a few very simple models rather than one complex model; the simple models would either relate to different aspects of the problem, or else address the same questions from different perspectives.

—Instead of trying to validate a model, one should aim to gain confidence in it and to modify it as one uses it. One's approach to the model should be like that of a detective rather than a mathematician.

—The purpose of modelling data-limited problems is to gain understanding and to explore potential trade-offs and alternatives, rather than to make absolute predictions.

—One progresses, slowly and painfully, from region 4 of Holling's diagram towards region 3 by a kind of "bootstrap" operation. First one builds a simple model and exercises it in a conjectural way. The results almost always suggest new ways of obtaining data, or new ways of interpreting available data. New data, in turn, suggest improvements to the model or ideas for new models. Implementing those improvements leads to new data requirements or insights, and so on. The whole process may be termed "adaptive modelling."

How do these ideas apply to rock mechanics? We believe they can be converted into a preliminary set of guidelines to modelling.

(1) Be sure, before you start, that you are quite clear about *why* you are building a model and *what* questions you are trying to answer.
(2) Use a model at the earliest possible stage in a project to generate both data and understanding. Do not delay while waiting for field data. You need a conceptual model [3] in place as soon as possible. A good conceptual model can lead to savings in time and money on field tests that are better designed.
(3) Look at the *mechanics* of the problem. Try to identify important mechanisms, modes of deformation and likely modes of failure.
(4) Think of experiments one would like to perform on the model and try to visualize, qualitatively, what the answers might be. In particular, if you have two or more conflicting ideas about what is going on in the field, propose simple numerical experiments to eliminate one or more of them (i.e. use the model to falsify hypotheses or rule out inconsistent data).
(5) Design or borrow the simplest model that will allow the important mechanisms to occur, and could serve as a laboratory for the experiments you have in mind.
(6) *Implement the model*, choose your simplest experiment, and run it. If the model ties in with your expectations, proceed to more complex experiments; if not, identify the weaknesses in your thinking (or the weaknesses in the model) and remedy them before continuing. Take a similar approach to the other experiments: explore possibilities and be critical!
(7) If your only available model has weaknesses that you cannot remedy (e.g. you only have a two-dimensional model for a situation that is clearly three-dimensional), make a series of simulations that will bracket the true case. In many instances the bounds will be sufficiently close so that useful insight will be obtained for the case that could not be simulated.
(8) Once you have learned all you can from the simple model or models, you may want to run more complex models to explore those neglected aspects of the geology that are most likely to affect the behaviour of the simple model. You may also want to do one or two design runs using more complex models, but these will often be more cosmetic than illuminating; it is often better, for design, to develop simple equations based on the mechanisms revealed by the models. These equations are then used for design rather than numerical models.

Note that steps 4–6 are crucial to the adaptive process. They may be implemented in a number of different ways. For example, you might want to test a series of simple alternative models rather than just one model. In running the model (or models) there are bound to be parameters whose values you will have to guess at. Be sure to run the model for ranges of plausible values. Where the model output is relatively insensitive to parameter values, you are safe; where it is sensitive, the form of sensitivity should suggest limits to the parameter values or field experiments that must be performed.

Visualizing and anticipating solutions *before* running a model is an important discipline. Hardy Cross [4] has this to say about structures: "The ability of a designer of continuous structures is measured chiefly by his ability to visualize the deformation of the structure under load. If he cannot form a rough picture of these deformations when he begins the analysis he will probably analyze the structure in some very awkward and difficult way; if he cannot picture these deformations after he has made the analysis, he doesn't know what he is talking about."

It should also be noted that implicit in the above guidelines is a rule that rock mechanics models should never be run only once; it is in the sensitivity of the results to changes in parameters and assumptions that the model is most informing. The guidelines are not presented as a panacea for rock mechanics modelling; they are presented as an approach that exemplifies an underlying philosophy. People commonly say "you only get out of a computer what you put into it" or "the results are only as good as the data". At one level these comments are of course true, but at another they are misleading. Modelling in a cautious and considered way leads to new knowledge or, at the least, fresh understanding. Even the writer of a modelling program learns new things when running his program; a system of interacting parts often behaves in ways that are surprising to those who specified the rules of interaction. Exploring and explaining those interactions is a form of learning; the model becomes a laboratory for those who built it.

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The philosophy that underlies the above guidelines is that models should be used in rock mechanics with the same caution and curiosity that exemplifies good laboratory work. In the next section we use three case studies to illustrate this approach.

THREE CASE STUDIES

1. The mystery of the staggered path

A large number of microseismic events were recorded during fluid injection 2000 m below surface in a geothermal energy project in Cornwall. The locations of events indicated a general trend in the spread (apparently of the injecting fluid) that was, surprisingly, not in the direction of the major joint sets, but rather in the direction of major principal stress [5].

At first sight this result is puzzling; injection pressures were too low to create new fractures, so the fluid must flow along existing joints. The rock/fluid system was modelled with program FRIP [6], although the problem may seem to be a poor candidate for numerical modelling in view of the data deficiency; there is little hard evidence on joint persistence, joint properties, and the initial fluid and mechanical state. However the model is conceptually simple: the intact rock is represented as a linear, elastic material. The joints deform linearly in shear, until the shear stress becomes equal to the shear strength, when Coulomb slip occurs. Since shear strength is equal to the friction coefficient multiplied by the effective normal stress, the effect of fluid pressure is to decrease shear strength and allow slip. Joints are programmed to dilate at a constant angle when sliding: this dilatation increases the permeability of the joint in proportion to the square of the aperture.

Treating the model as a laboratory, one can make a series of guesses about joint persistence, joint properties, etc. When these guesses are made, the model reveals a mechanism that explains how the general direction of propagation can differ from the joint directions. Figure 2 shows the results from a typical run: there is not one single fluid pathway, but a complex series of staggered paths that transport the fluid in a general direction that differs from the individual joint directions. This *en echelon* effect is even more pronounced when the model contains non-continuous joints.

The explanation for the behaviour is as follows. Increased fluid pressure causes slip on the one joint that intersects the injection point and is also a member of the set (call it the "primary set") that has the lower normal stress. At each end of this slipped section, the *in situ* stresses parallel to the joint are thereby increased on one side and decreased on the other. On the sides with the decreased stresses, some of the cross-joints are enabled to slip because their normal stresses are reduced. Fluid is then able to migrate to the end of the cross-joint (owing to slip-induced dilation), and the whole process continues, since fluid has now found its way to the next joint in the primary set.

Once we understand this basic mechanism, we can exercise the model with various parameters so as to discover what conditions are necessary to bring about

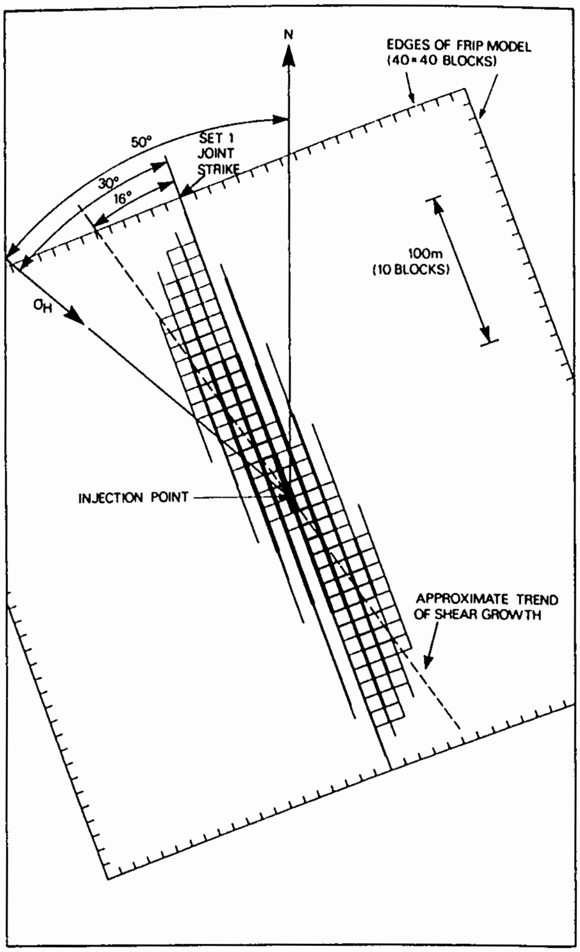

Fig. 2. Plan view of dilated joints, from a simulation by the FRIP program; after Pine [5]. Line thicknesses are proportional to apertures.

the observed behaviour. In a sense the computer supplies some of the missing field data, since the model only exhibits the observed pattern of fluid migration for a rather restricted set of data on joint geometry, *in situ* conditions and material properties.

2. A study of creeping sediment

In sediment layers deposited over thousands of years by large rivers, complicated structures such as faults and salt domes are often seen but not often understood, partly because of their apparent complexity. Since there are almost no data on properties, boundary conditions or initial conditions, geologists are uninhibited when inventing scenarios, but one prime suspect is that the non-uniform thickness of overburden causes an underlying layer (of salt, for example) to creep, and thereby induce distortions into the overburden.

A computer model is valuable in two ways. First it provides some specific examples that serve to keep our imaginations within realistic bounds. Second, by exercising some of the simpler cases covered, it is able to test the theories critically. Last [7] uses a large-strain finite difference model to represent a layer of viscous salt over which frictional material is progressively deposited. Figure 3 illustrates the initial state of the model, and Fig. 4 the state at 350,000 years. Even this simple model

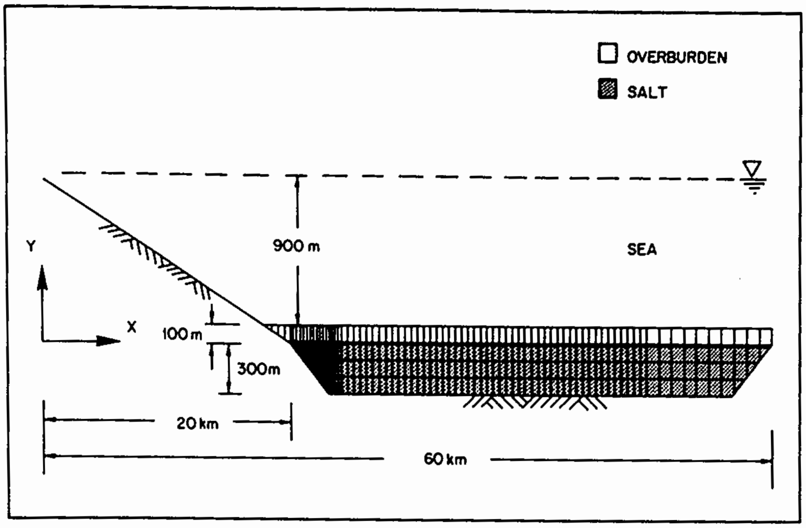

Fig. 3. Initial geometry of a numerical grid, showing 300 m of salt under 100 m of overburden. The program adds sloping layers of overburden from the left; after Last [7].

produces quite complicated patterns of distortion: a subsidence basin has formed to the left, and the beginnings of a salt dome is forming on the right. These types of structure are certainly observed in the field. The model has thus provided a picture of the distortions that can be induced when salt creeps outwards in response to an overburden load.

This is not the end of the story. As Last points out, the region between the basin and the "dome" seems to be moving as a rigid body, while the left and right sides are in the active and passive plastic states. Considering horizontal equilibrium, a simple equation can be written that relates many of the important parameters in the problem (see Fig. 5).

The numerical model has suggested a simple mechanism that was obscured by the apparent complexity of the field data. The mechanism is then described by an equation that can readily be applied to future field observations. The conceptual model and the equation act as filters that help us to interpret incoming evidence by reducing data overload.

3. The paradoxical rock slope

A planned rock slope was to be constructed in sedimentary rock. The bedding planes dip steeply, so the designer cleverly arranged for the slope to dip at the same angle as the bedding (see Fig. 6). In this way he hoped to avoid failure, since potential failure planes do not daylight in the slope. When the slope was constructed however, the face was found to break up continuously, with sheets of rock sliding from the slope. On closer inspection some horizontal fracture planes were noticed, but it was not immediately evident how these could be associated with the instability since rock does not normally move horizontally.

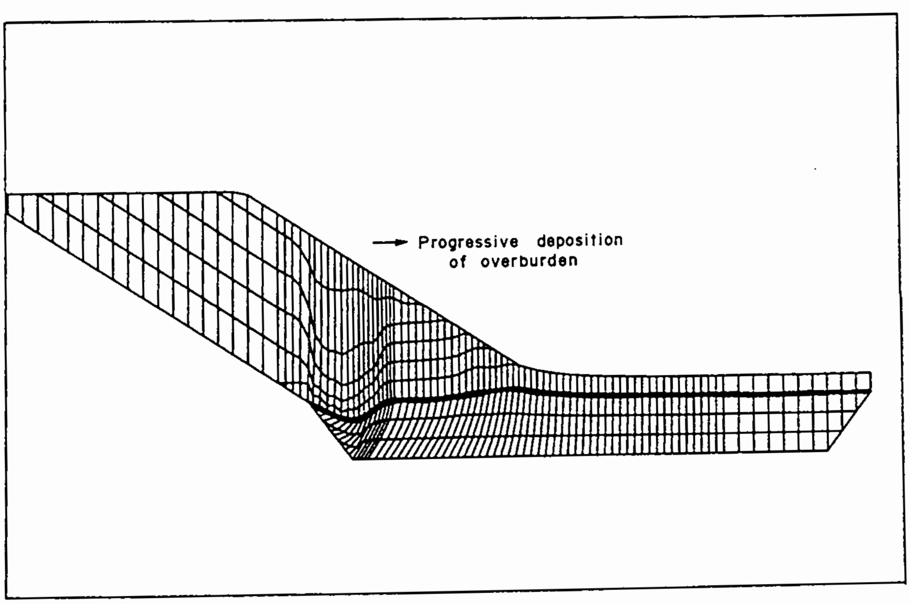

Fig. 4. Numerical grid after simulated time of 350,000 years: a basin has formed in the new overburden, and a salt dome is beginning to form at the toe of the slope; after Last [7].

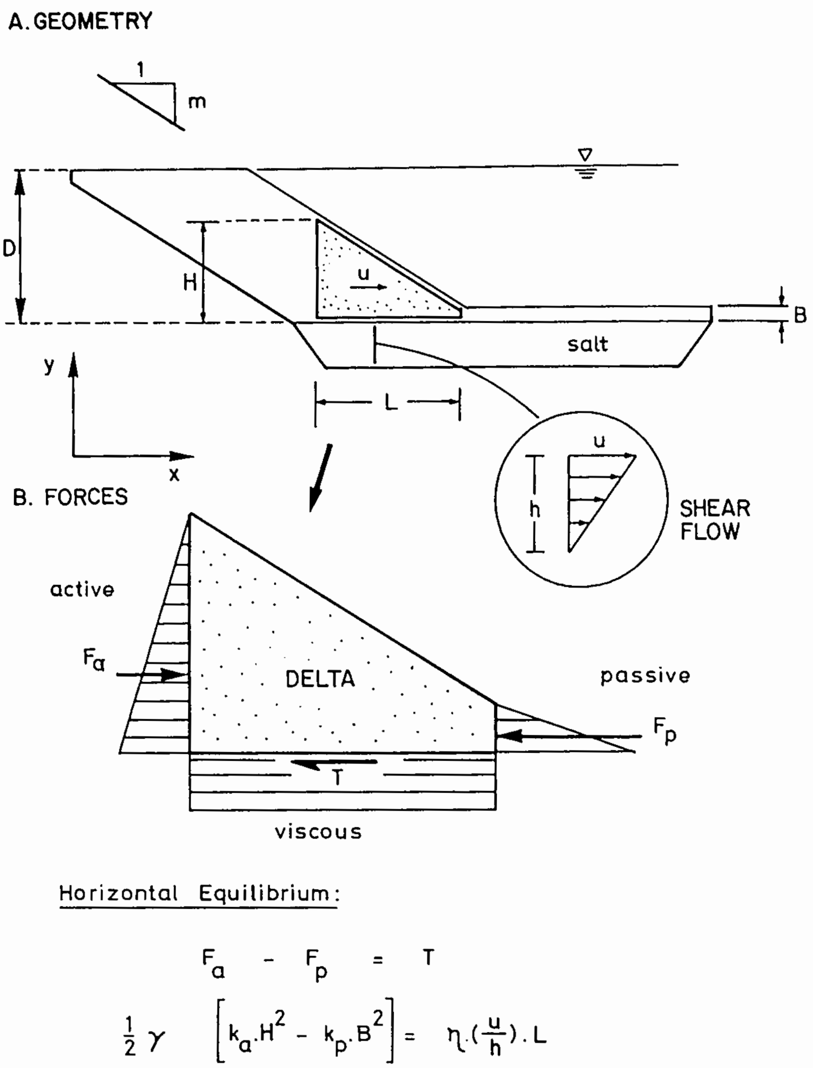

Fig. 5. A simple formula that expresses the simplified mechanics of the salt/sediment system; after Last [7].

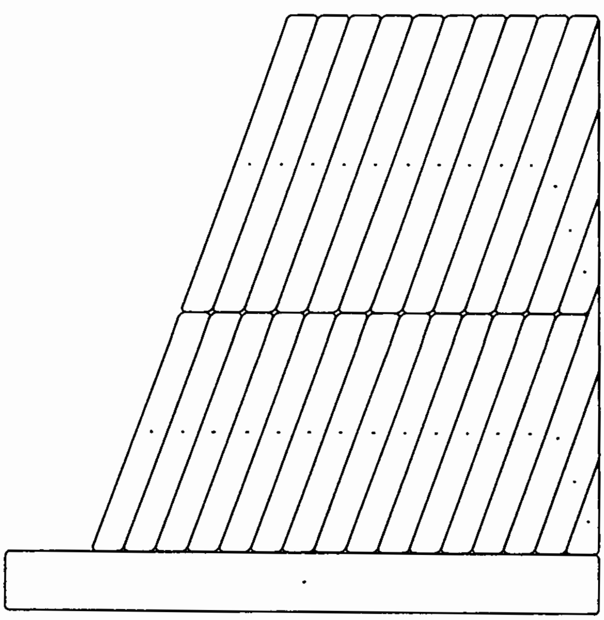

Fig. 6. Initial geometry of a rock slope, in which the slope face is parallel to the bedding planes. Note that two horizontal faults are also present.

A numerical simulation was made with the distinct element program UDEC [8], using various values of friction for the bedding planes and the fracture planes. It is found that the horizontal planes are indeed the culprits since they allow the rock blocks to rotate (see Fig. 7, which shows successive plots from the computer simulation). By making several runs with different properties, we define, empirically, the conditions under which the failure can occur, and can design remedial measures.

Perhaps more importantly, once the computer model has suggested (or confirmed) the mechanism of failure, we can try to develop a simple formula based on equilibrium of forces. For example, an equation can be derived for a single, thin slab that relates friction angle to slope angle, at limiting equilibrium.

The failure mechanism seems obvious after it has been identified, but in the field things are not often so clear, as one can be overwhelmed by extraneous factors.

The example illustrates the importance of selecting a model that is capable, in principle, of reproducing the suspected mechanisms. We might have tried using, as an alternative, a transversely isotropic material to represent

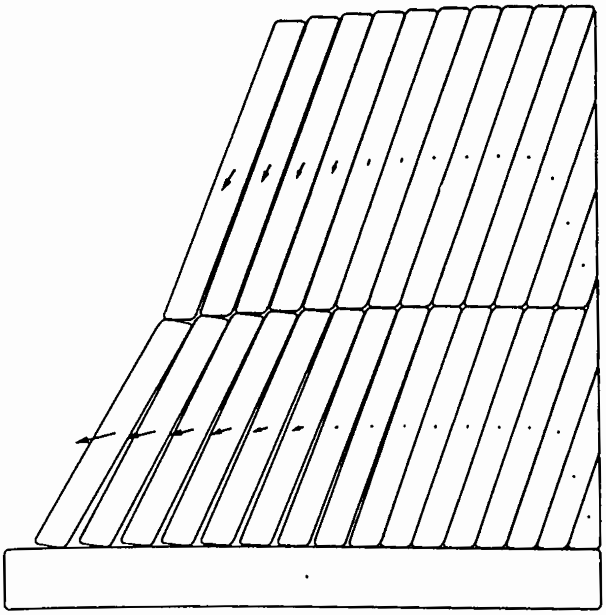

Fig. 7. Geometry of rock blocks after some time has elapsed. The calculation was made by program UDEC for a friction coefficient of 0.15. The horizontal faults allow block rotations to occur.

the effect of the bedding planes, but a slope constructed of this material would not be able to fail in a mode that involves rotation.

Notice how each of the case studies described above was motivated by puzzling field observations. Notice too that in each case there were very few mechanical or geometrical data; all three fall in the category of data-limited problems. We have deliberately presented the cases so as to resemble detective stories, precisely because the modeller should be playing the role of detective. The modelling exercises led, in all three cases, to plausible mechanisms that explained the puzzling observations, and from there to further investigations and understanding. In at least one case, once the mechanisms were highlighted by the model, simpler analytic solutions could be found to "explain" the modelling results even more succinctly. All three cases illustrate how the computer can be used adaptively as a "laboratory" and exemplify the "bootstrap" approach of Starfield and Bleloch [2].

It is often tempting, once the modelling process is complete and new understanding has been acquired, to point out that the mechanisms revealed are "obvious" and that the modelling was not really necessary. This is easy to say with hindsight; those who read detective novels also tend to say: "I knew who did it all along," but only *after* they have finished the book!

CONCLUDING REMARKS

Historically, modelling in rock mechanics has been driven by a perceived need to include more geological realism. That drive has led to large, clumsy and complex computer models that people build and run, not because they are an integral part of the design process, but because it is considered irresponsible not to bolster design with plots of stress contours and the like.

We hope that we have convinced at least some of our readers that detail can smother a model, and that simplification is a crucial part of rock mechanics modelling. There is a dialectic between geological detail and engineering understanding. We also hope that we have shown how a model is an aid to thought, rather than a substitute for thinking.

Rock mechanics is an experimental science if only because its subject, rock, cannot be described in a meaningful way without measurement. Rock engineering is the discipline that exploits the science for design purposes, and so it too must be based on measurement. Yet, paradoxically, measurement and experiments in rock masses are costly, difficult to perform or even impossible to carry out. It follows that modelling, as an ersatz form of experimentation, is an *essential* ingredient of both the science and the design discipline. We suggest that it is both necessary and constructive to think of rock mechanics modelling in this way; the first rule of a modelling methodology is one that says "plan the modelling exercise in the same way as you would plan a laboratory experiment."

Methodology is bound to become a vital area of debate in rock mechanics. In the first place there is an urgent need for it, if only because the ability to communicate, argue from and defend a model will be put to the test in large engineering projects such as the underground storage of high-level radioactive waste. In the second place, the burgeoning field of expert systems and knowledge engineering provides a "language" for talking about, teaching and implementing methodology.

This paper provides a pragmatic philosophy and some robust heuristics for modelling. It is conceived as one of the opening statements in the upcoming debate. Methodology is not a subject that lends itself to research proposals, higher degrees or even papers in engineering journals, but it is however a part of everything we do. Methodology will develop if everybody who builds a model pauses to think about why they are building it at all, why they built one model rather than another, whether they could have built a more useful model, and how their model has influenced their understanding or design. The purpose of this paper is to encourage people to do this, and to think, talk and write about it.

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